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OBSTETRIC
MEMOIRS AND CONTRIBUTIONS.

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THE

OBSTETRIC

MEMOIRS AND CONTRIBUTIONS

OF

JAMES Y. SIMPSON, M.D., F.R.S.E.,

PROFESSOR OF MIDWIFERY IN THE UNIVERSITY OF EDINBURGH, ETC. ETC.

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VOLUME II.

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II.

DEDICATION.

TO DR. CHARLES LOCOCK,

FIRST PHYSICIAN-ACCOUCHEUR TO HER MAJESTY THE QUEEN; CONSULTING PHYSICIAN TO THE LONDON GENERAL LYING-IN HOSPITAL, ETC., ETC., ETC.

My dear Dr. Locock:

It is a great gratification to me to have the pleasure of inscribing the present volume to you. For thus I have an opportunity of testifying publicly—what I have often testified privately—my sincere respect for those high and sound practical acquirements that have so deservedly gained for you the first place among British accoucheurs; and, at the same time, my admiration of the honest independence of thought and opinion, and the sterling uprightness of conduct and action, by which you have added dignity to the proud position won for you by your talents.

Earnestly do I wish that the volume were more worthy of your acceptance. But as you well know, the life of a busy accoucheur is not a life fitted for literary work. Besides, even were time for it at my disposal, I fear that I am quite deficient in some of the principal qualifications generally laid down as requisite for success in medical authorship, having no heart or habit for the daily written annotation and collection of long individual cases and observations; no sufficient industry and endurance for the pursuit of any very tedious and protracted investigation; and no great love of lifting my pen, but the very reverse.

Pray accept, however, of this dedication, not as any token of the worth of the volume, but as a token merely of my cordial esteem, and as some memorial also of various kindnesses in early professional life unmerited, but not unremembered, by

Yours most faithfully,

J. Y. SIMPSON.

Edinburgh, 25th March, 1856.

PREFATORY EDITORIAL NOTE.

THE present volume contains a variety of essays and contributions by Dr. Simpson, on

The Pathology of the Puerperal State;
The Physiology and Pathology of the Products of Conception; and
The Pathology of Infancy and Childhood.

To these we have added the various papers and communications which he has at different times published on the use of

Anæsthetics in Midwifery, Surgery, &c.[1]

Various circumstances have delayed the appearance of the present volume, but particularly a desire on Dr. Simpson's own part to add to the value of some portions of the work by his own more direct superintendence. It will be found in consequence, that some communications, parts only of which were formerly published, have now been completed by him—as the essay (p. 441), "On the external use of oil in the prevention and treatment of Scrofula." Other contributions have been in a great measure remodelled and rewritten by Dr. Simpson for the present volume, as, for example, the observations (p. 412) "On Placental Phthisis as an intra-uterine cause of death among Premature Children;" the general laws (p. 433) regulating the "Simultaneous coexistence and progress of Cow-pox and Small-pox;" and the remarks (p. 289, &c.) on the Morphology of the Male Uterus, and on the Unity of Organic Type between the two sexes. Several contributions in this volume also appear for the first time—as, the Memoir (p. 46, &c.) entitled "Pathological Researches on Puerperal Arterial Obstruction and Inflammation;" and the essays "On the Rudimentary Reproduction of Extremities after their Spontaneous Amputation" (p. 344); "On the Practical Application of Chloroform as a topical Anæsthetic to mucous and cutaneous surfaces" (p. 680); and "On Carbonic Acid Gas as a local Anæsthetic in Uterine Diseases, &c." (p. 687). Several brief new contributions will also be found added by him throughout the volume in the way of foot-notes.

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[1] Most of these papers on Anæsthesia have already appeared in this country in a collective form. They have, however, now been carefully revised, additions and alterations have been made, and, as bearing on a most important and still often misinterpreted, and therefore much-abused point of practice, they are again republished, uniformly with the Edinburgh edition.—H. R. S.

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PART IV.

PATHOLOGY OF THE PUERPERAL STATE.

ON THE ANALOGY BETWEEN PUERPERAL AND SURGICAL FEVER.

(From Edinburgh Monthly Journal of Medical Science, November, 1850, p. 414.)

NEARLY 3000 mothers die in childbed every year in England and Wales.[1] Among these 3000 deaths, a comparatively small proportion only are the direct result of convulsions, hemorrhage, rupture of the uterus, and the other more immediate or primary complications and accidents connected with parturition. The great majority of these maternal deaths is produced by puerperal fever. Dr. Fergusson[2] believes that as many as seven-eighths of the total mortality in childbirth are owing to puerperal fever and its modifications. This estimate of its effects is probably too high; but the observations of Farr,[3] Kiwisch,[4] and others, have amply shown that puerperal fever, and the visceral inflammations and deposits which are included under that name, are undoubtedly by far the most common causes of death in childbirth. Among 2890 women delivered in the old Edinburgh Lying-in Hospital, from 1823 to 1837, 47 maternal deaths occurred. Of these 47 deaths, 36 were the consequence of puerperal fever; and the remaining 11 were produced by other primary obstetric complications and causes.

Among the individuals who are obliged to become the subjects of serious surgical operations, a considerable number die, however dexterously and well the operations may be performed. When the operation is severe, the proportion that thus die in consequence of it is much larger than is generally believed. For instance, among every 100 cases of amputation of the limbs—including amputation of the thigh, leg, arm, and forearm—about 30 terminate in death, and 70 in recovery. Dr. Fenwick, some time ago, published the results of 4937 cases of these amputations, collated from the practice of some of our best civil and military surgeons.[1] Among these 4937 patients, 1565 died after the operations, or nearly 1 in every 3. The numerous deaths which thus follow operations in surgical cases are, in a small proportion of instances only, the more immediate and direct results of primary complications or accidents connected with the operation itself, such as hemorrhage, syncope, collapse, &c. In a large proportion of instances the death of the surgical patient is the result, on the contrary, of a combined febrile and inflammatory morbid state, which I believe to be generically, if not specifically, the same as puerperal fever in the childbed mother; and in accordance with the common principles of nosological nomenclature, this disease may as correctly be denominated "surgical fever," as the other is denominated "puerperal fever."[2] A few years ago, Dr. Chevers published the apparent cause of death in 153 patients, whose bodies had been examined at Guy's Hospital,[3] London, and who had died within that institution in consequence of surgical operations or injuries. Among these 153 surgical cases, in 1 the precise cause of death could not be discovered, and 18 of them had sunk under hemorrhage, tetanus, sloughing, suppuration, gangrene, erysipelas, diarrhœa, and the total deficiency of reparative action in the wound. Excepting, then, these 19 cases, Dr. Chevers found that in all of the remaining 134 cases, the post-mortem examination betrayed, as the more immediate cause of death, the existence of acute inflammation of one or more internal organs or structures;[1] and, no doubt, in all of them the usual symptoms of surgical fever were more or less perfectly marked during the last days of life. In most of these 134 cases, the recent internal inflammatory lesions discovered on the bodies of the patients were, as we shall see in the sequel, not confined to a single organ or structure, but several were frequently found affected at the same time in the same patient.

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[1] According to the Reports of the Registrar-General, the mortality from childbirth in England and Wales was 2811 in 1838, 2915 in 1839, 2989 in 1840, 3007 in 1841, and 2687 in 1842. During these five years, 14,409 English women died in childbirth; or about one in every third hour. Perhaps the returns under this head show even less than the reality, deaths occurring during the puerperal month being too often referred to other causes than childbirth.

[2] Essays on the Most Important Diseases of Women, p. 1.

[3] Fifth Report of Registrar-General, p. 380.

[4] Brit. and For. Review, vol. xiii. p. 111.

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[1] See his excellent papers in the Monthly Journal for 1847, p. 238, &c.; and the corresponding evidence of the fatality of amputations and other operations, adduced by Malgaigne, Phillips, Lawrie, Inman, &c.

[2] Puerperal fever has been often denominated in accordance with the special notions which the writer happened to entertain of its pathological nature. Hence we have it spoken of in some works as puerperal peritonitis, or metritis, or phlebitis, peritoneal fever, &c. &c. Any name thus drawn from pathology must ever change with the changes and advances of pathology itself; while a nosological name—such as puerperal fever—never requires to be varied, and is always fixed and intelligible. For example, all physicians know and recognize dysentery under that, its nosological name; but if we had had it described and denominated in different ages and works according to the ideas entertained of its nature, an inextricable amount of confusion would long ere this have resulted; and so with regard to most other diseases. Some surgical pathologists have described the consecutive fever and internal inflammations under which surgical patients sink, under the names of pyæmia, phlebitis, &c., each in accordance with his own pathological notions. The term "surgical" or "chirurgical fever" would enable us to avoid all the difficulties and perplexities connected with such a pathological nomenclature; it implies no pathological theory; and it is sufficiently precise and distinctive as a simple nosological designation.

[3] Guy's Hospital Reports for 1843, p. 89.

Medical literature does not yet possess a sufficient series of data to enable us to institute a full comparison between all the elements of puerperal and of surgical fever.[2] But the consideration of a few points may prove enough to indicate at least a strong analogy, if not an identity, between these two forms of disease. With this view, I shall in the following notes attempt very briefly to show in what respects puerperal and surgical fevers are assimilated to each other: 1. In the anatomical conditions and constitutional peculiarities of those who are the subjects of them; 2. In the pathological nature of the attendant fever; 3. In the morbid lesions respectively left by either disease; and 4. In the symptoms which accompany each affection.

1. *Analogy in the Anatomical, &c., Conditions of the Subjects of Puerperal and Surgical Fever.*—The anatomical conditions of the puerperal patient after delivery, and of the surgical patient after an

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[1] It is proper to state, that, as Dr. Chevers remarks, in about 13 out of the above 134 cases the nature of the injuries was such that the patients had evidently from that cause alone no fair chance of recovery; but in the whole of the other 121 cases it appeared that there was nothing to render the patient's restoration impossible, had not severe inflammation or some other unfavorable change supervened.

[2] Perhaps I may be pardoned for remarking, in vindication of obstetrical literature, that it possesses many separate essays and volumes on puerperal fever; and every text-book on midwifery contains a full chapter on the subject. But the reverse holds too true with regard to the literature of surgery. "Works," says Dr. Fenwick, "upon operative surgery are daily written. which detail with the greatest accuracy the formation of flaps, and lay down to a hair's-breadth the extent of incisions, without mentioning the secondary affections liable to occur after the operation. Huge dictionaries issue from the press, discussing the rival merits of ancient authorities to some trifling improvement in the method of amputating, but forgetting to lay down rules for the prevention or detection of the many dangerous diseases which so often follow its performance. How many hospital surgeons seem to imagine that the necessity for their personal attention to a case of amputation terminates at the door of the operating theatre, and leave the after treatment to be solely directed by a house-surgeon or a dresser. How many content themselves with a hurried inspection of the pulse and tongue of the patient, after an amputation, instead of exploring the chest, or watching for the first indication of phlebitis; and how many young practitioners do we not find who speak of an amputation as though the interest connected with it ended with the operation, and as though success were certain if the patient surmounted the shock, or be unaffected with hemorrhage." Monthly Journal, vol. viii. p. 242.

operation, are in many respects the same. In the surgical patient, we have a wound or solution of continuity on the external part of the body, made by the knife of the surgeon; this wound has, opening upon its free surface, the mouths of numerous arteries and veins; and it comes to be repaired either by the direct adhesion of its opposed surfaces, or more slowly by exudation of organizable lymph upon its surface, and the ultimate formation or development of a new skin or new enveloping or connecting tissue. In the puerperal patient we have a wound or solution of continuity on the whole internal surface of the womb made by the separation of the placenta, and the exfoliation of the decidua or superficial layer of the mucous membrane of the fundus and body of the uterus;[1] this wound has, opening upon its free surface at the former site of the placenta, the mouths of numerous arteries and veins; and it comes to be repaired under the usual accompanying exudation of organizable lymph upon its surface, and by the ultimate formation or development of a new layer or coating of mucous membrane. The obstetrical patient has the wound complicated with constitutional states of the same kind as those observed in the subjects of surgical operations. Both, at the first, are liable to present the symptoms of shock or collapse, particularly if the labor or the operation has been unusually severe; both have generally a subsequent limited degree of febrile reaction—the traumatic fever of the surgeon—the so-called milk fever of the obstetrician; and both the external surgical wound and the internal obstetrical wound are liable to deviate from the standard mode of reparation; for their secretions may alter morbidly; or they may become the seat of an excess of inflammation or of ulceration; or of phlebitic suppuration and its consequences. In the internal obstetrical, as in the external surgical wound, immediately after their infliction, air occasionally enters by the mouths of the veins opening upon their free surface; from both forms of wound, dangerous hemorrhage, both primary and secondary, is liable to occur; both are occasionally, though very rarely, followed by delirium, tetanus, and other nervous complications; and, in like manner, but much more frequently, they are apt to be followed by that form of combined febrile and inflammatory action which we term surgical fever in the surgical patient, and puerperal fever in the puerperal patient. In short, the two species of wounds are subject to the same local pathological deviations, and liable to be attended with the same pathological constitutional effects and complications.

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[1] Dr. Heschl, Demonstrator of Pathological Anatomy to the large Hospital of Vienna, has there enjoyed greater opportunities of studying the changes in the uterus after delivery, than perhaps any other living observer, and has lately published the results of his observations on the subject.

In speaking of the condition of the uterus immediately after delivery, he remarks, "The veins of the placental spot are filled with dark-red, or grayish-red, clots sticking to their parietes, and gape into the cavity of the uterus with their orifices plugged with these clots. This placental spot, which always occupies a third part of the inner surface of the contracted uterus, still retains a projecting, uneven, and considerably lacerated surface. The *rest* of the inner surface of the corpus uteri (from which the decidua had separated) is composed of the *bare* muscular substance, from which hang here and there shreds, the remains of the decidua vera."

Dr. Heschl repudiates entirely the idea of Robin and Kilian as to the formation of a new mucous membrane between the decidua and muscular structure of the human uterus during the period of pregnancy.

In studying the mode of reconstruction of the mucous membrane after delivery, Dr. Heschl finds that in two days after birth, the entire inner surface of the uterus appears covered with a soft pap-like, flaky substance, which gradually spreads over that surface in layers like very fine meshed-net. Vessels are not evident in it, however, till the third week; and the appearance of the mucous glands is still later.

For more lengthened details, see his Researches on the Conduct of the Human Uterus after Delivery, 1853.—(Ed.)

2. *Analogy in the Pathological Nature of Puerperal and Surgical Fever.*—Two opinions were formerly held with respect to the pathological nature of puerperal fever. One class of pathologists (as Puzos, Levret, Hamilton, White, &c.) regarded it as an idiopathic or putrid fever, *sui generis;* another class (Hey, Armstrong, Mackintosh, Campbell, &c.) still more earnestly maintained that the disease was essentially a local inflammation—that the fever was merely a consequence of, and attendant upon, this local inflammatory irritation—and that the malady was to be treated and cured by venesection and other active antiphlogistics. The first of these doctrines became generally abandoned with the advances of pathological anatomy, because local inflammatory lesions in the uterus, peritoneum, chest, &c., were, after death, found far too frequently, and of far too marked and intense a character to be explained upon the doctrine of a previously existing fever alone. But again, on the other hand, the idea that the disease was essentially a local inflammation, and that the fever was merely an effect symptomatic or sympathetic of that local inflammation, has been in turn gradually disproved also, as the pathological anatomy of the disease has been of late years more completely investigated. For it has been found that—1*st,* There is no general uniformity of relation and sequence between the degree and intensity of the supposed cause (the local inflammatory lesions), and the degree and intensity of their supposed effect (the attendant fever); 2*d,* Sometimes the supposed cause (in the form of simple peritonitis or metritis, &c.), may exist, without these inflammations exciting the usual phenomena of their supposed effect, namely, the symptoms of puerperal fever; and, 3*d,* We see occasionally cases of true and fatal puerperal fever, without discover-

ing on the dead body any traces or evidence of the local inflammation which had been considered the origin of the disease. In other words, under this last class of cases we have the existence of the supposed effect without the existence of the supposed cause. And this observation holds good with regard not only to the individual local inflammations, which have been illogically dogmatized into the alleged invariable origin of puerperal fever; but it holds good with regard to the whole class of local inflammatory causes. For instance, puerperal fever has been regarded as a form of fever produced by inflammation of the omentum, according to Hulme and Leake; by inflammation of the peritoneum, according to Gordon, Mackintosh, and many others; by inflammation of the uterus, according to Astruc and Denman; by inflammation of the uterine veins or lymphatics, according to Dance and Duplay, &c., &c. Now, in different epidemics, and sometimes in the course of the same epidemic, we see well-marked and fatal cases of puerperal fever, without being able to trace on the dead body any evidence of the one local inflammation theoretically fixed upon as the origin and essence of the whole disease. We thus see fatal cases without any proof whatever of omentitis, or any proof of peritonitis, or of metritis, or of uterine phlebitis or lymphatitis. In other words, in answer to those who maintain the identity of puerperal fever and peritonitis, for example, we can point to cases of genuine and fatal puerperal fever, in which the peritoneum was found perfectly healthy after death;[1] and we can repeat this proof with regard to any other individual local inflammation that may be improperly adduced and considered as the cause and origin of the whole disease.

Some authors, while they maintain the disease to be a fever entirely symptomatic of some local inflammation, at the same time hold that this local inflammation may be seated in different parts in different cases, and different epidemics; and that the disease originates, in one case, in metritis; in another, in ovaritis; in a third, in peritonitis; and so on. Without remarking on the illogical nature of imagining that the same disease may have such varied origins, we may once more pointedly observe, that (as sometimes happens in continued fever) occasionally, though very rarely, no inflammatory lesions whatever can be traced upon the bodies of patients who have died of puerperal fever. Dr. Locock has observed several cases of this kind.[1] And, in the practice of the late Dr. Beilby, I saw one very marked and rapidly fatal case of puerperal fever, in which my colleague, Professor Bennett, was unable to detect anywhere in the abdomen, or in the uterus, its appendages or vessels, any traces of inflammatory action or effusion. The great rarity of such instances is no sufficient argument against their important bearing upon the question of the nature of puerperal fever.

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[1] For example, in 222 autopsies which he made of puerperal fever patients, Tonnellé found peritonitis in 193; and there was apparently no evidence of it in 39 of the dissections. "Inflammation of the peritoneum," he observes, "is one of the most frequent alterations which we met with as a result of puerperal fever; but," he adds, "it would be a grave error to think that it existed always. Sometimes, in fact, this membrane preserved its natural aspect, and the most minute researches could not detect in it any appreciable change."—Archives Générales de Médecine, vol. xxii. p. 350.

The evidence which I have thus briefly sketched has induced, of late, most of our best pathologists to reject the idea, either that puerperal fever is an idiopathic fever *sui generis*, or a disease originating in and identical with peritonitis, or with any other local inflammation. And, on the other hand, many investigations and experiments made during the last ten or twenty years upon the effects of an acquired, or artifically excited, state of vitiation or poisoning of the blood, have inclined them more and more to adopt the doctrine, that the real source and cause of puerperal fever is to be found in a toxæmia or morbid state of the circulating fluid. The direct injection of pus, and other morbid secretions and matters, into the blood of the lower animals by Gaspard, Cruveilhier, Castelnau, and others, have produced a series of symptoms during life, and a series of lesions on the dead body, showing a very strong analogy to those of puerperal fever. The commixture of pus with the blood in the human subject, in cases of phlebitis, &c., in which pure pus enters directly into the circulation, gives rise to a similarity both of febrile functional lesions as seen during life, and of inflammatory organic lesions as seen after death. And in the puerperal female, there exist such conditions as facilitate the infection of the general circulation, by pus and other morbid matters contained in the uterine cavity. For they may obtain easy access to the general circulation—1. Through the orifices of the utero-placental veins, that open upon the internal surface of the uterus; which are, perhaps, not always completely closed; and which have their mouths constantly in contact with the contents and secretions of the uterine cavity; 2. Through the inoculation of morbid and contagious matters upon the abraded surface of the vagina; and, 3. By any accidental inflammation commencing in the lining membrane of the maternal passages (which are distended and contused during delivery), readily passing by the law of continuity alone, through the venous orifices opening on the interior of the uterus, and thence along the lining membrane of these vessels. Under the now generally adopted view, that puerperal fever originates in a vitiated condition of the blood, we can solve more easily the problem with respect to the relation of the two elements, constituting puerperal fever—namely, first, the febrile action, and, secondly, the internal inflammations, which are present during it. For under this pathological view we see that the fever is not itself the cause of the attendant inflammations, nor these inflammations themselves the cause of the attendant fever; but that both of them—that is, both the fever and the inflammations—are the simultaneous sequences or effects of one common cause—namely, the original vitiated or diseased condition of the general circulating fluid. And farther, the same doctrine enables us to perceive, how in one set of cases, or one epidemic of puerperal fever, the febrile effect or element may be more marked than the inflammatory; and how, in others, and these generally the most amenable to treatment, the inflammatory element or effect may be more marked and more prominent that the febrile.

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[1] See his valuable article on Puerperal Fever, in the Library of Medicine, vol. i. In these several cases, "the most careful search," says Dr. Locock, "was made for morbid alterations of structure in the veins, the absorbents, the muscular structure, and the lining membrane of the uterus and of the adjacent parts, and nothing could be found to explain the cause of death."—*Loc. cit.* p. 351.

These views regarding the pathology of puerperal fever, are borne out and corroborated by what we see in surgical fever, and its inflammatory results among the internal viscera of the body; for no pathologist has ventured to maintain that the pleurisy or peritonitis, for instance, which we often see upon the dead bodies of patients who have died of surgical fever, is the effect or consequence of that fever; or that the fever itself is a consequence or effect of these local inflammations. All at the present day are agreed that the phenomena of the occurrence of these local internal inflammations is not explicable, and is not in any way explained, by the old doctrine of sympathy or metastasis; and few now dissent from the idea, that we are to look for their origin, and for the origin of the attendant fatal fever, in some diseased or vitiated condition of the blood. What this vitiated condition of the blood may specifically and actually consist of, in puerperal and in surgical fevers—whether it consists in the presence of some one or more of the elements of purulent matter alone; or whether other animal secretions than pus may be its common or occasional cause; and whether the attendant type of fever and its effects may not be fixed and regulated by specific differences in the diseased material present in the circulation, are all questions which a more advanced era of pathological research, and a more subtle chemistry and histology than has yet been brought to bear upon them, will perhaps yet ultimately determine. The character and phenomena of puerperal and surgical fever, and their inflammatory results, agree with those of small-pox, measles, scarlatina, rheumatism, and the other so-called "disseminated inflammations" of M. Chomel,[1] in this respect, that whilst originating, as he believes, in different specific "infections of the liquids" or blood, they all agree with each other in several respects; as that they cannot be excited artificially by the common causes of inflammation, but are developed by specific causes; they are thus secondary inflammations resulting from a primary morbid diathesis or alteration of the animal fluids; antiphlogistic measures possess usually comparatively little influence upon the duration of these disseminated inflammations, and often but a doubtful influence upon their intensity; and lastly, whilst the unity of each affection is preserved, each is represented by a multiplicity of local inflammatory lesions, developed simultaneously or successively, and frequently in organs and parts distant from each other. Of this last general fact, as far as it holds good with regard to puerperal and surgical fever, the next paragraphs will afford us due evidence.

8. *Analogy in the Internal Pathological Lesions left by Puerperal and Surgical Fever.*—Both diseases, as we have already so repeatedly stated, are liable to leave upon the dead subject, evidence of recent acute inflammation in one or in several of the internal surfaces or organs of the body. These internal inflammations are generally characterized by an unusual tendency to the effusion of loose coagulable lymph and pus. We possess as yet few statistical details as to the internal organs which are most frequently found affected with inflammatory lesions in surgical patients. But Dr. Chevers, in the valuable communication to which I have already referred, has given us some interesting details relative to this point, in the results of the post-mortem examination of the 153 fatal surgical cases that had occurred in Guy's Hospital; and which form the basis of his essay. I have already stated that, with the exception of 19 cases, recent internal inflammatory lesions were found in all of these cases; or, in other words, in 134 out of the 153. The following table shows the special recent internal inflammations that were discovered in the post-mortem examination of these 134 surgical patients:

Inflammatory Lesions in 184 *Fatal Cases of Surgical Fever.*
(From Chevers.)

Peritonitis, was observed in	52	cases.
Enteritis (excluding cases of Hernia), ...	9	"
Pneumonia and its results, ...	47	"
Pleuritis, ...	35	"
Bronchitis, Laryngitis, and Diphtheritis, ...	4	"
Pericarditis, ...	14	"
Arteritis and Aortitis, ...	4	"

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[1] Leçons de Clinique Médicale, 1834, pp. 529 to 539.

Phlebitis,	3	cases.
Meningitis,	27	"
Cerebritis,	9	"
Cystitis,	8	"
Pus in Muscles or Joints,	3	"
Inflammation of Tunica Vaginalis,	1	"

Let us contrast with the above, the records of the pathological anatomy of puerperal fever. These records are defective in this point, that most of the observers have apparently confined their examinations and descriptions almost entirely to the morbid changes seen in the uterus, uterine appendages, peritoneum, and other parts, which they believed to be the principal seats of the disease; and have not given the morbid appearances found within the chest, encephalon, &c. Dugès and Tonnellé have, however, left the results of their researches into the state of the thoracic organs, and have examined the condition of the encephalon in a few instances. Dugès reports the result of the examination of the bodies of not less than 341 women who had died of puerperal fever. Among these 341 cases he found the following lesions, in the following proportions:

Inflammatory Lesions in 341 Fatal Cases of Puerperal Fever.
(From Dugès.)

Peritonitis was observed in	266	cases.
Metritis, or pus in Veins, &c.,	200	"
Ovaritis,	48	"
Gastritis and Enteritis,	4	"
Pleuritis,	40	"
Pericarditis,	6	"
Arachnitis,	1	"
Pus in Muscles or Joints,	8	"

The observations published by Tonnellé are more minute, as respects, at least, the uterine and abdominal organs. He examined the bodies of 222 puerperal patients, with the following results:

Inflammatory Lesions in 222 Fatal Cases of Puerperal Fever.
(From Tonnellé.)

Peritonitis, was observed in	193	cases.
Metritis and Ovaritis,	197	"
Pus in the Uterine Veins or Lymphatics,	112	"
Gastritis and Enteritis,	6	"
Pleuritis,	43	"
Pneumonia,	21	"
Pericarditis and Hydro-pericarditis,	1	"
Pus in Liver, Pancreas, Muscles, &c.,	19	"
Pus in Joints,	10	"

The preceding three tables, from Chevers, Dugès, and Tonnellé, afford a variety of important evidence relative to the internal morid actions which precede death in puerperal and surgical fever.[1] Among other matters, they especially illustrate the following points: 1. Both diseases generally leave upon the dead body ample evidence of the occurrence before death of acute and often extensive internal nflammatory action. 2. The internal inflammatory lesions are eldom limited in the same case to one organ or texture only, but two or more different viscera or surfaces are usually observed to have been either the simultaneous or successive seats of inflammaory action, and the different parts thus attacked are sometimes very distinct and distant from each other. 3. The internal viscera or textures, which are the first and principal seats of inflammation, are often far removed from the original wound or lesion, paricularly in those cases in which the wound or lesion is in the head or extremities. From the days of Valsalva and Morgagni downwards, injuries of the head have been observed to be often followed by inflammation and the effusion of pus in the liver, lungs, pleura, &c. Dupuytren, Sir Charles Bell, Guthrie, and others, have long ago remarked that after amputation of the extremities, inflammation of the lungs or pleura (parts sufficiently distant from the seat of the wound) was a very common and a very fatal sequel.[2]

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[1] Let me here take an opportunity of remarking, that before we can expect tabular results from post-mortem examinations, showing, more completely than the above, the analogy between puerperal and surgical fever, the entire pathological anatomy of each disease will probably require to be studied and made out at the same periods, within the same hospital, and by the same pathologist. For judging at least from puerperal fever, the disease seems liable to vary somewhat at different times, and in different localities, in the forms, seats, and intensity of the local inflammatory lesions which it produces. And when we have the lesions noted down by different anatomists, the reports too often differ though the lesions are the same; for the attention of one observer is apt to be called particularly to one set of appearances, and that of a second observer to another. For example, we feel quite sure in stating that in Dr. Chevers' table the number of cases of phlebitis (three) is greatly understated; while in the tables of Dugès and Tonnellé a similar remark is applicable to the encephalic and perhaps other secondary lesions. A comparative inquiry, by the same anatomist, into the lesions respectively left by puerperal and surgical fever in some of the large hospitals of Paris, Vienna, or Prague, would in this respect be extremely interesting and important.

[2] From some notes which I made a few years ago, relative to the frequency of pulmonary inflammations after operations, I find that, out of 77 dissections of patients dying after amputation, and recorded by Drs. Lawrie, Reid, Peacock, Erichsen, Orr, and May, in 30 instances inflammation of the pulmonary organs was observed; in 38 out of the 77, phlebitis and purulent deposits in different parts were observed. In a valuable paper by Professor Erichsen, in the Medico-Chirurgical Transactions, vol. xxvi., that gentleman has shown that out of 62 cases of death after surgical operations and injuries (exclusive of burns), taken indiscriminately from the records of University College Hospital, in no less than 28 there were evident signs of pneumonia, as shown either by the diseased condition being confined to one lung, by its having advanced to solidification, or by the coexistence of marks of inflammatory action in the pleuræ, or bronchial mucous membrane. In 11, the lungs were in a doubtful condition, presenting the characters which are common to the first stage of pneumonia and to passive congestion, without there being any collateral circumstances, by which the diagnosis could be more clearly established. In 9, the lungs were found more or less diseased, but not inflamed or congested; and in 14 only were they quite healthy.

Pleurisy was found so often by Velpeau after these and other operations, that he proposed to give the disease the name of the "pleurisie purulente des opérés."[1] Rokitansky, Routh, and Kiwisch describe the inflammatory lesions of the chest as very common in the puerperal fever of Vienna and Prague; and I have seen it in some cases in Edinburgh; but generally, like many of these internal inflammations in puerperal and surgical fever, the phenomena of it are not very marked during life. This, like the other inflammations in these cases, is often latent in its symptoms, though the post-mortem results show how intense the morbid action had been. But, 4. In obstetric cases the uterus, uterine appendages, and peritoneum, are the most common constant seat of the accompanying internal inflammatory action, though the organs of the thorax, &c., are not unfrequently also affected. Various causes appear to lead to this special localization of the inflammatory action and effusions in puerperal fever. All kinds of wounds of the pelvic and generative organs are particularly liable to be followed by peritonitis, when they give rise to surgical fever.[2] In midwifery cases, the uterus—the seat of the original wound—is, like the external wound in surgical cases, liable to inflame; and this morbid action readily extends, by the law of continuity of tissue, to its appendages and peritoneum. The table of Dr. Chevers proves how very common peritonitis is in instances of surgical fever (being met with in above 30 per cent. of his dissections); but, besides the preceding reasons, there is another important one why it should be still more frequently met with in puerperal fever. The uterus and its peritoneal and other coats and appendages have been the immediate seat of injury and lesion in the act of parturition; and in all febrile diseases complicated with or ending in disseminated inflammations, we find the attendant inflammation particularly liable to localize itself upon any weak or injured part. The injury or previous diseased state of a joint is thus sometimes observed to regulate the seat of attack of gout or rheumatism. In cases of hemiplegia, herpes is said to attack the paralysed side in preference to the other. And in this way smallpox, &c., is sometimes observed to be specially prevalent upon a previously weak or injured part of the surface, though that part may not be at all a common seat of confluent variola. Thus—"A sailor was admitted into the Dreadnought on account of a bruise inflicted on one side of his breech by a fall into the hold of a ship. In the course of some days he left the hospital, having recovered from the injury, but ll showing a bruise-mark on the breech. A short time after, he was ain admitted with severe febrile symptoms, which terminated in e eruption of small-pox. The pustules were discrete, and very few number, all over the body, except in the exact seat of the former uise—and there they were extremely numerous, and for the most rt confluent."[1] Might we, in any cases, regulate in any degree e principal site of the secondary inflammatory effusions in puerral or surgical fever, by lowering, by some means or other, the tality of a selected portion of the surface of the body? Various ntinental surgeons have spoken highly of the beneficial action of e actual cautery to parts of the surface in bad cases of surgical ver. Does it act upon the principle alluded to?

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[1] Revue Médicale for 1826, vol. iv. p. 418.

[2] "Dr. Addison has long been in the habit of alluding in his lectures to the fact, that injuries and operations, especially when occurring about the generative organs or parts around the pelvis, are apt to be followed by peritonitis or pleurisy of the most rapidly fatal description."—Guy's Hospital Reports for 1843, p. 83.

4. *Analogy in the Symptoms of Puerperal and Surgical Fever.*—here is almost no disease which varies more than puerperal fever oes in different cases, in the intensity of its symptoms, and in the rms which they assume. Most authors have hence described veral varieties of the disease, as the inflammatory, the bilious, e gastro-enteric, the nervous or ataxic, &c., &c. The same reark, with respect to the variability of its type or forms, holds ood with regard to surgical fever. But when either disease is lly marked, the symptoms are sufficiently striking, and sufficiently similar in each—the more marked consisting of rigors; a ulse varying in strength, but always constant with regard to the ct of its rapidity; an altered and frequently darker, or almost teric hue of the surface; the skin sometimes hot and dry, someimes bathed in perspiration, or these states alternating, without ny material crisis in the febrile action; local pains, and functional erangements in the parts which are the seat of the internal inammatory effusions, but the local symptoms of these local inammations are often, it is to be remembered, very masked and tent; anxiety, and general prostration and adynamia; very freuently nausea and vomiting; occasionally diarrhœa, and latterly, bored or hurried respiration; sometimes sudden swellings and ffusions in the joints and subcutaneous tissue, &c.; and often, at st, rapid sinking, with or without delirium. There is no disease o which it is so difficult to assign a set of pathognomonic phenonena as puerperal and surgical fever; but whoever has seen much f the symptoms of the disease in one class of patients, has no lifficulty in identifying the disease by these same symptoms in the ther class.

In the preceding remarks, I have not attempted to discuss fully and minutely all the various points of analogy between puerperal fever and surgical fever. My object has been principally to direct, however imperfectly, the attention of my professional brethren to the subject, under the strong hope that the future comparative study of the disease, and of its characteristic symptoms, lesions, and causes, in the puerperal and in the surgical patient, may yet serve mutually to illustrate the pathology of this fatal affection in each class of cases; and perhaps this may hereafter lead to the discovery of more enlightened principles of prevention and of treatment, than the isolated and divided study of the same malady respectively by the obstetrician and by the surgeon has in times past been fortunate enough to elicit. And I do believe that if any man should ever have the good fortune to detect or suggest any simple and practicable measures, either to avert and prevent, or to mitigate and cure, surgical and puerperal fever, he would, in doing so, confer one of the greatest of all possible benefits upon the advancement of surgery and midwifery, and be the means of saving numerous lives in operative and obstetric practice. The discovery of any such measure or measures would undoubtedly form a most important era in the march of professional discovery. Nor does it seem utterly hopeless to expect the possible detection of some such measures, in the way of *prevention* at least, if not in the way of cure. We are the more encouraged to hope for such a result, as we already know various conditions capable of increasing on the one hand, and of decreasing on the other, the chances and the intensity of surgical and puerperal fever. The disease, for example, is confessedly more common and more severe among the population of towns, than among the population of the country;[1] it is more frequent and more fatal among hospital patients than in private practice; and much more so in crowded wards than in those where the patients are few and provided with a full and free supply of fresh air. There are epidemic states in which puerperal and surgical fever is frightfully common; others in which it is very rare and apparently difficult to excite; some localities and towns are far more frequently the seat of it than others are; and various states of the economy seem to predispose the constitution to it, or against it. Surgical and obstetric patients suffering under internal organic diseases (par-ularly of the abdominal viscera), and under certain functional rangements of the kidneys,[1] and perhaps of other organs, seem pecially liable to be attacked with this consecutive fever and in- mmation. In addition to the common antiphlogistic precautions d measures, various special prophylactic measures have been pro- sed with the view of guarding patients against attacks of puer- ral fever, when epidemic in its character, such as large and peated doses of quinine, muriate of iron, &c., before or imme- iately after delivery. This is a line of inquiry that seems par- cularly to demand attention, and to deserve further careful in- stigation and research. Some surgeons have, with a view of ndering their operations more successful, subjected their patients previous preparations, regimen, and drugging. But the exact ower and propriety of these and other measures have by no means et been precisely fixed and ascertained. Of the propriety and effi- cy of one class of prophylactic measures against puerperal fever, w practitioners in this country have any doubt. I allude to those easures which are calculated to prevent the medical practitioner or e nurse being the unhappy medium of carrying the contagion of e disease from one puerperal patient to another. This is not a fit pportunity to enter into such a long question as that relative to he occasional contagion of puerperal fever; nor to inquire under hat different circumstances and conditions it may prove capable f being communicated, as whether it ever originates from the mere halation of contagious effluvia, brought near to the puerperal atient, &c. I shall content myself with observing here (what I ave taught elsewhere for the last ten years), that there exists, I elieve, on record, a series of facts amply sufficient to prove this, at ast, that patients during labor have been and may be locally inocu- ated with a materies morbi capable of exciting puerperal fever; hat this materies morbi is liable to be inoculated into the dilated nd abraded lining membrane of the maternal passages[2] during elivery, by the fingers of the attendant; that thus in transferring from one patient to another, the fingers of the attendant act, as it ere, like the ivory points formerly used by some of the early vaccinators; that the materies morbi most capable of being thus inoculated and generating the disease in a new individual seems to be the inflammatory products effused upon the serous or mucous surfaces of females who are suffering under puerperal fever, or who have died of it; and lastly, that other inflammatory products, when in the same way transferred and inoculated into the puerperal female, appear to have sometimes the same effect, such as the effusions into tissues, that are the seat of an asthenic, erysipelatous, or gangrenous type of inflammation.[1]

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[1] Budd, in Medico-Chirurgical Transactions, vol. xxv. p. 129.

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[1] Among nearly the same amount of population living in the towns, and living in the country districts of England, childbirth is far more fatal among the inhabitants of the former than of the latter. In the four years ending 1841, there died out of a town population of about 1,800,000, 3195 women in childbirth; and out of nearly the same amount of rural population there died only 1806 mothers. The excess in towns was thus more than 1000 lives; the mortality was 221 and 137.—(Mr. Parr, in Registrar-General's Fifth Report, p. 408.) A similar difference will, I believe, be found to hold good relatively to the success of amputations and other surgical operations in town and country practice.

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[1] Among the 153 fatal surgical cases published by Dr. Chevers, in as many as 93 of the tients some lesion was found in the kidneys, liver, or spleen. In 72 of these 93 cases the kidneys were observed to be in a state of marked disease, either presenting remarkable ngestion, softening, mottling, or the granular or cystiform alterations." In 21 of the re- aining cases there was "marked disease of the liver or spleen."—Loc. cit. p. 91.

[2] We know from the phenomena of dissection-wounds, how very small an abrasion or ound is sufficient to allow of the fatal inoculation of the morbid poison. Mr. Travers, tates a case of dissection-wound of the finger of a medical gentleman followed by fatal ver, inflammation, and gangrenous erysipelas, where the original lesion or point of inocu- tion was so minute that it was not seen by the naked eye, till it was first detected by using ens.—See his work on Constitutional Irritation, vol. i. p. 224.

Believing as many practitioners do, in this occasional communicability of puerperal fever, it becomes, of course, their bounden duty to avoid as far as may be, the possible propagation of it in this manner, and to use every available precaution against such a sad misfortune. And the omission of these precautions has proved, I sincerely believe, the cause of many deaths in childbed, and continues still to do so, particularly on the continent of Europe. In the large hospital at Vienna, out of 21,120 women delivered from 1840 to 1846, 2260 died; or about 1 in every 10 mothers delivered, perished, chiefly from puerperal fever. Latterly this mortality has diminished so far that, in 1848, not above 1 in 74 mothers died. This great change was effected, in consequence simply of means being adopted to prevent the contagious inoculation of the disease being carried and transferred by the medical attendants and students, from those already affected and dead of the malady, to women who were in labor. Before 1847, almost every woman delivered in the wards attended by the medical students, was examined by a number of students; and these students had been often allowed immediately previously to touch and handle the bodies of women who had died of puerperal fever, and were even taught upon them the manipulations and operations of midwifery. The mortality altered and diminished immensely and immediately from the time, May, 1847, that the assistant-physician, Dr. Semelweiss, prevented students from touching parts at the autopsies, and directed all of them to wash their hands in a solution of chlorine before and after every vaginal examination.[2]

In this instance, and in others which might be cited, was not the want of a due knowledge of the communicability of puerperal fever cause immediately leading to these numerous maternal deaths? ere these mothers not sacrificed merely to medical prejudice, in e form of a total disbelief on the part of the attendant physians, in the contagious communicability of puerperal fever? Is ot the very high mortality seen in most continental lying-in hositals, as compared with those of Great Britain, a result principally f inattention to the doctrine of the communicability of puerperal ever? And, lastly, if puerperal fever may be occasionally commuicated by inoculation to puerperal patients, may not surgical fever e occasionally communicated to surgical patients in the same way? The question is perhaps a far more momentous one than the simple past neglect of it might *à priori* lead us to infer. It would be perhaps wandering too far out of my own province if I were venturing to discuss it here. But I may state that, on inquiring into the subject, I have repeatedly heard of instances of a rapid succession of surgical fever cases and disasters in the practice of the same surgeon, while the other surgeons in the same locality had their patients recovering as usual; and I have been told of one eminent surgeon having locked up all his cutting instruments for some weeks, under the impression that they were in some way or other infected, and unaccountably dangerous to all the patients upon whom, for a short time previously, he had occasion to employ them. A gentleman, who was formerly surgeon to a very large hospital, and also an extensive practitioner in midwifery, informs me that, during the period of his surgical superintendence of the hospital, and when consequently often touching the discharges from all kinds of wounds and breaches of surface, puerperal fever was, from time to time, common in his private obstetric practice—and, at the same period, he saw many of his hospital surgical patients die with similar symptoms. Since giving up the surgical charge of the hospital, the occurrence of puerperal fever has ceased in his private midwifery practice. A sufficient series of such cases would so far add another proof of identity in the two forms of fever—the puerperal and surgical—by proving an identity in their origin and mode of causation.

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[1] It is perhaps worthy of remark, that in cases in which puerperal fever is thus communicated and produced, two or three days after delivery usually elapse before the disease breaks out; or, in other words, there is a latent period similar to what we see in the inoculation of small-pox, measles, and other communicated diseases.

[2] See Wieger in the Révue Medico-Chirurgicale, vol. vi. p. 136; and Routh in Medico-Chirurgical Transactions, vol. xxxii. p. 27. In his interesting work on Austria, Mr. Wilde informs us, that in the Vienna Hospital new patients were sometimes placed for delivery in the warm beds from which patients who had died of puerperal fever, had just been removed.

ON THE
COMMUNICABILITY AND PROPAGATION OF PUERPERAL FEVER.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1851, p. 72.)

DR. ARNETH, of Vienna, having read a paper on the cause of the puerperal fever at the Lying-in Hospital of Vienna; and Dr. Moir having related the history of some cases which had lately occurred in his practice, Dr. Simpson expressed a similar opinion of this last series of cases, to what Dr. Moir himself had given, viz., that the original focus of contagion in them was to be traced to the diseased blood and tissues of the mother who was first delivered and first attacked—that her blood had affected the infant which she carried within her—and that probably the vaginal secretions and discharges from this said patient during labor had unhappily formed the virus or material which had been unwittingly carried by Dr. Moir, so as to affect his other patients.[1] It was only by careful and searching analysis of cases of puerperal fever, like Dr. Moir's, when they did occur, that we could hope ultimately to arrive at a knowledge of all the various ways and means in which the disease may originate or be spread, and consequently of all the different means which may be adopted to prevent its spreading. Dr. Hill, of Leuchars, has described one instance which was interesting in this respect, that, as in Dr. Moir's, both the mother and the child seemed affected before delivery. A carpenter had his hand wounded and poisoned by the discharge issuing from a dead body, whilst placing the corpse in the coffin. A severe attack of erysipelas followed. Subsequently his wife had a similar attack of erysipelas. Their daughter living with them, and in the seventh month of pregnancy, was then taken with an attack of fever. In a day or two she gave birth to a dead child, whose body had all the appearance of being affected with erysipelas, as the arms of the mother's parents previously were. The mother herself died within twenty-four hours, with the symptoms of malignant puerperal fever. On his road home from visiting this patient, Dr. Hill was called to a case of labor, and this other was also attacked with puerperal fever. Dr. Arneth's very valuable paper adduced what was apparently incontrovertible evidence of puerperal fever being propagated in the way he suggested, viz., by medical men carrying on their fingers matter capable of producing it from bodies which they were dissecting, and inadvertently inoculating that matter into the mucous membrane of the vagina of patients in labor. In these cases, the fingers of the accoucheur when once dipped in the poison might retain it, till they had again inoculated that poison into the bodies of other healthy subjects. The vaginal mucous membrane was generally stretched and abraded in labor; the perineum was often slightly torn; and the whole afforded a surface in a condition easily inoculable. But if students and practitioners, with their hands containing some portion of morbid matter, can thus, by inoculating that matter on the abraded surface of the vagina, produce puerperal fever, no doubt, under similar circumstances, surgeons could and did inoculate into the wounds which they made or dressed, similar matter producing the similar disease of surgical fever in their patients. If it could be inoculated into the abraded surface of the vagina, it could be inoculated into a recent wound. If it produced fever in the one set of patients, it would produce fever in the other. And since bringing under the attention of the profession the communicability of surgical fever, Dr. Simpson stated, that he had heard various facts in regard to it, all of which more and more convinced him that surgeons, like accoucheurs were occasionally the unhappy media of inoculating their patients with morbid matter, producing in them surgical fever, as in puerperal patients, obstetricians, by the same means, produced in their patients puerperal fever. He had no doubt that it would take many long years fully to convince surgeons of this fact; but still it was his conviction, that surgeons would ultimately both believe and act upon it, and that their doing so would be a means of preventing many of the numerous deaths which now occur after operations, particularly in hospital surgical practice.

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[1] From Proceedings of Medico-Chirurgical Society, April 16, 1851.

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[1] For details of Dr. Moir's cases, by himself, see Ed. Monthly Journal, July, 1851.

Continental accoucheurs generally did not understand exactly the kind or description of evidence upon which British practitioners founded their belief in the contagious communicability of puerperal fever. Some of the continental writers on this subject seem to imagine that British obstetricians believe that puerperal fever was usually propagated directly from one patient to another; and not seeing this occur, when a puerperal fever patient, in their continental hospitals, lay by the side of another and healthy woman they imagine that from this fact they had a disproof of the opinion of the contagious communicability of the disease.

But in this country we do not believe that the disease is usually propagated in this way, directly from individual to individual, but indirectly, through the medium of a third person; and that person generally the medical attendant or nurse. But that it was so propagated by the medical attendant or nurse, we further believe upon the following species of evidence, viz., that it was, as in Dr. Moir's late cases, and in most other instances, distinctly and precisely limited to the practice of one or two practitioners only, out of a large number of medical practitioners, practising in a large community. Many examples were recorded, and many more unrecorded were known to the profession, of the disease being thus limited to the practice of a single practitioner in a town or city; all, or almost all, the patients of that practitioner being affected with it, where none of the patients of other practitioners were seized with any attack of the disease. In these cases we could not believe it to be owing to any morbific influence present in the air, or emanating from the locality in these cities or towns. For if so, it would affect indiscriminately the patients of all practitioners. But it had been often seen, as it was just now remarked, to haunt the steps of a single practitioner, and a single practitioner only in a community. Many instances of this were known and published. One would suffice for illustration. Dr. Roberton, of Manchester, tells us, that in 1840, upwards of 400 women were delivered by different midwives in connection with the Lying-in Hospital in Manchester. These 400 women were delivered in different parts of the town at their own houses; 16 of them died of puerperal fever; all the others made good recoveries. The production of this could not have arisen from any general epidemic, or atmospheric, or telluric influence; for the fatal cases occurred in no one particular district, but were scattered through different parts of the town. Now, these 400 women and more, were attended in their confinements by twelve different midwives. Eleven of these twelve midwives had no puerperal fever amongst their patients. The sixteen fatal cases had occurred in the practice of one only of the twelve. The disease, in fact, was limited entirely to her patients. There must have been something, then, connected with that one midwife, in which she differed from the other midwives, inasmuch as all her patients took the disease, whilst the patients of all the other midwives escaped from it. And in medical philosophy, we cannot fancy that this something, consisted of aught else than some form of that morbific principle or virus, to which pathologists give the name of contagion.

Further, that the disease is really, in such instances, propagated by this third person (the physician or the nurse) carrying to the parturient patients a virus capable of producing the disease, is shown by this kind of additional evidence: That when the disease has broken out in the practice of one accoucheur, it will spread to the practice of others of his obstetrical brethren, provided they put themselves in a condition to carry the contagious virus from the patients of the first practitioner. In 1836 or 1837, Mr. Sidey of this city had a rapid succession of five or six fatal cases of puerperal fever in his practice, at a time when the disease was not known to exist in the practice of any other practitioners in this locality. Dr. Simpson, who had then no full and proper belief in the contagious propagation of puerperal fever, attended the dissection of two of Dr. Sidey's patients, and freely handled the diseased parts. The next four cases of midwifery which Dr. Simpson attended were all affected with puerperal fever, and it was the first time that he had seen it in practice. Dr. Paterson of Leith examined the ovaries, &c., from these cases in Dr. S.'s lodgings, as he was at the time collecting facts for valuable papers on Corpora Lutea. The three next cases which Dr. Paterson attended in that town were attacked with the disease. It was upon evidence of this kind that British pathologists generally rested in founding their belief on the contagious communicability of puerperal fever.

And it was evidence of this kind which had intuitively driven them to adopt those means of prevention or avoidance, which are so highly necessary, in order to arrest the propagation of this fearful malady. The measures proposed and so successfully adopted by Dr. Semelweiss in the Vienna Hospital, were beautiful from their mere simplicity; but they were full also of a great lesson to us all. They proved, in a manner beyond all dispute, the great importance of carefully ridding the fingers from all matters in the least degree likely to prove hurtful if inoculated into the vagina of a puerperal patient. And no doubt, as Dr. Arneth had remarked, such matters were always present in the fingers as long as, despite even of common ablutions, they emitted a disagreeable animal odor, the presence of that odor being a perfect proof of the presence of morbid matter capable of producing the odor. Drs. Semelweiss and Arneth recommended, for the purpose of ridding the fingers of this morbific matter, the use of chloride of lime. Dr. Simpson had used for the same object for years daily (or rather, generally often during the day), a solution of cyanide of potass, which was more effective even than chloride of lime, and had this other advantage, that it removed readily and at once all such stains as the fingers of the accoucheur were apt to receive in treating uterine diseases—with nitrate of silver, iodine, and the like.

Dr. Semelweiss believed that animal matter, in a state of *putrefaction*, was the material which constituted the inoculable virus capable of being transmitted to puerperal patients, and of producing puerperal fever in these patients. Dr. Simpson had strong doubts as to the idea of this matter being necessarily putrid being correct. We see cases in which animal substances are allowed to putrefy within the vagina, and to be applied to the mucous membrane of that canal, without producing puerperal fever. When a polypus, for example, was ligatured and left in the vagina, it often was killed and putrefied there for days before the stalk was completely cut through by the applied ligature. And yet in these cases the patient had little or no liability to attacks of disease like puerperal fever. Besides, in these cases, the other condition is present, of an abraded surface, as well as putrid matter in contact with that surface, for the vagina was sometimes, no doubt, more or less injured in its mucous surface while passing the ligature; and the ligature itself always made a raw, open, and inoculable surface, as it cut through the pedicle of the tumor. Surgery on other parts of the body admitted of many similar proofs against this doctrine. Dr. Simpson had always believed and taught another theory, but not perhaps a perfectly correct one, in regard to the nature of the contagious material. He believed that generally, if not always, the material which, when carried from one subject to another, could produce puerperal or surgical fever in a newly inoculable subject, was an *inflammatory secretion*, just as the inoculable matter of small-pox, cow-pox, syphilis, &c., was an inflammatory secretion. The case adduced by Dr. Arneth, of puerperal fever breaking out in the hospital apparently in consequence of matter being conveyed from cancer of the uterus to a series of puerperal patients, was not so strong an argument against this view as might at first sight appear. For the cancer patient was, according to Dr. Arneth's own account, several days in labor, the carcinomatous degeneration of the cervix preventing the opening of the os. And there can be very little doubt, that by the end of several days the carcinomatous structures were in a state of inflammation, and probably gangrenous decomposition, from the protraction of parturition. At all events, if the carcinomatous cervix was really putrid, it was in all likelihood putrid from the result of gangrenous inflammation in its compressed and irritated structures. But be this the case or not, it was important to remark, that obstetricians had now very decided proof of various kinds of morbid matters which were capable, when inoculated into the vagina, of leading on to puerperal fever.

For, *first* of all, when the bodies of patients who died of puerperal fever were opened, the inflammatory effusions in the abdomen and elsewhere, when brought in contact with the fingers of the accoucheur, were capable of producing the same disease in other healthy patients upon whom they were accidentally inoculated. In other words, the morbid effusions of puerperal fever in one woman were capable of producing puerperal fever in another woman when inoculated into her system.

But, *secondly*, the same seems to hold true with regard to the secretions coming from the bodies of such patients, even when they did not die and were not dissected. Dr. Simpson alluded to the cases, for example, of nurses and midwives, whose fingers came into contact with the discharges from the vagina of puerperal patients, giving the disease to other parturient women, and who had not, of course, in the way of post-mortem examinations, been bringing their fingers in contact with the more internal secretions. Dr. Gordon mentions more than one case of this kind in relation to midwives, in his history of the Aberdeen puerperal epidemic.

Thirdly, he believed that the cases recorded by the late Mr. Storrs, Hutchinson, Ingleby, and others, sufficiently proved that the inflammatory secretions in some other inflammatory diseases besides puerperal fever, when carried by the medical attendant, and inoculated into the maternal canals of a parturient female, were sometimes capable of producing in such females true puerperal fever. This seemed more particularly true with regard to the inflammatory effusions in erysipelas and gangrenous inflammation of the limbs, scrotum, vulva, or other part of the body. That the morbid matters thrown out in those more subacute forms of disseminated or phlebitic inflammation which sometimes occur after delivery, were capable of producing puerperal fever when inoculated into puerperal patients, was a fact of some importance to hold in view. And the following recent case will perhaps impress the truth of it. A short time ago, Dr. Simpson was requested to see a case of pelvic abscess in a patient delivered four or five weeks previously. The abscess was artificially evacuated, but only with partial relief; as there were evidently other local inflammations going on, both in the abdomen and chest. The patient died about six or seven weeks after delivery. The practitioner who originally attended her, and who had no puerperal fever cases in his practice, was not able to be present at the dissection. Another able medical practitioner, whom he had called to the case after the inflammatory attack had begun, opened the body. Though an excellent and well-informed physician, he rather decried any fear about the possibility of contagion, when Dr. Simpson suggested it to him as he came into the room, and found him opening the body. This gentleman had no puerperal fever cases in his own practice; but within fifty hours after opening this body, he happened to be called to five cases of midwifery. Four of these patients were attacked with puerperal fever, three in a very severe, and one in a mild or abortive form. The fifth patient altogether escaped, the child having been born before the practitioner's arrival.

Fourthly, there were one or two recorded circumstances which would lead one to the belief that some varieties of febrile exhalations received by inhalation into the blood of a newly delivered woman, are capable of producing in her a disease analogous to, if not identical with, puerperal fever, the effect being the same as if morbid matter had been introduced into her blood, not by inhalation into the lungs, but by inoculation and imbibition into the vagina, just as in the spreading of small-pox we see the disease liable to be produced in two ways—first, by the direct inoculation of the morbid inflammatory matter contained in pustules on the arm of a healthy individual; or, secondly, by individuals inhaling the morbid effluvia from the bodies of patients laboring under the disease, without its being inoculated into them. Dr. Collins mentions an instance in which a patient was admitted into the Dublin

Lying-in Hospital, laboring under a bad form of typhus fever. Two puerperal females, who occupied the adjoining beds, were attacked with puerperal fever and died. In another instance, in the same hospital, a similar accident happened. A patient laboring under typhus fever was admitted into one of the small wards of the house, which contained only some four beds—all the three other women were attacked with puerperal fever, and two of them died. But we had no very decided evidence, so far as Dr. Simpson knew, from hospital observation, that a woman, laboring under puerperal fever, could, by the exhalations from her body, infect with the same disease other patients lying near her in the same ward.

Fifthly, some accoucheurs believe in the possibility of the imbibition of the effluvia from typhus or puerperal fever patients by the clothes of the medical attendant, and that the subsequent inhalation of such matter by the parturient female, might be a means of artificially infecting that female with the disease. Dr. Simpson could not doubt that the saturation of the bed-clothes, &c., with the discharges of a puerperal fever patient, might give the same disease to another puerperal patient who was laid in them. This, and one or two other circumstances, were enough to show that, for safety's sake, it was always well to act upon the possibility of the clothes even of the medical attendant being thus a medium of contagion. In some observations on the subject of the contagion of puerperal fever, Dr. Merriman states, that he once attended the dissection of a puerperal patient, but did not touch the body or any of the parts. The same evening he attended a lady in labor, and she was attacked with the disease. In his account of the Aberdeen fever, Dr. Gordon mentions that a man-servant appeared to carry the infection of the disease from his sister in Aberdeen to his wife in the parish of Fintry, six miles from Aberdeen. The midwife who attended this woman infected two other parturient patients in the same parish soon afterwards, both of whom died. If a statement of this kind could be established as a fact, by careful analysis of the requisite evidence, it would be a matter of importance, as adding to our knowledge of the modes in which this disease may be propagated. In the instance which Dr. Moir had mentioned, of Dr. Hamilton visiting the patient of another practitioner, affected with puerperal fever, and immediately after having several cases in his own practice, it was not at all unlikely that he had made some examination of the patient, or, at all events, without proof that he had not, it would not be proper to conclude that the disease in that instance could be carried by the clothes of the physician acting in the way of *fomites*. The history of the other case adduced by Dr. Moir, of the fever breaking out on shipboard, when bed-clothes had been used which had been employed previously in the beds of women who had died of puerperal fever, would be exceedingly important in the way of proof, if it had been more substantially reported and authenticated. One can scarcely believe that such clothes should be shipped after being thus used, without having been previously thoroughly washed and cleansed. Dr. Simpson had also been informed of an instance by Professor Patterson, in which a medical gentleman, after having lost several cases of puerperal fever, got rid of the disease in his practice by changing his clothes, and using chloride of lime, &c., but it again returned to him when he happened to deliver a patient immediately after wearing a pair of gloves which he had used during the time of the puerperal epidemic; and certainly, if there was any piece of dress more apt to retain the contagion than another, it was this useless and superfluous appendage to our attire; for it might retain the morbid secretions that were originally on the fingers of the accoucheur, just as our vaccinating glasses would retain the cow-pox matter.

Sixthly, in a small ward or small hospital, one could almost, as it were, manufacture puerperal fever at will, by crowding a great number of puerperal patients together in the same ill-ventilated room. The discharges from the different patients in a few days rendered the air of such a room so loaded and morbific, as to be oppressive to all entering it, and capable of producing febrile action by the inspiration of it, in those puerperal patients who occupied its beds. This, no doubt, was true when this experiment was driven, as it sometimes accidentally had been, to an extreme. But it was true also in its lesser degree; for Dr. Simpson believed that one great cause of weed, ephemera, and febrile attacks during puerperal convalescence, was the still too slight attention that was paid to the ventilation of the lying-in chamber. He had repeatedly, he thought, seen more or less slight febrile action set up in a patient, from the curtains being closely drawn around her bed for eight or ten hours during the night, being thus obliged to breathe an air loaded and affected with the morbid animal discharges from her own body.

Seventhly, Dr. Arneth had not alluded to the question, Whether the disease was ever caused or not, or a predisposition at least given to it, by epidemic influence? Dr. Simpson believed that we ought not altogether to forget the possibility of epidemic influences acting directly or indirectly in the causation of it. During the present century the disease had nearly, in two or three instances, as in 1819–20 and 1829, prevailed in most of the cities and lying-in hospitals of Europe. And it was difficult or impossible to account for this simultaneous existence everywhere, without believing that everywhere there was some general epidemic cause tending to its production. In this the history of puerperal fever did not differ from the history of other contagious febrile diseases. During the latter part of the last century, for instance, small-pox contagion existed in almost every town and village in England, because in almost every one of them there were artificial causes operating to produce and perpetuate the disease, inoculation being very generally practised. But it was only in particular years, and sometimes at a considerable distance of time, that the disease became epidemic. And when it did so, it was owing to other causes being in action in addition to the mere inoculation. Nay more, in some conditions, as during the blowing of the Harmattan wind, we know that small-pox and cow-pox cannot be propagated even by direct inoculation—facts, showing us the influence of epidemic constitution in effecting a greater or less tendency to the production and spread of particular diseases.

One predisposing cause to attacks of puerperal fever was no doubt the state of the constitution of the patient immediately after delivery. Dr. Collins' cases in the Dublin Hospital showed, not only that the disease was far more apt to attack those who were worn out by long labors, than those women who had escaped with parturitions short in their duration; but also, that the malady, when it did appear, was much more fatal in the former than in the latter class of patients. The Society was aware that it had been proposed by various pathologists of late years to give various prophylactic medicines to puerperal patients after delivery, and to surgical patients after operations, in order to prevent the attacks of puerperal or surgical fever. All these measures, such as sulphate of quinine, muriate of iron, &c., had the object in view of strengthening the constitution of those to whom they were exhibited, so as to diminish or destroy the predisposition to these feverish attacks. And we could understand their proposed mode of action when we reflected upon the fact, that a predisposition to such attacks was given by any unusual degree of exhaustion or debility in the patient. Every patient exposed to the contagion, and even to the inoculation, of small-pox, for example, did not take small-pox. There were other means by which the predisposition to that disease was reduced or removed, than by previous variolation or previous vaccination. And, perhaps, particularly, or otherwise, by medicine, we may be able to reduce or remove the predisposition to puerperal fever, as well as to scarlatina, measles, &c.

Lastly, Dr. Simpson observed, that no doubt sporadic cases of puerperal fever frequently did occur, traceable to no contagion or any other cause capable of being averted; some of them owing, as in Dr. Moir's first case, to morbid actions going on in the constitution of the patient even before delivery; but oftener owing to morbific agencies—capable, under other circumstances, of producing fever or inflammation—acting upon the patient during or after delivery.

Dr. Arneth had particularly called the attention of the Society to the connection which was generally believed by British accoucheurs to exist between erysipelas and puerperal fever; and he had stated that the relation between these two diseases had not been observed in Vienna. Dr. Simpson, however, expressed his opinion, that now that Dr. Arneth's attention had been directed to it, he and his compatriots would find such relations existing between these two diseases, as English accoucheurs spoke of. We all of us, often overlook such facts in pathology till our attention happens to be prominently called to them. Dr. Simpson had long believed and taught that there was a pathological connection between the two diseases in question, as to their pathological nature, their pathological anatomy, their symptomatology, and their causation. The two diseases had in Britain been repeatedly observed to prevail at the same time, in the same town, in the same hospital, or even in the same wards. There were various accurately recorded instances in our British journals, which he had already alluded to as showing this—that when the fingers of medical men were impregnated with the morbid secretions thrown out in erysipelatous inflammation, the inoculation of these matters into the genital canals of parturient females produced puerperal fever in them, in the same way as the inoculation of the secretions from patients who had died of puerperal fever itself. The effused morbid matters in the one disease, as in the other, were capable of producing the same effect when introduced into the vagina of a puerperal patient. In an instance recorded by Mr. Hutchinson, two surgeons, living at ten miles distance from each other, met half way to make incisions into a limb affected with erysipelas and sloughing. Both practitioners touched and handled the inflamed and sloughing parts; and the first parturient patients that both practitioners attended within thirty or forty hours afterwards, in their own distant but respective localities, were attacked with, and died of, puerperal fever. The late Mr. Ingleby mentions an instance of a practitioner making incisions into structures affected with erysipelas, and going directly from this patient to a patient in labor. This patient took puerperal fever and died. And within the course of the next ten days, seven cases of puerperal fever occurred in the practice of the same practitioner, almost all of them proving fatal. And various other cases, similar to the preceding, were well known to the profession.

Again, however, the reverse of this was equally true. Not only was the morbid matter in erysipelas apparently sometimes capable of producing puerperal fever, but the secretions and exhalations from puerperal fever patients seemed, on the other hand, sometimes capable of producing erysipelas. In the series of puerperal cases met with by Mr. Sidey in this city about 1837, and which had already been alluded to, the morbific matter carried from two or three patients seemed, as has been previously stated, to produce the disease, both in Dr. Simpson's own practice, and also in the practice of Dr. Paterson. The morbid effusions of these patients created the same disease of puerperal fever in other patients to whom that matter was carried. But the morbid secretions and exhalations from these same patients appeared to do more, viz., they produced also erysipelas in several of the nurses, relations, and attendants upon the patients. Four or five cases of erysipelas followed upon a single one of these puerperal cases in Mr. Sidey's practice, and that during the week subsequent to the puerperal patient's death. The patient's mother-in-law, who was in constant attendance upon her, was attacked with fever and erysipelas of the face and head. One of the patient's sons, a boy five years of age, was attacked with erysipelas of the face; a daughter was seized with fever and sore throat, with dusky redness, which continued for some time; and the patient's sister-in-law was attacked with acute gastric symptoms, and great abdominal irritation, under which she sunk in a few days. Here we have apparently the same focus of contagion producing puerperal fever in puerperal patients, and erysipelas, inflammatory sore throat, &c., in patients who were not in a puerperal state. Dr. Hill of Leuchars had published in the Monthly Journal of last year, two very important series of cases, showing, in a similar way, the connection between puerperal fever and erysipelas, in the identity of the poison that was capable of producing these two diseases. We have already seen that the inoculation of the morbid matters from erysipelatous structures into parturient patients will occasionally produce puerperal fever. But furthermore, the converse of this is so far also true, viz., that the inoculation of morbid matters or secretions from puerperal fever patients into other healthy individuals, will occasionally produce in the latter, attacks of erysipelas. A considerable number of instances have been published by Dr. Duncan, Mr. Travers, and others, in which medical men have died from punctures received in dissection, or rather from erysipelatous inflammation of the arm and side following such punctures. The history of a large proportion of these cases shows further, that the matter thus inoculated, and which produced the fatal erysipelas and fever, was a puerperal fever secretion; as in most instances the disease resulted to these medical men from opening the bodies of patients who had died of puerperal fever.

Whilst thus arguing for some pathological connection between erysipelas and puerperal fever, Dr. Simpson further stated, that though, in a few cases, patients laboring under puerperal fever had been attacked with erysipelas of the skin and cellular tissues; and on the other hand, patients who were delivered when suffering under erysipelas, were sometimes subsequently attacked with puerperal fever; yet these results were not always observed. He alluded to cases where erysipelas had attacked women before delivery, but was not followed by puerperal fever. And he had seen more than one patient laboring under puerperal fever have inflammation of the skin, but that of a pustular type, like ecthyma, and not of an erysipelatous character. In stating this, he wished to state his belief, that the diseases were not in all respects pathologically identical, though the morbid secretions in the one were capable of producing, in those predisposed to it, the other disease—erysipelatous effusions producing puerperal fever, and puerperal fever secretions producing erysipelas.

Other febrile and inflammatory products, besides those of puerperal fever, when inhaled through the lungs into the blood, or inoculated into the blood through the vagina, may, as already stated, probably produce puerperal fever, in addition to those we have principally spoken of, viz., the secretions from puerperal fever and erysipelatous patients. And he thought the whole subject one of extreme importance to have more thoroughly investigated; because, in a disease like puerperal fever, it was the means of prevention that we were to look to, and to expect success in, more than the means of cure. It was here, as elsewhere, evident that human life would probably be saved to a far greater extent by studying the means of preventing the causation of disease, than by any study of the means of treatment, after disease was once actually commenced. And when it was remembered that about 3000 women still died in childbirth in England and Wales alone, every year, and that a large proportion of these 3000 maternal deaths were deaths from puerperal fever, he thought he need not make any further observations on the importance of studying the means of prevention and prophylaxis in such a fatal and formidable malady, nor offer any apology for the length of the remarks which he had ventured to offer on this important subject.

PATHOLOGICAL OBSERVATIONS ON PUERPERAL ARTERIAL OBSTRUCTION AND INFLAMMATION.

(Communicated to the Medico-Chirurgical Society of Edinburgh, 4th Jan. 1854.)[1]

It is only within the last thirty years that the inflammation and obstruction of veins has become a prominent subject of research. Yet all are now ready to acknowledge the high practical importance of phlebitis and its effects in puerperal, in surgical, and in medical pathology.

The inflammation, however, and obstruction of arteries has hitherto been little attended to, especially in midwifery. They are not subjects mentioned even, so far as I know, in any obstetric work; and, doubtless, they are not so common as venous inflammation and obstruction. But arterial obstruction and inflammation will be found, I believe, to be much more frequent than is supposed —1. When we come to know better, and really search for, their effects and symptoms in the living body; and 2. When we do—what has hitherto been almost entirely neglected to be done—namely, look properly for their existence in our dissections of the dead body.

In the present communication, my principal object is merely to prove the fact that arterial obstructions do occasionally occur in the puerperal patient, and are accompanied with various important pathological consequences. As far as our present very limited and very imperfect investigations go, these arterial obstructions in the puerperal female seem capable of being produced by a variety of morbid causes, particularly—1. By the separation of old or organized cardiac vegetations, and their subsequent transference into the arterial canals; 2. By the passing forward into the current of the circulation, of recent fibrinous masses formed in the cavities of the heart or larger arterial vessels; 3. By local arteritis; 4. By laceration of the internal coats of the occluded vessels; and 5. By morbid materials carried from the systemic venous system, and lodged in the pulmonary artery or its branches. I propose to adduce one or more examples illustrative of each of these pathological causes of arterial obstruction in the puerperal female; and to add a few general remarks upon the causes, and the symptoms, or effects of such obstructions.

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[1] The following essay is drawn up from the notes used on that occasion; but several more recent cases are now added to it. See abstracts of the communication in the Medical Times for 1854, p. 41; and Provincial Journal for 1854, p. 94, &c.

SECTION I. ARTERIAL OBSTRUCTION FROM SEPARATED CARDIAC VEGETATIONS BLOCKING UP THE ARTERIES.

This pathological cause of sudden arterial obstruction was for the irst time, I believe, specially noted in a puerperal patient whom I ittended in 1842, along with Dr. Abercrombie and Dr. Beilby. The following particulars with regard to the case are stated by me n the published proceedings of the Edinburgh Obstetric Society, or 15th June, 1847.[1]

CASE I.—The lady, about a year before becoming pregnant, abored under a very severe attack of rheumatic endocarditis. During the later months of utero-gestation, she suffered greatly from attacks of difficult breathing, which amounted sometimes to orthopnœa. About the eighth month hemorrhage suddenly supervened, and I found that, in addition to her other complications, she had the edge of the placenta projecting over the posterior lip of the uterus. After the os uteri was nearly dilated, the membranes having been ruptured some hours before, without entirely suppressing the very severe and exhausting hemorrhage that was present, I extracted a child, who is still living, with the long forceps. The mother seemed for the time to make a most perfect and satisfactory recovery. Some symptoms of irritation, however, supervened; and, during the second week after her confinement, I one morning found that there was no pulse to be felt in the right arm lower than the elbow, whilst it was distinct and strong down to that point. This forearm had felt, for some hours previously, coldish, stiff, and benumbed. In the course of a few days, the pulsation in the right radial artery gradually but feebly returned; but the circulation, first in one and then in the other leg, seemed to be similarly affected. At last, unequivocal symptoms of erratic phlebitis began to show themselves, and, five weeks after delivery, ended in a fatal attack of phlegmasia dolens in the left arm and left side of the face. On opening the body, the vena innominata on the left side, and its affluent trunks, were found entirely obstructed by coagulable lymph. The humeral artery, at the bend of the arm, was shut up by an obstructing mass of coagulum; but the inner coat of the vessel had no appearance whatever of laceration. The valves of the left side of the heart were profusely covered over with small wart-like excrescences. The lower limbs were not examined.

In first describing the preceding instance of puerperal disease to the Obstetric Society, I proposed the three following questions:— 1. "Was the obstruction of the artery, or arteries, in this case, produced by any mechanical cause, as one of the vegetations separated from the cardiac valves, carried along, in the case of the arm, for example, to the bifurcation of the humeral artery, and impacted there?" 2. "Was it not rather the result of an original puerperal arteritis?" 3. "Or might it be the effect of an effusion of coagulable lymph from phlebitic inflammation in the coats of the artery, a secondary phlebitic deposit upon the lining arterial membrane."

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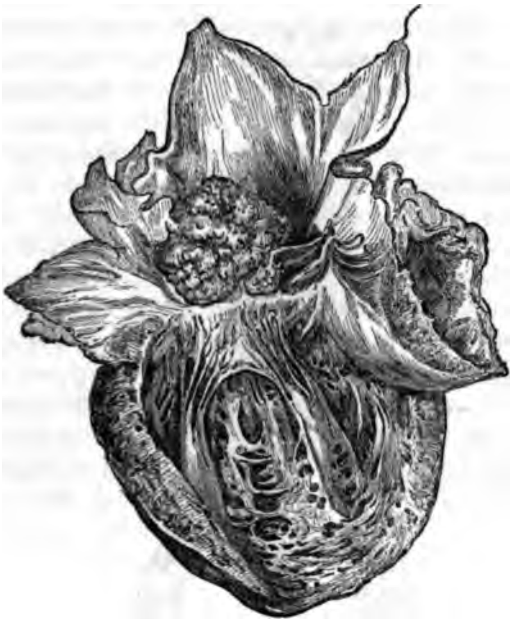
[1] See Monthly Journal of Medical Science, 1847, p. 211.

In the summer of 1853, I saw, in consultation with my friend Dr. Moir, a similar case, in which, from the symptoms present during life, I made the diagnosis of arterial obstruction from separated cardiac vegetations—an opinion that was confirmed by the results of the autopsy.

CASE II.—The patient was prematurely delivered in the country, of her first child, and continued to recover favorably for three weeks, when remittent feverish symptoms and diarrhœa supervened, and the lochia became slightly hemorrhagic. Soon afterwards pains, like those of neuralgia, were experienced in the right leg, and then in the left, where they remained fixed and occasionally very violent. Seven weeks after delivery, sudden pain and tenderness came on in the left groin. At this time I heard a loud systolic bruit on listening to the heart. No rheumatism had preceded. Some days subsequently, the pulse in the right arm became arrested suddenly, as the evening before it had been felt by Dr. Moir. Next day a careful examination was made of the arteries of the two lower extremities. No pulsation could be traced in the left femoral artery or its branches. The pulse in the right femoral and iliac was very weak, and in a day or two entirely disappeared, so that now the arterial pulsation in the larger vessels of all the extremities had ceased, with the exception of the left arm. It returned slightly for a few days before death, in the right wrist. At last, in the left lower extremity, where latterly there had always been excessive pain, gangrene of the great and two next toes began, ten weeks after delivery; and the mortification had not extended beyond these toes when the patient gradually sank and died a few days afterwards.

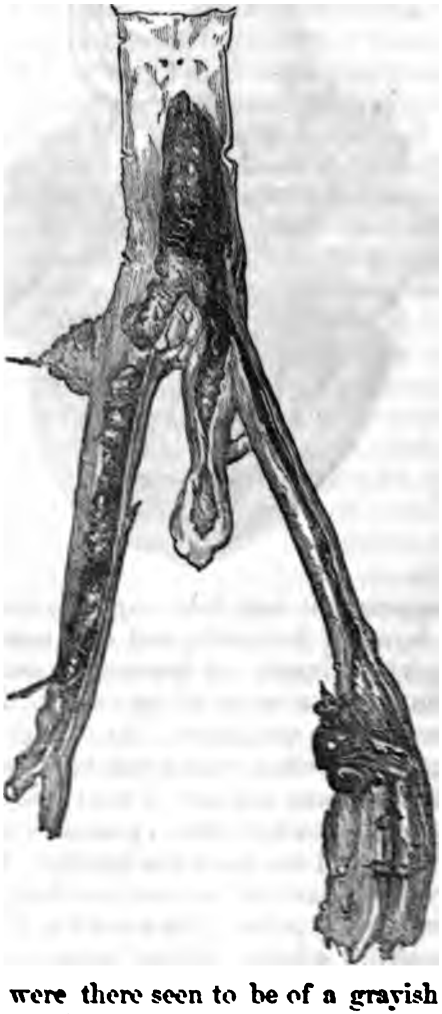
On opening the body, and examining the heart, the aortic aperture was blocked up by a mass of valvular excrescence, which appeared nearly as large as the whole calibre of the artery. It is represented in the accompanying wood-cut, Fig. 1. This excrescence was composed of three portions, one attached to each of the three semilunar valves. The left valve presented only a comparatively small excrescence on the centre of its free border. The vegetation attached to the right valve formed by far the largest proportion of the morbid mass; while that on the posterior valve was intermediate in size. The whole excrescence seemed of comparatively recent formation, being very friable and granular. In each valve, the corpus aurantii seemed the centre of the morbid growth, and

Fig. 1.



each vegetation appeared to have been originally developed between the two serous layers of the valves, and only secondarily to have protruded through these layers, and become projected as cauliflower-like growths, into the free cavity of the vessel. The openings of the coronary arteries were unaffected. Around the margin of the left auriculo-ventricular orifice, were a number of small red excrescences or carunculæ, about the size of sago grains, but evidently of the same nature as the larger masses placed on the aortic valves. The pericardial surface of the heart was healthy. The aorta, at the fourth lumbar vertebra, and iliac vessels were firmly bound by exudation to the surrounding parts. The aorta was found occluded at its lower extremity, by a large irregular mass, composed of small portions similar in consistence and general appearance to the vegetations formed on the aortic valves. The mass thus situated in the aorta, extended more than two inches above its bifurcation, and was somewhat conical in form—the apex pointing upwards, while prolongations were sent from its base along the common iliac arteries on each side. It was loose, and unattached to the walls of the vessels, and was covered with a coating of dark-colored blood, through which, at scattered points, portions of the granular structure were seen projecting, not colored like the rest, but bearing to the naked eye a marked resemblance to the valvular excrescences in the heart. In the right common iliac artery, some of the detached concretions were found not covered with blood, but lying loose in the canal of the vessels, and were there seen to be of a grayish color, and granular appearance. The external iliac arteries on both sides were occupied by sanguineo-purulent matter, and here and there mixed with this purulent matter were small granular concretions, similar to the granular mass above. On the right side, this inflammatory appearance of the artery extended down as far as Poupart's liga-

Fig. 2.



ment, but on the left side it reached two inches below Poupart's ligament; and a small granular mass was found blocking up the origin of the profunda on the left side. Two inches down the left internal iliac artery, a pulpy mass was found blocking up the vessel, and from this a yellowish pus extended upwards to the origin of the artery. The mode and extent to which these concretions filled up the lower part of the aorta is represented in the wood-cut, Fig. 2. Let me add, that the whole coats of the obstructed vessels were thickened, and the lining membrane in contact with the occluding masses was of a deep scarlet color. Along the external iliacs the lining membrane assumed a dirty red color. The left femoral vein for two inches below Poupart's ligament was occupied by a fibrinous clot. The lower part of the right humeral artery presented an inflammatory condition, in some respects similar to that of the iliac arteries. No portions of excrescence were found in its cavity, but at the bifurcation into the radial and ulnar branches, a pulpy mass was seen adherent to the lining membrane of the vessel, and seemed like a small granular portion of the valvular excrescence broken down and mixed with pus, which occupied the vessels at this situation. The humeral artery as far upwards as the pectoralis minor, and the radial and ulnar branches for several inches downwards, were quite obliterated and seemed like small cords firmly bound by exudation to the adjoining textures. When a portion of the mass from the aortic valves was placed under the microscope, it was seen to be composed of separated fatty granules, with blood-corpuscles, and a few compound granular cells interspersed. The sanguineo-purulent matter in the neighborhood of the masses deposited in the aorta and other vessels, was seen to be crowded with pus-corpuscles; while the masses themselves were chiefly composed of fat-granules, blood-corpuscles, and compound granular cells, like the excrescences present on the cardiac valves. The uterus presented no appearance of disease either in its cavity or walls. The spleen was pulpy and diffluent, except at one point, where there was a small mass of a cheesy consistence, and grayish-white color. The other viscera were healthy.[1]

For notes of the following two analogous cases, I am indebted to the kindness of my friends, Professor Macfarlane of Glasgow, and Dr. Lever of London.

CASE III.—In this instance, the obstruction of the artery occurred suddenly ten days after the lady was delivered naturally and easily of her fifth child. At that time she began to complain of acute pain and numbness in her right arm, which, with slight intermission

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[1] The morbid parts in this case were, when still recent, shown to the Medico-Chirurgical Society.

from opiates, &c., continued till her death, which took place at th
end of three weeks. No pulsation could, from the first or subs
quently, be felt below the middle of the brachial artery. A wee
after this arterial occlusion in the upper extremity, a similar chan
took place in the right thigh, accompanied with acute pain. Th
ceased in a great measure four or five days after, when unequivoc
indications of gangrene showed themselves, commencing at the to
and extending to the knee. On dissection, the aortic valves we
found by Dr. Macfarlane covered with numerous vegetations, no
of which exceeded in size a grain of linseed. The aorta was dilat
and studded over with atheromatous deposits. At the points
obstruction (viz., the middle of the brachial, and the commenc
ment of the lower third of the femoral arteries) fibrous clots we
found, which completely closed these vessels. The upper or cardi
end of the clots contained, and was firmly attached to, *a small ha*
body, which, on more careful inspection, was found to be identic
in size, appearance, and structure with the aortic excrescences.

CASE IV.—This patient suffered under an attack of acute rhe
matism during pregnancy, and died of a few days' illness after d
livery. During the short illness following the confinement, it w
discovered by Dr. Lever, who attended the lady, that there was
pulsation in the arteries of the left arm; and subsequently the sa
want of pulse was observed in the left inferior extremity. The pa
in both extremities was distressing; the hand and foot were bo
livid in color, and local symptoms of gangrene were manifest
each. At the post-mortem examination, mushroom-like vege
tions were discovered on the valves of the heart; and in the arteri
of both the affected extremities, vegetations were found obstructi
their canal, and in the veins of both were phlebolites.

The first case which I have mentioned above, occurred, as I ha
already stated, in 1842, and was published in 1847. During t
same year, 1847, Professor Virchow, of Wurzburg, perhaps t
most original and industrious of living pathologists, published
essay on inflammation of arteries,[1] in which he mentioned seve
cases of arterial obstruction apparently produced by cardiac vege
tions detached and carried forward into the arterial circulation.
1852, Dr. Kirkes read an excellent paper before the Medico-Chir
gical Society of London on the same subject;[2] and Ruhle,[3] Tuffn

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[1] "Ueber die akute Entzundung der Arterien," in Archiv. für Pathologische Anato
vol. i. (1847), part ii, p. 272. See cases, pp. 338, 342, &c.

[2] London Medico-Chirurgical Transactions, vol. xxv, p. 285.

[3] "Three cases of Hemiplegia produced by the plugging of one of the cerebral arteri
—Medical Times and Gazette for March, 1853, p. 299.

[4] Dublin Quarterly Journal of Medical Science, vol. xv (1853), p. 371.

and other pathologists have subsequently reported some additional instances. The cases observed by Virchow, Kirkes, Ruhle, &c., were not in puerperal patients. The instances detailed by Kirkes and Ruhle, referred principally to detached cardiac vegetations lodging in the cerebral vessels, and producing death by encephalic lesions. The following interesting case, which occurred in the wife of a very esteemed obstetrical friend, shows that the same lesion may occur from the same cause in the puerperal female. For the following particulars of this case I am indebted to Dr. Burrows, who, during the life of the patient, made a perfect diagnosis of the exact morbid states found after death.

CASE V.—The lady was seen by Dr. Burrows about six weeks after delivery; she was then in that state of semi-hectic which results from over-lactation. Besides this she had obscure pains in the limbs, resembling rheumatism, and more particularly pain in the calf of one leg, and other symptoms, which led him to suspect she might have abscesses forming. All these symptoms gradually abated, under the influence of cessation of lactation, great quietude, tonics, and change of air. While in the country, however, and after rather more exertion than common, she became suddenly hemiplegic on the right side, but without symptoms of cerebral congestion. Dr. Burrows saw her on the following day, and immediately suspected the case to be one of the class detailed by Dr. Kirkes. Auscultation confirmed these suspicions, for there was a loud rasping systolic murmur over the valves. The hemiplegia and impaired powers of speech and memory remained to the time of death.

At the autopsy abundant vegetations were discovered on the mitral and aortic valves, so that they were softened and ulcerated through. The left corpus striatum was reduced to a mere diffluent pulp, and the branch of the left middle cerebral artery passing to this part of the brain was obliterated, with a small mass of fibrin, like a grain of wheat, implanted in the vessel at its origin from the middle cerebral artery. The artery beyond the obstruction looked like a pale, thin string, and was quite impervious. The arteries of the limbs, in which the patient, at an early part of her illness, had suffered so much pain, were not examined.

The five preceding cases were all apparently of the same nature, and there were two special circumstances true as applied to the whole of them—1st. In all the five, vegetations were found on the aortic valves; and 2d. Loose bodies, having the same physical appearance and structure as the cardiac vegetations, were discovered on dissection in the obstructed arteries.[1] That the cause of arterial obstruction in these instances was not local arteritis, is proved by the supervention of the symptoms of arterial obstruction being sudden and almost instantaneous; and by the obstructed artery, in some instances, as in Case V, showing no post-mortem evidence of thickening or previous inflammatory disease. In all, the vegetations were probably the result of previous endocarditis; and in three of them, rheumatic symptoms had been present at a preceding period of life. Furthermore, let me observe, it seems certain, that if vegetations are once detached from the cardiac valves, they must be carried forward in the current of the circulation. Are there, however, any circumstances or reasons which render it probable that such vegetations ever become detached? In favor of this view, analogy may be appealed to in reference to the spontaneous separation of adventitious structures in other parts of the body. Thus, cartilaginous and other pediculated bodies become detached in the interior of joints; polypi from mucous surfaces; and polypoid or fibrous growths, from the free surface of the peritoneum, as described by Dr. Reid and Mr. Hodgkin. In the heart there are conditions which render the separation of vegetations much more probable than perhaps the detachment of polypoid growths in other parts of the body. For 1st. The vegetations, whether sessile or pediculated, are often loosely attached, being easily removed after death, with the handle of the scalpel. 2d. The valves to which they are adherent are parts constantly in motion. 3d. Currents of blood are ever rushing over them with considerable force. When the vegetations are once separated, they will be carried along, until, meeting at last with a vessel whose calibre is smaller than their bulk, they become impacted; or they may become arrested where a larger vessel divides into two branches, each of which is smaller than the detached vegetation. In the occluded artery more than one result may follow such impaction. Accumulating coagula may be formed from the blood around and behind the obstructing mass. The artery may inflame at the site of its occlusion, and this inflammation may ultimately involve, as in Case II, the accompanying vein. The vegetations and coagula thus deposited may at last become disintegrated and pulpy, as occurred in the first and second instances of the group of cases that we have now described; and their own detritus, or the inflammatory exudations formed around them, may pass downwards and obstruct still smaller arteries beyond; or, if minute enough, may even traverse the capillary circulation, vitiate the general mass of blood, and produce, as in Case I, phlegmasia dolens, or other consequences. And lastly, the arterial pulse may betimes become restored in the artery below, either from gradual clearance or dilatation of the obstructed vessel above; or, as perhaps happens more frequently, from an increase in the collateral circulation.

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[1] In Ruhle's first case, in a patient aged 19, the cardiac vegetation found on the aortic valves, and the plug found in the left arteria fossæ sylvii, were both calcareous; and the walls of the occluded artery were free from all inflammatory changes. In a similar case reported by Dr. Burrows, both the cardiac vegetation and the plug in the middle cerebral artery were cretaceous. See Medical Times for 1853, pp. 136 and 299.

SECTION II.—ARTERIAL OBSTRUCTION FROM THE ESCAPE OF RECENT FIBRINOUS OR UNORGANIZED COAGULA FROM THE LEFT CAVITIES OF THE HEART, OR FROM THE LARGER INTO THE SMALLER ARTERIES.

In the well-known experiment when recently drawn blood is actively stirred, or whipped *with* a rod, the rod forms a mechanical nucleus around which the fibrine readily and speedily coagulates. In the same way in the living system, under particular circumstances, when the blood in the heart or large vessels is carried or whipped *against* a fixed rod or a rough body, occasionally such obstructing points constitute mechanical nuclei upon which coagula form, and from which they may subsequently separate and be carried forward with the arterial current till they are ultimately arrested in some arterial canal too contracted and small to allow them to pass further. Accident has sometimes offered us a kind of crucial pathological experiment upon the living human being demonstrative of the preceding remark. Thus, in a case shown in 1849 by M. Laugier to the Anatomical Society of Paris, a needle happened to be thrust through the apex of the heart into the cavity of the left ventricle. The needle was found after death, fixed with its long axis running vertically from the apex of the heart towards the aortic orifice of the ventricle. The patient, a young man, to whom this accident happened, died with gangrene of the left leg. It became an interesting pathological question, what was the relation between the cardiac injury and the gangrene of the limb? The dissection seemed to resolve this problem. For the whole length of the needle projecting into the cavity of the left ventricle was covered with decolorized irregular concretions, which were prolonged beyond the apex of the needle into the cavity of the aorta. The lower end of the aorta and the iliac arteries were filled with adhering sanguineous concretions; and it occurred, observes Cruveilheir, to all the members of the Society who saw the preparations, that some of the clots formed around the foreign body in the left ventricle of the heart had been projected forward from it into the arterial canals, and become there the nuclei of new obstructing concretions.[1]

In the preceding case we have a foreign body, in the interior of the left ventricle forming a basis around and upon which unorganized sanguineous concretions accumulate; and these concretions, soon after their formation, becoming detached and projected into the arterial canals so as to close them,—produce gangrene,—like the similar arterial plugs resulting from the separation and transference of old and organized valvular vegetations. But if the interior of the same ventricle be anywhere mechanically rough and irregular—not by a foreign body, such as a needle—but by the results of disease, as from the presence of globular polypi in the cavity, vegetations on the valves, or endocarditic inflammation of its lining membrane—similar unorganized coagula may at times form in superfibrinated or other morbid states of the blood upon these roughened and diseased points, and subsequently shut up arterial canals in different parts by being projected from the heart into the current of the circulation. Thus, in a case described in 1837 to the French Academy of Medicine by M. Legroux,[2]—a lady, aged 29 years, passed through an attack of acute rheumatism. Three months subsequently to her first attack there was uneasiness in the precordial region, an irregular pulse, and a loud bellows-sound; and these symptoms were attributed to endocarditis. Antiphlogistic treatment was employed. At the end of a month she seemed to be getting well, when all at once violent pains came on in the legs and feet, with coldness of the extremities. In a fortnight the arterial pulsations had completely ceased in the lower limbs. Eight days before death, the same symptoms were observed in the left arm, and the pulsations ceased there also. The pains continued to the last; no gangrene took place. On post-mortem examination, the cavities of the heart were found to contain old and adherent fibrinous clots; and the lining membrane of the auricles had lost its polish and transparency. The right subclavian artery, the termination of the aorta, and the common and external iliac arteries were completely obliterated by old and adherent clots. The right iliac artery was dilated, at the level of the hypogastric, to the volume of a nut, and contained a fibrinous, cystiform coagulum. Three of the lumbar arteries were closed by clots, which projected into the aorta like the heads of nails. As M. Legroux points out: –1st. This disease was originally rheumatic endocarditis. 2d. In consequence of this endocardial inflammation, fibrinous coagula were formed in the cavities of the heart. 3d. The obliteration of the arteries was commenced by the expulsion of these coagula from the heart into the vessels.[1] 4th. The arterial obliteration, at first imperfect, was completed by successive additions of coagulum; for the structure of the clots indicated that they had been formed at various times. 5th. Probably the adherence of the coagula to the walls of the artery was the result of inflammation, caused by the presence of the clot as a foreign body, the inner membrane of the artery being only notably altered at points where the obliteration had existed for some time; that of the subclavian, for example, which was recently closed, being only slightly injected.

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[1] See Cruveilhier's Traité d'Anatomie Pathologique Generale, tom. ii, pp. 201, 296, and 388.

[2] "Cette opinion est fondée sur des faits assez nombreux recueillis par lui (M. Legroux), et dont il possede les pieces anatomiques,—faits qui demontrent, dit il, une coincidence presque constante entre les caillots obturateurs des artères, et des caillots anciens dans les cavites du cœur." In the history of the subject this passage is important, as having been published in 1837.

In the puerperal state—with the blood, as in rheumatism, overcharged with fibrine and otherwise abnormal—recent coagula seem sometimes to form on rough points in the interior of the heart, and subsequently becoming projected into the arterial current, shut up one or more arterial canals. To produce this result two elements are perhaps necessary:—1. A chemical tendency in the blood from superfibrination or other causes, to coagulate; and 2. Mechanical facilities for the formation of coagula, from the presence of rough surfaces or projections. The two following cases of puerperal arterial obstruction probably belong to this category. In the first of these cases, a rough globular polypus, such as is sometimes found in the interior of the heart as a result of phlebitis and endocarditis, seems to have served as a nucleus for the formation of the sanguineous coagula or concretions. In the second case, for notes of which I am indebted to Professor Macfarlane, rough cardiac vegetations, which did not, however, themselves separate, appear to have acted a similar office.

CASE VI.—The patient, æt. 33, was admitted into St. Thomas' Hospital, August 2, 1853, under the care of Dr. Risdon Bennett.[2] She had generally enjoyed good health, but for several weeks before her last confinement, which took place nine weeks before her admission, she had debility, dyspnœa, and œdema of the legs, with pain in the left side and occasional palpitations. She had never suffered from rheumatism. Her labor was difficult, and her recovery after it had been imperfect. A fortnight before admission she had pain across the chest and down the left arm. A week later there occurred apparent syncope, followed by impaired vision and slight cough, with frothy bloody expectoration. When she entered the hospital she seemed extremely ill, but there was no sufficient cause discoverable to account for her alarming symptoms. There was, however, tumultuous, feeble, and irregular action of the heart, attended by a variable bruit. Besides this there was some puffiness of the ankles, and pain and tenderness generally in the lower extremities. On the 5th, or three days after admission into the hospital, the left leg appeared much as if affected by phlegmasia dolens. It was painful, paler than natural, and towards the foot of a lower temperature than the rest of the body. A day or two after this the opposite limb exhibited similar symptoms, and then the toes of both feet became shrivelled and parchment-like. By the 13th of August both limbs were gangrenous, some vesication appeared on the thighs, and no pulsation could be felt in the femoral arteries, even under Poupart's ligament. On the 3d of September she became typhoid and died on the 6th. At the autopsy no vegetations were found on the cardiac valves, but under the attached fold of the mitral valve. A thinning of the walls of the left ventricle existed, and a notable bulging of the cavity, like a commencing "diffuse true aneurism." This cavity did not contain coagula, but at the apex of the ventricle was a partially adherent globular cyst or polypus, and another very similar was found in the right ventricle. These polypi were softened in the centre, and filled with pus-like matter. The aorta was empty as far as the origin of the superior mesenteric artery. From this point downwards it was filled with a clot in different stages of change. Just at the bifurcation of the aorta the clot seemed to be very firm, and adhered with more tenacity on the left than on the right side, where the artery continued patent for some distance lower. The iliac arteries, common, external, and partially internal, were blocked up, as was also the femoral, for two inches below the groin. The clot adhered pretty firmly to the arterial walls, but when peeled off, the internal coat was not roughened. The clot had begun to soften in different parts, more especially along the aorta itself, and the products under the microscope were precisely similar to the one presented by the globular cysts or clots in the heart. All the arteries which were given off from the aorta between the superior mesenteric and deep femoral were blocked up for a short distance. Each of the femoral veins at the groin contained a short recent clot. The crural nerves were implicated in the surrounding inflammatory changes and thickening. In both kidneys there were fibrinous or purulent deposits. All the other organs examined were healthy.

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[1] Bulletin de l'Académie de Médecine, 1837, tom. i, p. 434.

[2] This interesting case has been published by Dr. Bennett in the Provincial Medical and Surgical Journal for 1854, p. 143.

CASE VII.—A lady, aged about 38, had, after an attack of rheumatic fever seven or eight years previously, become subject to cough and dyspnœa. Three or four years after, when Dr. Macfarlane was first consulted, the physical signs of endocarditis and of disease of the aortic valves were well marked. She became pregnant for the first time in 1841, and was delivered with the forceps after about thirty hours severe labor. Six hours before delivery was effected, she complained of violent pain in the right arm, which gradually increased, and almost appeared to exceed in intensity the parturient action. As she could only lie on the right side, and was obliged to do so during the whole labor, little attention was paid for some time to this symptom, under the belief that it was the effect of position and pressure. The persistent severity of the pain, however, and the distress it appeared to occasion, ultimately led to the arm being examined, when it was found that the pulse had ceased at the wrist, and that no arterial pulsation could be detected below the axillary end of the subclavian. The pain, especially on each side of the elbow, at the wrist and in the fingers, was violent and incessant, and continued for six weeks, alleviated only from time to time by opiates, anodyne frictions, and heat. The pulsations never returned to the main vessels, but the heat of the limb was soon restored, and the shrivelled and exsanguine appearance of the points of the fingers and nails, at first so marked after the obstruction took place, gave way to a more natural appearance as the collateral circulation became established. She died of general dropsy three months after delivery. Dr. Macfarlane could only obtain permission, and that after much importunity, to examine the state of the subclavian artery, at the point of its obliteration. It was found completely plugged up between the clavicle and axilla by a firm fibrinous clot, consisting of several layers, and hence of successive growths, but it did not contain any roundish or hard portion, or any appearance whatever of an excrescence or vegetation separated from the valves of the heart.

I am not aware of any direct observations, either in the puerperal or non-puerperal states, showing that sanguineous concretions or coagula may occasionally form around rough and diseased points existing in the walls of the larger arteries of the body, and be thence separated and carried onwards into smaller arteries, so as to obstruct and occlude the latter. Such specimens, however, are not rare in our pathological museums. The Museum of the University of Edinburgh contains, for example, a specimen of this kind, unfortunately without a history. There are rough, ulcerated, atheromatous spots on the interior of the aorta, with coagula adhering to their edges, and the tube of the iliac artery below is completely shut up with a mass of sanguineous coagula. In a case of gangrene of the leg, published and delineated by Sir Robert Carswell,[1] the artery of the affected limb is occluded with fibrinous concretions, and the interior of the aorta above is full of coagula, formed around atheromatous obstructions in the walls of the vessel, and all pointing downwards in the direction of the circulation.

Before leaving these two first sections, or forms of arterial obstructions, it is, perhaps, proper to observe, that the exact position and site of obstructions in the arterial tree, produced by detached cardiac vegetations, and fibrinous clots, appear to be regulated by various circumstances, some of which seem recognizable. 1st. Where the morbid mass which leaves the heart is very large, or has a large coagulum gathered around it, it sometimes proves of too great a size to pass at all out of the aorta, and is then usually carried down and lodged in the lowest part of that vessel, or immediately above its bifurcation. 2d. When of less size, and capable of entering arterial canals of the second, third, or still smaller dimensions, the course and ultimate site of the vegetation or coagulum is perhaps principally determined by the directness of the arterial current into particular arterial tubes, and by the angles at which these vessels leave their larger trunks. Thus, small solid masses floating in the blood are more liable to be carried upward, according to Ruhle, into the left than into the right carotid, because the stream of blood, running from the heart and aorta, pours more directly into the former than into the latter vessel; and Virchow attributes the greater frequency of their presence in the left lower limb than in the right to a similar cause—the left iliac artery leaving the abdominal aorta at a less angle than the right. 3d. The mode in which an artery divides may regulate their seat. Cardiac vegetations have now been very often found lodged at the mouth, or in the course, of the middle cerebral artery. This, according to Dr. Kirkes, is owing to the fact, that, after the division of the internal carotid into its two main branches within the skull, viz., the middle and anterior cerebral, the middle cerebral not only remains the larger vessel of the two, and hence is more capable of receiving any floating body; but further, it maintains also far more nearly than the anterior cerebral, the original direction of the trunk from which they both spring, and hence is more likely to have sent forward, into its canal, any solid body floating in the current of blood that passes upward through the carotid.

In all the preceding remarks, I have only spoken of organized vegetations and unorganized fibrinous masses passing from the heart into the arteries, of such a size as to obstruct and occlude vessels of considerable dimensions. But vegetations and coagula of a far *smaller* size are ejected from the heart perhaps still more frequently, and ultimately obstruct and collect in some of the minuter arteries and capillary vessels. The more or less decolorized, angular, lobulated deposits, frequently seen in the spleen, kidney, liver, &c., sometimes, if not always, originate in this manner, and primarily consist of cardiac concretions and fibrine, or occasionally of pus and fibrine, carried into and lodged, first, in the smaller arteries and capillaries of these parts, and subsequently accumulated in the vessels and tissues behind and around these obstructions. In other words, these instances of so-called "capillary phlebitis," or of yellowish "fibrinous deposits" in the organs named, and in other parts distant from the heart, appear to consist usually and primarily of solid particles of fibrine carried from the interior of the heart or larger vessels, and lodged and accumulated in the capillaries and smaller arteries at the sites of the deposits. Out of twenty-one instances in which Dr. Kirkes observed these yellow or white masses in the spleen, kidneys, and other parts supplied by blood directly from the left side of the heart, he found in nineteen of the cases disease of the valves or of the interior of the heart (in fourteen there were fibrinous endocardial growths); in the twentieth, the aorta was the seat of aneurism, with laminæ of fibrine lining it; and the twenty-first, or only exceptional case, was one of cholera, where a doubtful mass of capillary phlebitis existed in the liver.[1] In describing the *secondary* phenomena liable to be produced by endocarditis, Professor Hasse observes, "A considerable portion of the effused substances being carried away in the first instance by the circulation, and another portion during the period of softening, it is obvious that these inflammatory products must, just as in phlebitis, act a subordinate part within the capillary system. The spleen and kidneys appear particularly liable to such changes. In the spleen, coagulated fibrine is found of some breadth at the periphery, but gradually tapering towards the centre, having mostly a tolerably sharp outline, and a brownish-yellow hue. Smaller deposits of a similar kind are met with in the cortical substance of the kidney, sometimes even intersecting the papillæ. According," he adds, "as the inflammatory product of the endocardium partakes more of the character of fibrine or of pus, the secondary deposit will be of a more or less consistent kind, and shrivel in the event of recovery, or else liquefy and terminate in abscess. Though frequent in the localities above indicated, these secondary phenomena are proportionately rare, as in the liver; sometimes, however, they occur in the serous cavities."[1]

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[1] See his Pathological Anatomy. Article, "Mortification." Pl. iii. fig. 2.

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[1] Medico-Chirurgical Transactions, vol. xxxv, pp. 307–309.

SECTION III. ARTERIAL OBSTRUCTION FROM LOCAL ARTERITIS.

In the two classes of arterial occlusion which I have already described, there was generally found thickening, condensation, and other evidences of inflammation in the walls of the artery at the obstructed points. But, in these instances, the arterial inflammation was secondary, and not primary. It was the result of the irritation and impaction of a plug, carried from a distance and fixed in an arterial tube, which became first obstructed, and then inflamed, by its presence. But we come now to speak of another division of arterial obstructions, in which the arterial occlusion is the result, and not the origin, of arterial inflammation; its cause, and not its consequence.

Without entering into the debated question of whether or not a part possessing no *vasa vasorum*, like the lining membrane of arteries, can be itself the seat of inflammatory exudations, and whether or not, in all cases of arteritis, the exudations thrown out are not secreted from the vessels of the more external arterial coats, and particularly of the cellular, it is enough for our present purpose to grant, what pathologists all allow, that local arteritis in general speedily ends in, if it does begin with, the formation of an obstructing fibrinous or sanguineous concretion in the tube of the inflamed artery.

The following well-marked case of puerperal arteritis, and arterial obstruction, terminating in gangrene, occurred to Dr. James Duncan, when acting as surgeon to the Royal Infirmary. I am indebted to him for the following notes of it. The preparation of the obstructed aorta and iliacs in this case, is contained in Mr. Spence's surgical museum.

CASE VIII.—A patient from the neighborhood of Dalkeith was delivered of her first child after a protracted labor. Two weeks afterwards she was brought into the Edinburgh Infirmary, suffering under pain and acute gangrene of both the lower extremities. The gangrene came on rapidly, with vesications, and ascended before death as high as the thighs. The patient sank three or four days after she was placed in the hospital. On dissection, no disease was found in the heart, its walls, valves, or cavities. But beginning about an inch and a half above the bifurcation of the aorta was found a fibrinous plug, which completely occluded the artery, and extended along the common iliacs, and, as far as one or two inches down the external iliac arteries on each side; a prolongation of the plug shut up also the canal of the internal iliac artery on the left side, while, on the right side, the opening of the internal iliac was occluded by the obstructing mass occupying the common iliac artery. The upper end of the plug in the aorta was flattened, and some lymphy filaments passed from the arterial parietes to be attached to this superior extremity. In the aorta the mass was firmly adherent to the coats of the vessel, which coats were thickened; and apparently, at the most inflamed point, the calibre of the tube was contracted. The adhesion of the obstructing concretions to the inner coat was less along the left iliac artery; the plug in the right iliac artery was not adherent at all to the coats, but presented a loose cast of the arterial canal. The inferior extremities of the plugs, in each external iliac artery, did not terminate abruptly, but were continued as a lymphy layer, like a false membrane, over the inner arterial coat, on the right side, for an inch downwards, and on the left for an inch and a half. The femoral and other arteries below were quite healthy, and the veins of the limbs presented no special morbid appearances.

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[1] Hasse on "Diseases of the Organs of Circulation," &c. Swaine's Translation, p. 133. See also p. 61, on similar deposits after aortitis, &c.

In relation to the preceding case of local arteritis, it is difficult to assign any reasonable cause for the occurrence of the obstructive inflammation. It has been suggested to me that the inflammatory action might have been produced by great pressure upon the vessel from the enlarged pregnant or puerperal uterus. But then arteries do not readily inflame and become obstructed under mere external pressure; and in every puerperal case there is a greater or less amount of this pressure on the aorta without inflammation and obstruction following.

Perhaps some of the arterial obstructions which I have previously described in the cases detailed under the first two sections, particularly in Section II, may be considered by some pathologists as referable rather to the effects of local arteritis than of plugs of organized or inorganized fibrine carried into the arterial tube from a distance. It is only by watching more carefully the suddenness or slowness of the arterial obstruction as it occurs during life, and by observing and analyzing far more carefully than has been hitherto done in our autopsies, the structure of the existing plug and the exact condition of the arterial wall at the point of obstruction, that such a question can be definitely settled.

SECTION IV.—ARTERIAL OBSTRUCTION FROM SPONTANEOUS LACERATION AND CORRUGATION OF THE INTERNAL COAT OF THE ARTERY.

When the internal coat of an artery is divided artificially, as in the forced elongation of arteries under accident—or when it ruptures, as it sometimes does, spontaneously, the divided ends of the lacerated tunic or tunics—for the middle coat may also give way—coil and curl up so as to occlude at once, more or less completely, the arterial canal. The friability and softening leading to spontaneous rupture of the lining membrane is evidently the result of a previous state of disease, in all likelihood of an inflammatory character; and so far, the present group is possibly more truly referable to the last, or to those that are indirectly, if not directly, the consequence of Arteritis.

The first case in which the sudden arrestment of pulsation in an artery during life was traced, by dissection after death, to obstruction of the arterial tube from the spontaneous rupture of its internal coat, was published by Professor John Thomson in Dr. Hodgson's excellent work on Diseases of the Arteries, in 1815. Some years subsequently—1829—Professor Turner read a masterly essay on the same subject to the Medico-Chirurgical Society of Edinburgh.[1] He cites ten cases of arterial occlusion.

In one of these ten cases the arterial obstruction occurred in a puerperal patient, but unfortunately there was no post-mortem examination instituted to show whether the arrestment of the arterial pulsation was or was not the result of spontaneous rupture and corrugation of the lining membrane of the occluded artery. The case is quoted by Mr. Turner from Dr. Parry of Bath, who details it in his work on the Arterial Pulse,[2] in the following words:

CASE IX.—"I have seen the total loss of pulse in one arm, with coldness, but complete power of motion in that part; while the other arm was warm, and possessed a perfectly good pulse, but had lost all power of voluntary motion. These symptoms commenced suddenly, two or three days after parturition. The patient soon died, but a dissection was not obtained."

In 1842 Dr. Oke of Southampton, published a case of sudden arterial obstruction occurring in the humeral artery after abortion, which he deemed referable to this "spontaneous rupture of the inner coat of the vessel, and the projection of its edges into the tube."[3]

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[1] See Transactions of the Edinburgh Medico-Chirurgical Society, vol. iii, p. 105.

[2] Experimental Inquiry into the Nature, &c., of the Arterial Pulse, p. 139.

[3] Provincial Medical and Surgical Journal for April 23, 1842, p. 51.

CASE X. In the autumn of 1831, a patient, æt. 24, during the tenth week of pregnancy had hemorrhage from the uterus. On the following day it was more profuse, and terminated in abortion. The day after delivery she was seized with severe headache, giddiness, dimness of vision, and vomiting. So great was the interruption to the vision, that she could not distinguish the hands of a common clock. The fingers of the left hand felt as though scorched, and were extremely painful. On the following day the dimness of vision continued, and there was intense pain and numbness in the left arm, which at length became cold and insensible to external impression. The wrist and the tips of the fingers were discolored, especially the tip of the ring finger, which was turning black.

On examination no pulsation could be felt in any of the arteries of the arm above the affected hand, but the subclavian was distinctly felt pulsating above the clavicle. There was no perceptible disturbance of the action of the heart, and no embarrassment of the respiration. This patient eventually recovered, with the loss only, by gangrene, of the integument of the thumb and fingers of the left hand. In three days the power of vision was restored. The pulsation, however, never returned in the obliterated arteries of the left arm, but the arm itself gradually regained its sensibility and ordinary plumpness.

As proof that in this instance the obstruction originated in the affected vessel, and not in any morbid state or lesion of the heart, Dr. Oke, in a letter to me, dated November 30, 1853, observes:—"No abnormal action of the heart or its valves could be detected when the obstruction in the left axillary artery took place, nor during my observation of the case subsequently. To make the answer (to your inquiry on this point) more complete, I lately, along with Dr. Lake, made a careful examination of the region of the heart of this patient, who is still living. After listening attentively to its action at different points, we could not detect any sound that would indicate a vegetated or morbid condition of the valves, nor indeed any other abnormal sound of the organ."

SECTION V.—OBSTRUCTION OF THE PULMONARY ARTERY AND ITS BRANCHES, BY MORBID MATERIALS COMING FROM THE SYSTEMIC VENOUS CIRCULATION, AND PASSING THROUGH THE RIGHT SIDE OF THE HEART.

Hitherto I have spoken only of the puerperal obstruction of arteries belonging to the general or systemic circulation, and produced either by local diseases and lesions in the arteries themselves, or by morbid masses projected into them from the centre to the circumference of the arterial circulation, from the trunk to the bran of the arterial tree, from the cavities of the heart or larger art into the canals of the smaller arteries. But we have now to sider an interesting group of puerperal cases of different chara and in which the pulmonary arteries or their branches bec blocked up by morbid materials carried forward into them f some part or parts of the systemic veins. In this case the mo materials, in being carried onward from the points at which are originally formed in the systemic venous system, to the p in which they become ultimately arrested and lodged in the pu nary arterial system, necessarily traverse the right auricle and r ventricle of the heart. In the puerperal female two or three ferent kinds of morbific matter appear thus occasionally to pass f the venous system, and become lodged and accumulated in the sions of the pulmonary artery, namely, pus, or some of the m rials associated with pus, separately or serving as a nucleus coagulated blood or fibrine: more solid nuclei formed by loos and detached fragments of those fibrinous concretions that so c fill and obstruct the systemic venous canals of the pelvis, ut legs, &c., in puerperal phlebitis; or equally solid portions of fib or coagulable lymph, not the result of inflammatory secretion, made up possibly of the natural plugs by which nature norm shuts up the open orifices and canals of the uterine veins after livery.

Metastatic, lobular pneumonia, originating apparently from transference and lodgment of pus in the smaller pulmonary ve and capillaries, is a complication occasionally met with in the p bitic form of puerperal fever. In the disease, as witnessed by at Prague, Dr. Kiwisch tells us that in most cases where the m tatic pneumonia was violent, he found "the *pulmonary arteri* the diseased lobe usually inflamed as far as their point of exit the pulmonary tissue, and filled with adherent lymph and p Perhaps more minute inquiry may yet prove that such obstruc of the pulmonary arteries are by no means uncommon in the monary inflammations, deposits, and abscesses which occur in nection with surgical and obstetrical phlebitis.

In the two following cases, described, and one of them deline above twenty years ago by Professor Cruveilhier in his large w on Morbid Anatomy, the pulmonary arteries and their bran had pus and fibrine lodged in their canals, as a consequen puerperal phlebitis.[2]

CASE XI.—A woman, recently delivered, was seized on the

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[1] Die Krankheiten der Wöchnerinnen, &c., 1840, p. 116.

[2] Anatomie Pathologique, livrais. ii. tab. 1 and 2, pp. 18 and 21.

of July, 1830, with all the symptoms of uterine phlebitis, which were combated successfully by leeches to the epigastrium and emollient injections. From the 25th July to the 3d of August she appeared quite convalescent, and suckled her infant without inconvenience. An invasion of new symptoms, however, took place at the latter date, namely, oppression, cough, anxiety, nervousness, and extreme frequence of the pulse. She died on the 9th of August, twenty-eight days after delivery. On post-mortem inspection, the uterus was found little above its ordinary size, but the uterine, ovarian, and almost all the hypogastric veins, were like hard cords. They owed this hardness to compact clots filling them, which were adherent and decolorized. The external iliac veins, with the left crural vein and some of its divisions, contained clots less compact and adherent, and evidently of a recent date. At the base of the left lung were found several portions of lobular pneumonia, in the state of red induration, and two superficial purulent patches. There was also œdema of both lungs in the posterior half of the two inferior lobes. Some hard concretions filled the divisions of the pulmonary artery. The lesser branches were filled with red and scarcely adherent clots, while the concretions in the principal trunk were coherent and decolorized. In the centre of the principal clot or plug was a collection of pus, which offered all the characters of phlegmonous matter; further on, the clot or plug was decolorized, and formed a complete cylinder. In this case uterine phlebitis occurred from the 12th to the 25th of July; and from the 25th July to the 3d of August the patient seemed to be recovering, when suddenly, at the latter date, symptoms of pulmonary mischief supervened, under which she sank in six days.

CASE XII.—In a woman, who died of puerperal phlebitis two weeks after delivery, and who presented typhoid symptoms during the later days of life, the primitive iliac vein and its divisions were found filled with adherent sanguineous concretions, some of older, others of more recent formation. These concretions were pale in the principal trunks, and contained pus in their centre; they were red, and gradually less and less coherent in the smaller divisions of the vessels. The lower extremities only presented a very slight œdema. The liver was of a yellowish color, and softened. The uterus was equally soft, but without any other morbid alteration. There were marks of true recent pleurisy on the posterior border of the right lung, with a combination of ordinary or diffuse and lobular pneumonia on the same side. The tissue of the lung, when divided, presented sections of vessels full of fibrinous concretions. These obstructed canals or vessels were, on further examination,

found to be the divisions of the right pulmonary artery. The
cretions in the arteries were pale and full of pus at their centre
as they divided and subdivided with the course of the arteries,
became red and hardly coherent. The concretions in the sm
pulmonary vessels only existed in the neighborhood of the par
the lung affected with lobular pneumonia. In the portion of
attacked with ordinary inflammation, the tissue of the organ wa
filtrated with pus, and in the state known under the name of
hepatization. The circumscribed patches of lobular pneum
presented great differences in respect to volume, and the atten
degree of inflammation. In one place there was red indurat
in another place marbled patches of white and red; in a third
infiltrated on the tissue, or diffused in irregular patches.

A case of puerperal obstruction of the pulmonary arteries,
similar to the two detailed by M. Cruveilhier, has been publi
by Professor Levy of Copenhagen.[1] There were symptoms du
life and evidence after death of uterine phlebitis, though not
severe form; and the patient died apparently of two consec
attacks of lodgment of morbid materials in the pulmonary arte

Case XIII.—The 19th of February, 1853, a primipara was e
confined, and seemed well with the exception of a catarrhal co
On the 20th there was pain over the uterus, but it disappeared
the application of leeches. On the 22d it returned with fever
tympanitis. On the 24th there was less sensibility, and the
had diminished. In the evening, however, she had an attac
violent dyspnœa, with painful constriction of the epigastri
anxiety, dry cough, and a quick pulse. Auscultation discov
nothing but a moist rhonchus, and the symptoms disappeared
bleeding. Convalescence went on favorably until the 3d of M
when, after supper, sudden and acute pain came on in the epi
trium, with oppression, the respiration became anxious and la
ous, and death surpervened at the end of a few minutes. On
section, Dr. Levy found the two cavities of the right side of
heart, and the pulmonary artery, even to its small divisions,
with a quantity of blood-clots of different sizes, some freshly for
others older and more consistent, of a grayish red, more or
discolored; while others, again, contained a grayish mass, fai
grained, and of a purulent appearance. The walls of the pu
nary artery were slightly tinged. There was nothing in the left
of the heart, but the aorta had four regular insufficient valves.
veins of the uterus contained concrete pus, more especially tow
the broad ligaments.

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[1] See it quoted from the Deutsche Clinik into l'Union Médicale, Dec. 27, 1855.

An instance in which puerperal obstruction of the pulmonary artery suddenly supervened during convalescence from phlegmasia dolens, and where, consequently, as in the preceding examples, the pulmonary disease was connected with, and dependent upon, previous phlebitis in the systemic venous system, was published at Berlin, several years ago, by Dr. Hoogeiveg, in the Preussische Vereinzeitung for 1852.[1]

CASE XIV.—A young woman, three days after delivery of her first child, was attacked with phlebitis of the left extremity, which yielded to appropriate treatment. But, during convalescence, she suddenly uttered a scream, fell down, and expired. On post-mortem inspection, the left crural vein and its branches were found obstructed with phlebitic coagula, which extended upwards to the junction of the crural and iliac veins. The pulmonary artery was filled with similar coagula. These coagula could be traced into some of the smaller ramifications of the pulmonary artery.

In the following instances of puerperal phlebitis, published in the Deutsche Clinik, by Dr. Hecker of Berlin, symptoms of pulmonary distress and death suddenly came on—in one case during convalescence from, in the other during the course of, the phlebitic disease; in both women obstruction of the pulmonary artery or its branches by fibrinous or sanguineous plugs was discovered on dissection; and in both, the description, as given by Dr. Hecker, of the upper ends of the concretions found in the inflamed veins, is such as to show that a portion of it was loose in the tube of the vein, and hence easily capable of being detached and carried forward towards the right side of the heart by the current of the circulation. Virchow has found the upper and floating portions of some of these venous plugs so loose that they were separated by succussion of the dead body. Hence we need not feel surprised at sudden alterations in the positions of the patient, or sudden muscular exertions apparently detaching portions of them, as in the following instances.

CASE XV.—A primipara, æt. 31, on the third day after a natural labor, was seized with phlebitis of the left crural vein, followed by œdema of the limb. She so far recovered, however, as to be able to leave her bed on the 20th day, but complained of feebleness and numbness of the foot. On the 29th day, in removing some article from the stove, she suddenly sank down; and although the intellect was clear, there was great anxiety, pallor of the face, pinched nose, gasping, very frequent respiration, a small, depressed pulse, and cold extremities. Notwithstanding all the efforts made to rally he she sank and died at the end of three-quarters of an hour. At th autopsy Dr. Hecker found the left crural vein entirely closed b fibrinous clots, which extended along the common iliac vein to th vena cava. A portion of clot an inch to an inch and a half long projec ing into the cava was quite free, and terminated by a conical poin The hypogastric vein was equally plugged on this side. On furthe examination, the two primary branches of the pulmonary arter were found filled with fibrinous coagula, which reached to thei final ramifications. These clots in the pulmonary arteries were n where adherent to the walls of the vessel, but had the same appea ance as those in the crural vein. There were no morbid appearanc traceable in the heart or head.

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[1] Loc. cit. p. 52.

Case XVI.—A robust woman, aged 30 years, after an easy de livery, on the 1st of October, suffered from severe hemorrhage i consequence of an adherent placenta, which had to be extracted Her state was satisfactory till thirty-six hours after her accouche ment, which was prolonged to the morning of the 3d of Octobe and was succeeded by febrile heat. The uterus was very tender t pressure; the skin hot and dry; the pulse 120; and there w anxiety, feebleness, and violent headache. During the next thre days the symptoms became graver, and the pulse rose to 136 or 14 A uterine lymphatitis was considered as probable. On the 6th October she seemed better, but the great frequency of the pul still produced serious fears. On the 8th, she reported herself having passed a good night and felt well. After noon, howeve notwithstanding a warning to the contrary, she got up, but imm diately fell to the ground, and rose with great difficulty. At t end of an hour and a half she was found sitting at the edge of h bed, and at four o'clock seemed dying. Her pulse was thread-lik and difficult to count; her respiration very frequent, 60 to 64 minute; the face was cold, blue, and bore the appearance of e treme anxiety; her extremities were cold. In a short time s became insensible, and died at half-past ten in the evening. O dissection, small portions of the placenta, acording to Dr. Hecke were found still adherent to the anterior wall of the uterus. In i walls, the lymphatics were filled with pus, and the veins wi fibrinous coagula. The left hypogastric veins were obstructed wi clots, which extended to some distance into the tubes of the comm iliac veins. The heart was normal, but the trunk of the pulmona artery was plugged by a thrombus or coagulum, extending into i two branches, and capable of being followed far into their ramific tions.

In all the preceding instances of obstruction of the pulmonary artery, that obstruction occurred in connection with, and as a sequence of, the existence of phlebitis and phlebitic concretions in the uterine, pelvic, or other systemic veins. But sometimes obstruction and occlusion of the pulmonary artery or its branches takes place in the puerperal state, independently of any antecedent venous inflammation of the body. The researches of Mr. Paget, Baron, Dubini, Richelot, and other pathologists, have now accumulated a number of instances in which, in the male and in the non-puerperal female, death has occurred in connection with, if not as a consequence of, sanguineous and fibrinous concretions in the pulmonary arterial system, and when there was no evidence of phlebitis being anywhere present. In two most interesting essays on "Obstruction of the Pulmonary Arteries," published by Mr. Paget in the Medico-Chirurgical Transactions,[1] this distinguished pathologist has reported a variety of such cases—none of them in the puerperal female—and has endeavored to trace the cause of the plugging of the pulmonary arteries, and the formation of coagula in them, sometimes to impediments in front to the pulmonary circulation, as from disease either in the lungs themselves, or in the left side of the heart; sometimes to coagulation of the slowly-moving blood in patients whose hearts and systems were nearly exhausted towards the close of life; in other instances, he inclines to refer the obstruction to inflammation and disease of the pulmonary artery itself; and, lastly, in others again he suggests that the coagulation of blood in the pulmonary vessels may be owing to a morbid state or states affecting the constitution of the blood itself, so as to "increase that adhesion of it to the walls of the vessels which constitutes, even in the healthy state, the greatest resistance which the heart's power has to overcome."

Out of seventeen cases collected by Mr. Paget of obstructing concretions found in the pulmonary arteries after death, in seven death occurred suddenly and unexpectedly. As I have already stated, none of the instances referred to in Mr. Paget's essays occurred in puerperal females. Out of the six cases which I have already given of plugging of the pulmonary arteries in the puerperal state, in four death occurred with fearful suddenness and rapidity. In all these instances uterine or crural phlebitis preceded the pulmonary obstruction. In the two following cases death struck down the patients with the same appalling abruptness; in both there was found, on dissection, the same fatal obstruction of the pulmonary vessels; but in neither of them was there any decided evidence of previous phlebitis, certainly none in the first of the two cases.

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[1] London Medico-Chirurgical Transactions, vol. xxvii, p. 162, and vol. xxviii, p. 352.

Could the concretions in the pulmonary vessels be the results, as I have ventured to suggest at the commencement of the present section, of the detachment and transfer by the circulation of one or more of the plugs normally formed in natural labor in the open orifices of the uterine veins after the separation of the placenta, and acting as nuclei for further coagulation when lodged in the pulmonary arteries;—or were these sanguineous concretions a local coagulation of blood produced by the morbid qualities of the blood itself—and similar perhaps in kind to the obstructing coagula which we sometimes find rapidly formed in the tubes of varicose veins? Let me add that I am indebted to Mr. Paget for directing my attention to the two following interesting cases, and I owe to his kindness the notes which I possess of the second of them. Mr. Paget had an opportunity of examining the recent morbid appearances in both of these instances, and I have his authority—the greatest I could quote on such a question—for stating that he believed the immediate cause of death in both patients was the mechanical obstruction of the pulmonary arteries, and that in both instances the concretions in the pulmonary arteries had, as appeared from their anatomical structure, been formed before death.

CASE XVII.—The patient,[1] a lady, æt. 34, was confined of her second child on the 18th of August, 1851. Her labor was easy and natural, and she made a tolerably good recovery up to the 30th; by this time moving about and apparently in good health and spirits. On that day, however, while dressing, she fell on the bed; the nurse observed frothiness at the mouth and slight convulsion of the face. She spoke feebly once, then laid herself back and died; the whole circumstances occupying but a few seconds.

At the post-mortem examination, the muscular structure of the heart was found pale and thin, especially that of the right ventricle, which contained some dark blood. Each of the pulmonary arteries contained a clot of blood, nearly filling the calibre of the vessels. The chief clots were about an inch and a quarter in length, mottled and firm, and in some instances slightly adherent to the side of the vessel. In tracing the divisions of the pulmonary artery, numerous other clots were found of the same character as the larger ones, and they extended even into the smaller ramifications of the vessels.

The following analogous case occurred in the practice of Mr. J. H. Hewer, and I give the account of it in that gentleman's own words.

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[1] This case has already been published by Mr. Havers, in the Medical Times and Gazette for February, 1852, p. 161.

CASE XVIII.—Mrs. —— was safely confined of her third child on July 4, 1853, and continued remarkably well until eight days after that time. On the afternoon of the eighth day, at two o'clock, she was seen by her medical attendant, who found her very well, and in excellent spirits. A little before five o'clock she complained of a slight pain in one of her hips, which she attributed to having remained rather long in one position. She obtained relief from sitting up, and then used some little exertion in passing urine. As she was returning to the recumbent posture, the nurse observed her face become suddenly pale, and she turned her head round with a most painful expression of countenance, and said, "Oh, nurse, I am dying!" The nearest medical man was summoned, who arrived in ten minutes after the first signs of danger. He found her pulseless, very pale, and breathing with difficulty. She was quite sensible, and in this state she remained to the time of her death, which took place in about thirty-five minutes from the commencement of the attack.

She had always enjoyed excellent health, with the exception of slight derangements of digestion, which were referred to a sluggish liver; she was rather stout, and a very hearty person.

In the examination, 24 hours after death, nothing unusual was observed about the exterior of the body. All the internal organs were healthy, except that the liver was rather large, and a few small gall-stones were present in the gall-bladder. The heart appeared healthy, although its muscular tissue was thought rather pale. The pulmonary arteries were distended with firm, hard, cylindrical coagula. No other vessels contained clots. The vena cava inferior was empty and collapsed. The pulmonary arteries were healthy in structure; their coats smooth and without earthy deposit. The clots in them were firm, laminated like those in an aneurismal sac, but not closely adherent to their walls. The external layers appeared most firm; the interior being darker, softer, and apparently of a more recent date.

It was ascertained, after the examination, that before the patient's confinement, her left thigh and leg had been greatly swollen. This swelling had gradually subsided after her delivery, and at death there was no apparent difference of size in her lower extremities.

The occasional occurrence of sudden and unexpected death in the puerperal mother, is a subject which has engaged the attention and pen of various obstetrical pathologists, as Drs. Denman, Chevalier, M'Clintock, Robert, and others. Different causes lead to it in different cases. Sometimes it is apparently the result of the entrance of air into the circulating system through the open orifices of the uterine sinuses, as in the cases described in Vol. I, p. 719, and in instances reported by Bessems, Wintrich, Berry, &c. I have seen one case where hepatic abscess ruptured into the peritoneum by the efforts of labor, and speedily destroyed the patient; and another where abdominal bleeding from peritoneal fissures on the uterus rapidly led to the same result. Diseased heart, especially disease of the mitral valve, with ventricular dilatation, sometimes seems to produce sudden death after delivery, especially upon moving (see Cases, Vol. I, p. 722); and simple "shock," "syncope," and "idiopathic asphyxia," have all been severally brought forward as possible or probable explanations of this appalling occurrence. All such cases and causes, however, evidently require to be reinvestigated, with a view of ascertaining whether the more palpable condition illustrated by the preceding instances, is not in truth the origin of many examples of this frightful complication, viz., obstruction of the pulmonary artery, or its larger branches.

It requires, however, to be noted, that obstruction of the pulmonary arteries sometimes proceeds to a very great extent, without producing any necessary tendency to sudden dissolution, particularly if the obstructing concretions have gradually accumulated, and only successively invaded one division of the pulmonary vessels after another, as in Cases XI, XII, &c. Under these circumstances there may be set up, as Mr. Paget has shown, a mutual adjustment between the decreased quantity, but increased velocity of the blood sent through the partially occluded pulmonary circulation, and the diminished volume and rate of the circulating fluid in the systemic circulation. Every new amount of occlusion, however, adds to the danger of the patient; and when at last the obstruction has become very extensive, death in some cases supervenes suddenly, under muscular exertion or movement, but less perhaps from true asphyxia than from true anæmia of the left side of the heart and brain, or want of blood in the systemic circulation; while in other instances, dissolution very early and rapidly ensues from the obstruction in the pulmonary artery or its larger branches being at once so great as to produce complete and fatal arrest to the flow of blood through the pulmonary artery or its larger subdivisions, a result which is probably also sometimes produced by large obstructing coagula forming in and filling up the right cavities of the heart itself.

In 1849, Dr. Meigs published, in the March number of the Philadelphia Medical Examiner, an ingenious and suggestive paper on sudden death, after delivery, from "heart-clot." In this communication, Dr. Meigs argues that, white fibrinous coagula of blood or polypi are occasionally apt to form in the right auricle and ventricle, equence merely of great hemorrhages, attended or compli- rith syncope (states, both of which predispose to sanguineous ation), and that these coagula or polypi may afterwards aug- n size around an original small nucleus, and at last fatally :t the flow of blood through the pulmonic side of the heart. ly gives full details of one case, and in it the coagulated stretched from the distended and obstructed right auricle and le into the pulmonary artery and its principal branches.

: XIX.—A lady, after the birth of her fifth child, had post- hemorrhage to the extent of about thirty ounces. On visit- r next day, her medical attendant found her quite well in espect, and the pulse at 75. She was taken out of her re- nt position and placed upright in bed, when she became ly ill; and only three hours after his morning visit, her an was again summoned to see her in a most distressed and ntly dying state, with the pulse at 164, very feeble and thread; he hands cold, and the respiration violent and irregular. tation of the heart disclosed a feeble impulse, with great arity of the systolic action. There was thoracic pain or on respiration; but no pain or anything abnormal in the on of the abdomen. She continued to survive for forty-eight nore, suffering during that time inexpressible respiratory dis- On post-mortem examination, the right auricle and ventricle, auriculo-ventricular opening were found—to use Dr. Meigs's 'ords—"completely tamponed with a clot, which consisted ntly of a firm, whitish-yellow mass of fibrine, out of which particle of hæmato-globulin had been washed away or ex- l." The pulmonary artery, he tells elsewhere, was in this so obstructed or "tamponed with a cylindrical clot, extending inches along the vessel and its principal branches."[1]

SES OF ARTERIAL OBSTRUCTION AND INFLAMMATION IN THE PUERPERAL STATE.

ere happen to exist on the cardiac valves old pediculated tions, capable of being readily separated, an increased force current of the circulation may at any time detach them. citement of the circulation occurring during the exertions of ition, might, *a priori*, be supposed to be calculated to accom- heir separation. But in only one of the preceding cases VII), did the arterial obstruction occur during or shortly after and in that instance the obstructing agent was probably not hed cardiac vegetation.

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[1] Loc. cit. p. 145.

The liability to arterial obstruction and inflammation in the puerperal state must be, therefore, traced—if traceable at all—to other conditions. These conditions will probably be found in certain peculiarities in the blood of the puerperal female, or in certain morbid states which these peculiarities predispose to and produce.

The occurrence of endocarditis, and consequently of endocarditic vegetations, concretions, and coagula, is observed principally in connection with morbid states of the system in which there exists a vitiated condition of the blood. The two pathological states, however, with which endocarditic as well as pericarditic lesions are found most frequently combined,[1] are—1, acute rheumatism; and 2, the more chronic forms of Bright's disease—two diseases which all modern medical authorities acknowledge to be complicated with, and attended by, if they do not actually consist of, certain morbid states or alterations of the constitution of the circulating fluid. In these two diseases, acute rheumatism and chronic albuminuria, the morbid state of the blood is no doubt specifically different, but, at the same time, in several respects analogous; and hence the analogous tendency in both affections to the occurrence of endocarditis and other internal inflammations. In both of these affections, under certain conditions and stages, the blood, as taught us by chemical pathologists, contains special irritating principles in the form of morbid matters (urea and probably lactic acid) accumulated within the circulating system from want of due depuration or excretion. In chronic albuminuria, as in acute rheumatism, there is generally, also, according to Simon and others, an increase of serum and a diminution of blood-globules.[2] Further, in acute rheumatism the quantity of fibrine is usually much increased; and the same is true of acute albuminuria, but not of the chronic forms of the disease, except when some of the inflammations liable to be produced by the disease—such as endocarditis, pleuritis, &c., spring up, when the quantity of fibrine rapidly rises, and a temporary state of hyperfibrinosis or excess of fibrine is engendered.

Among the list of cases of arterial occlusion and inflammation in on-puerperal state which have been recorded by Virchow, , Paget, &c., several had been suffering previously, or at the f the arterial obstruction, from some form of febrile disturb- and hence of blood disease; most were the subjects either of atic complications or of chronic renal disease. Thus in two f Ruhle's three cases of obstruction of the cerebral arteries, rheumatism existed. In three out of five cases of obstruction pulmonary artery by old clots observed by Mr. Paget, the ts had Bright's disease of the kidney.

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[1] In an admirable paper on "the Causes of Pericarditis," etc., published by the late Professor Taylor in the London Medico-Chirurgical Transactions, vol. xxviii, that accomplished pathologist shows, from the analysis of an elaborate series of cases and data, that acute *Endocarditis* is principally seen in connection with acute rheumatism and chronic albuminuria, and occurs in about 8 out of every 100 cases of acute rheumatism, and also in about 8 out of every 100 cases of chronic Bright's disease (p. 559); whilst in the subjects of these two diseases, evidence of recent as well as *old* endocarditis (as indicated by valvular lesions) is found in nearly a half of all those affected (p. 560.)

[2] See the analyses by Drs. Andral, Gavarret, Christison, &c., in Simon's Animal Chemistry, vol. i, pp. 273, 323, &c.

if such blood diseases as rheumatism and albuminuria give lency in the non-puerperal condition to endocarditis and to l concretions and inflammation, we cannot wonder that simi- terial lesions should occasionally occur in the puerperal female the constitution of her blood is temporarily changed even normal circumstances at the time of delivery, and often be- much and morbidly changed under the abnormal circum- s to which she is then liable to be exposed, so that its cha- s are more or less assimilated to the characters of the same n the subjects of acute rheumatism and chronic albuminuria. analyses and investigations of Becquerel, Regnauld, Kiwisch, ux, and others, have shown that during the last weeks of ancy there is, as in acute rheumatism and chronic albumi- a diminution of the red globules of the blood, an increase of , and a marked state of hyperfibrinous or excess of fibrine. ding to the researches of Gavarret and Delafond, this redun- of fibrine not only appears to remain after parturition, but even for a time to increase—giving, doubtlessly, among other , a predisposition to obstruction and inflammation of the con- g vessels, whether arteries or veins. Besides, the constitution blood in the puerperal female is altered in other ways. The ion and accumulation of matters destined for excretion—as nd lactic acid—are supposed to give that tendency to internal mations, which is possessed by rheumatic and albuminurious , these retained excretory matters probably producing the in- ations in question, by being—by a kind of perverted secre- -thrown out upon surfaces, or into tissues, which are directly d into inflammation by their presence as stimulating and irri- matters. During the puerperal condition, the blood is more d with new materials, intended, some for excretion, and some cretion, than at any other term of life; and hence is specially to diseased changes under the superaddition of any exciting tic causes. For the uterus, during the first weeks after de- , is becoming involved and absorbed by a kind of retrograde orphosis, and the effete materials resulting from its disintegration necessarily first pass into the blood before they are ultimately discharged and depurated from the system; there is an excretory action going on in its interior in the form of the lochial discharge; and the elements for the formation of a new and important secretion—the milk—are present in the circulation.

But under various accidental causes, or by the supervention of fever, under exposure to cold, from indiscretion in diet, etc., the normal discharge of these natural puerperal excretions and secretions is not unfrequently impeded or arrested; and the blood becomes morbidly altered and diseased from their retention or non-elimination. Besides, morbid matters sometimes pass into the circulation in the puerperal state through the uterine veins, etc., more particularly pus or some of the elements of pus, and afterwards are carried round with the circulating mass. Some of these new or non-eliminated materials appear occasionally to have a direct tendency to produce coagulation or consolidation in the superfibrinated and diseased blood, as is seen experimentally when particular varieties of pus are mixed with it; while others of them have a greater or less tendency to irritate, and produce changes upon the lining membrane of the vascular system, ending in obstruction and inflammation of the veins (phlegmasia dolens); or in obstruction or inflammation of the arteries; or, as in Cases I, II, etc., in simultaneous or successive obstruction and inflammation of both individual arteries and individual veins in the same patient. If the cardiac valves or endocardial membrane have been in previous periods of life, as in some of the other cases detailed in Section I, the seat of inflammation and consequent vegetations, such *diseased* parts and excrescences will, in accordance with an acknowledged pathological law, be more liable than any other more healthy and normal part of the interior of the circulating system to take on a renewed inflammation under these renewed inflammatory conditions; and when these parts, and the pedicles by which the vegetations are attached to them, are attacked and rendered friable by inflammatory action, the common force of the blood-current may be sufficient at last to detach and separate one of them, or a succession of them. There seems some ground also for believing that, occasionally in a protracted puerperal convalescence, as in Cases II, and V, the endocarditis and resulting endocarditic vegetations may both form and separate after delivery, the detached vegetations being recent, and not of old standing; and perhaps when obstetricians have studied more accurately the liability of the puerperal female to endocarditis and pericarditis, they may come to acknowledge with Testa, one or more forms of puerperal carditic inflammation.[1]

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[1] See his work, Della Malattie del Cuore, tom. iii, p. 178, Capo ix, "Delle Pericarditi et delle Idropericarditi Puerperali."

SYMPTOMS OF PUERPERAL ARTERIAL OBSTRUCTION.

I. IN THE ARTERIES OF INTERNAL ORGANS.

symptoms of arterial obstruction necessarily vary with the obstructed, or rather, to speak more correctly, they vary with ıction of that part to which the artery belongs, and hence are ɔr less severe and serious, according as the function of the d part is important or not in the economy. The results in ıy of arterial obstruction are very different, according as the ed artery belong to organs connected with the head, chest, or en, or is an artery belonging to one of the extremities of the

immediate symptoms of arterial obstruction are occasion- odified also by the slowness or suddenness with which the ction is produced, and by the fact whether the stoppage artery or arteries perfectly or imperfectly shuts off the flow od to the parts beyond, and thus perfectly, or only imper- interferes with and paralyses the function of that part. when a cardiac vegetation, displaced from the heart, is ıly carried upwards, as happened in Case IV, and in other :es, in the middle cerebral artery, the function of the portion n supplied by that artery seems, in general, at once suddenly ›ed or totally arrested—the part supplied by the middle al artery having little or no anastomoses with the neighbor- rebral branches. In the instance in question—Case IV— ʋas sudden loss of innervation, or hemiplegia of the opposite f the body, followed by the symptoms of ramollissement or ɪne of the implicated portion of brain; and the nature of the ; and the lesion were correctly diagnosed by Dr. Burrows on ,t visit to his patient, from his finding, 1st, sudden hemiplegia ening in a comparatively young patient, who presented at the :ime, 2dly, auscultatory signs of cardiac valvular disease or ırditis.

history of other cases of obstructed cerebral arteries shows, er, that the resulting paralysis and other nervous symptoms ›metimes come on more slowly, especially when the arterial ction from an impacted concretion is not complete at first, ıly becomes completed from coagula collecting round this tion as a nucleus; or when the obstruction occurs more from disease of the coats of the implicated vessels them-

possible that the complete occlusion of a single artery by a tion or concretion thrown into it, may destroy entirely the function of an individual organ, when that organ depends for its function on a single artery. In his large work on Diseases of the Heart, Testa dedicates a chapter to blindness as a complication sometimes seen in cardiac diseases.[1] Corvisart describes a case of sudden dissolution of the right eye, without any preceding or accompanying inflammation, or any appearance such as we see in phlebitic ophthalmia, occurring in a patient suffering under cardiac inflammation. "The most careful examination," observes Dr. Stokes, when speaking of this case, "failed to detect any cerebral disease, and the sudden dissolution of the eye remains an unexplained fact."[2] The fact, however, appears not inexplicable upon the idea that a small fibrinous concretion or plug had passed upward out of the cavity of the inflamed heart, and lodged in and occluded the ophthalmic artery. Of course, the truth of such an explanation can only be ascertained by examining the state of the artery in any similar case. Several years ago I saw the destruction of an eye in a puerperal patient, which I have sometimes thought might possibly belong to this form of disease.

CASE XX.—A patient, six weeks after delivery, walked some distance to my house to show me her right eye, which was quite blind and opaque, but the case differed from Corvisart's in this respect, that the eyeball was distended with purulent matter. A fortnight subsequent to her accouchement, on making some unusual muscular exertion, pain suddenly came on in this eye, which had been weakened during an attack of measles in childhood. Inflammation speedily followed. When I saw her, there was no constitutional disturbance, such as we see in cases of puerperal phlebitic ophthalmia—a disease that is almost always, if not always, fatal. I have lately seen this patient with the eye quite collapsed and atrophied. She has an irregular pulse, and other symptoms of old disease of the heart.

Of the symptoms of obstruction in the arteries of the abdomen and chest we know in pathology, as yet, little or nothing.

The cases which I have described under Section V of obstruction of the pulmonary artery show that the patients upon whom this obstruction is found after death, suffer during life under symptoms indicating distressing disturbance in the actions of the heart and lungs. The first symptoms in some appear to have been sudden pain or angina in the region of the heart, with faintness, palpitation, and afterwards great increase in the rapidity of the pulse. The respiratory symptoms have mostly consisted of labored, anxious,

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[1] Loc. cit. (tomo ii. capo ix. p. 132.)

[2] Stokes on Diseases of the Heart and Aorta, p. 59.

d irregular breathing, particularly on the least movement; with
metimes the addition of cough and frothy or bloody expectora-
on. There occasionally breaks out a cold perspiration on the sur-
ce, and there is often a marked and deadly coldness of the hands
other extreme parts of the body, with congestion of the face.
one or two cases, temporary unconsciousness, or delirium, or a
nvulsive fit, has been observed; and in others, great nausea and
miting have formed early symptoms. If such symptoms of car-
ac and pulmonary suffering suddenly supervene in a puerperal
tient, especially after change of position or exertion, and more
rticularly when recovering from any form of phlebitis, we might
ve reason to suspect obstruction, to a greater or less extent, of the
rculation in the pulmonary arteries. Hitherto auscultation has
t enabled pathologists to make out any special signs indicative
the existence of obstruction in the pulmonary artery; and, in-
ed, the sudden supervention and presence of such severe cardiac
pulmonic disturbance *without* any corresponding stethoscopic
gns, forms in itself, perhaps, one of the best tests of the lesions
nsisting in some occlusion in the pulmonary vessels. These
mptoms, in some instances, have, as I have already stated, been
bserved to come on slowly, in all probability from the obstruction
the pulmonary arteries only gradually increasing and accumula-
ng. When the occlusion at last reaches the larger pulmonary
rteries, or shuts up their canals from the first, death, as we have
reviously seen, comes on with unusual rapidity and suddenness.
See Cases XIII., XIV., XV., &c.)

Gangrene of the lung, with its characteristic fœtor, sometimes
omes on in the puerperal, as in other states, without any adequate
mount or intensity of preceding inflammation, and sometimes, in-
eed, the "inflammation," to quote the words of Laennec, "sur-
ounding the gangrenous spot seems to be rather the effect than the
ause of the sphacelus." Is the gangrene in such instances not
ometimes an effect, and hence also a symptom, of the obstruction
f the pulmonary, or rather of the bronchial arterial branch supply-
ng the sphacelating part, and is not pulmonary gangrene, like the
angrene of the extremities, frequently the consequence of arterial
bstruction?

Perhaps some instances of impeded and arrested function, gan-
rene, &c., in the abdominal viscera may yet be found to admit of a
milar explanation. Dr. Abercrombie pointed out a remarkable
ries of fatal cases of ileus in which apparently there existed merely
ralysis of a limited portion of the intestinal canal, in consequence
which loss of muscular power, that portion did not act in concert
ith the other parts of the tube, and the non-transmission of the

intestinal contents was the result. In these cases of paralytic ileus, there was usually violent pain during life; and on dissection the paralysed portion of bowel was found in some instances white and discolored, in others livid, and in one instance gangrenous, but without any appearances of inflammatory exudation.[1] In the remarks which I have to make immediately upon the symptoms of arterial obstruction as seen in the extremities of the body, we shall find that in the limbs, arterial plugging and occlusion give rise to these same various effects, paralysis, pain, and occasionally gangrene. If arterial obstruction produces similar effects internally to those which it thus produces externally, would not the plugging of a mesenteric artery, or of some of its branches, give rise to the symptoms and consequences seen in the paralytic form of ileus described by Dr. Abercrombie? The whole subject, however, of the pathological signs and results of the obstruction of the arteries supplying the internal organs of the body, is as yet so unknown and uninvestigated, that speculating on the matter is mere hypothesis.

II.—SYMPTOMS OR EFFECTS OF OBSTRUCTIONS SEATED IN THE ARTERIES OF THE EXTREMITIES.

The symptoms, however, of arterial obstruction have hitherto been principally observed and studied as they are met with in occlusion of the arteries of the extremities of the body. These obstructions are generally preceded in the puerperal female by more or less febrile and constitutional symptoms, and occur especially in patients who are suffering or have suffered from rheumatic or other diseases of the heart or cardiac valves. When arterial obstruction at length takes place in one or more of the vessels of a limb in patients presenting these predisposed states, the following local symptoms are liable to occur in the obstructed limb, viz.—1st, Arrest of the pulse below the site of obstruction; 2d, Increased force of pulsation in the artery above this site; 3d, Change in the temperature of the affected limb; 4th, Paralysis, or neuralgia in it; 5th, Gangrene. Let me speak briefly of some of these symptoms of puerperal arterial obstruction and inflammation in succession.

1. *Arrest of the pulse below the site of obstruction.*—The arrest of the pulse from a point in the course of the artery of a limb downwards may supervene suddenly, as when the arterial obstruction is produced by a vegetation or coagulum carried from a distance and impacted in the canal of the vessel; or the pulsation may cease gradually, as when the obstruction is produced more slowly by the presence of a local arteritis. In practice, physicians are seldom in

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[1] Abercrombie on Diseases of the Stomach, &c. 2d edit. pp. 143–144. Cases 25, 26, 27, 28, &c.

he habit of examining any artery, except the radial; and hence robably the cause why arterial obstruction has been more frequently ound in the arm than in other parts during life. In any doubtful ase, especially where there are any other symptoms of obstruction present in the lower extremities, as neuralgia or partial paralysis, it s our duty to examine the course of the arteries in that extremity, in order to ascertain whether at any point the pulse is obstructed in them. It is necessary also to remember, in relation to this symptom, that after a time, the pulse may return in an artery previously obstructed, as happened in Cases I., &c.

2. *Increased force of pulsation in the artery above the site of obstruction.*—Though absent in some instances, this symptom has been well marked in a few others. Thus, in a very important case described by Mr. Tuffnell,[1] and in which the popliteal artery was occluded by cardiac vegetations impacted in the canal of the vessel, there was such strong pulsatile action in the femoral artery above the site of obstruction, as at first to suggest the idea of the possibility of aneurism, but there was no aneurismal bruit de soufflet present. This symptom may even serve us in the diagnosis of the obstruction of some internal arteries. In a case of puerperal hemiplegia, which I saw with Dr. Alexander, this symptom, with others, led me to suspect the probable occurrence of impaction of a cardiac vegetation or concretion in one of the cerebral arteries.

CASE XXI.—The patient was the subject of old-standing disease of the mitral valve, and considerable hypertrophy of the heart. About the eighth month of her pregnancy she returned home from a long walk scarcely able to articulate, and with commencing symptoms of hemiplegia in the right side. There was no albuminuria. During the course of the next twenty-four hours these paralytic symptoms increased in intensity, and the arm and leg became gradually more powerless, so that the leg at last did not answer to the reflex irritation of tickling the sole; the hand was spasmodically shut. At the same time the patient evidently retained completely her consciousness and mental faculties, though quite unable to give utterance to her wishes and feelings. There was no tendency whatever to stupor, and no febrile reaction. On feeling simultaneously with the fingers of the two hands, the right and left carotids, the amount of pulsation in the artery on the left side was found by Dr. Alexander and myself greatly stronger and more marked than the pulsation of the corresponding vessel on the other side of the neck. The patient went on in much the same state to the full time, and was safely delivered. The hemiplegia has decreased since that period, but still—now three years since the attack—the power of articulation remains much impaired, and the right leg and arm are feebler than the left.

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[1] Dublin Quarterly Journal of Medical Science, vol. xv. p. 371.

If the hemiplegia in this instance had been the result of sanguineous effusion into, or other lesion of, the left hemisphere of the brain, and not the result of arterial obstruction in one of the arteries supplying that hemisphere, would there have been this extraordinary impulse in the left carotid artery? I am not aware that pathology yet possesses facts and observations to answer the question definitely; but reasoning *a priori*, one would expect it to be answered in the negative. And if this case be an instance of hemiplegia from arterial obstruction, it is invested with this additional interest, that it affords us evidence of the possibility of partial recovery at least, from this variety of encephalic lesion.

3. *Fall in the Temperature of the Limb.*—When the artery or arteries are obstructed in a limb, a fall in the temperature of the extreme parts of the limb usually soon follows. A morbid feeling of heat is sometimes complained of by the patient in the first instance; but the application of the hand, and still more of the thermometer, certifies, in general, an actual and sometimes a considerable fall. The extent of this diminution of temperature is regulated by the extent and completeness of the obstruction to the supply of arterial blood in the limb.

4. *Lesions of the Motory and Sensory Powers in obstruction of the Arteries of the Limb*—PARALYSIS—NEURALGIA, &c.—The functions of sensation and motion in limbs, in which the arteries have become obliterated, are very differently affected in different instances. These differences depend upon the degree of arterial obstruction that is present, and upon some collateral circumstances. The amount of blood sent to the obstructed part may be sufficient for maintaining the mere purposes of its vegetative or nutritive life, but not sufficient for maintaining in it, in their normal state, the higher functions of sensation and motion pertaining to its nerves, or the irritability of its muscles. The production of paralysis in the two posterior extremities in animals, by ligature of the aorta, and in individual limbs, by tying and obstructing the principal arteries of these limbs, has been proved experimentally on the lower animals by Stenson, Arnemann, Englehart, Segalas, and other physiologists; and similar results are observed in operative surgery on the human subject—the power of motion being wholly or partially lost till the collateral circulation is developed. Treviranus and Müller infer that the resulting paralysis of muscular motion in the obstructed limbs is referable in these experiments and operations to the muscular fibre being deprived of its requisite constant afflux of arterial

blood. In some of these experiments and cases, however, there is evidence of the arrested flow of blood influencing directly the functions of the nerves of the limbs, as a state of cutaneous anæsthesia is observed along with the paralysis. In many cases of arterial obstruction, another effect, more difficult of explanation, presents itself, namely, pain or neuralgia in the implicated limb, a result sometimes temporarily seen, though seldom to a very marked degree, after the ligature and obstruction of vessels for aneurism. In practice we find very various degrees and combinations of these motory and sensory lesions in extremities whose arteries are the seat of obstruction. Thus—

First, When the arterial obstruction is sudden and complete, the limb may at once be found perfectly paralysed, both in relation to motion and sensation, as happened in the famous case in which Sir Astley Cooper first threw a ligature around the aorta. In a case of arterial paralysis of the leg, mentioned by Cruveilhier, the limb was from the first in a state of death, or, as he terms it, "cadaverization," and totally without feeling or movement.

Local arterial paralysis, let me here add, differs in one or two important diagnostic points from cerebral or nervous paralysis. In the arterial paralysis there is a want of those head symptoms that generally precede or accompany all paralyses that arise from lesions of the central organs of the nervous system. The temperature of the limb is commonly diminished in arterial paralysis; but not, at least in the first instance, in cerebral or nervous paralysis. In the former variety of paralysis, there is an arrest of pulsation in the arteries of the affected limb; but the arterial pulsations are not influenced or changed in the latter variety of the disease. In arterial paralysis, gangrene sometimes speedily supervenes; in nervous or cerebral paralysis, this result is unknown.

Secondly, In cases in which the arterial obstruction in the extremities is not complete, the accompanying lesions of innervation sometimes only amount to a feeling of numbness, or prickling and stiffness or semi-paralysis in the implicated limb, like that occasionally produced by pressure on the sciatic nerve in sitting. Usually there is pain, and at all events, tenderness on pressure, at the site of obstruction, and more or less along the course of the occluded artery. But—

Thirdly, In connection with arterial obstruction in a limb, there has now been also often observed a feeling of extreme pain and tenderness on pressure, not only at the site of occlusion, and along the course of the obstructed vessel, but more or less throughout the extremity; and this increased supersensibility often amounts to a true and severe arterial neuralgia, that may be either constant or intermittent in its type. Sometimes the pain or neuralgia accompanying arterial obstruction is combined with paralysis of the motory powers of the limb; sometimes, as in a case of arterial obstruction and gangrene in the right leg, recorded by Drs. Graves and Stokes, the ultimate paralysis of motion and sensation in the limb is preceded by pains; and in other instances the arterial neuralgia is excessive, and without any accompanying paralysis having at least been remarked. This pain is occasionally the first, and throughout the most prominent symptom in arterial obstruction. In a case which lately occurred in the practice of my friend Dr. Paterson of Leith, and where at last gangrene attacked the two lower, and one of the upper extremities in a lady of thirty years of age, who was the subject of chronic valvular disease, the first symptom observed consisted in sudden and excruciating pain or neuralgia in the leg first affected, and which wakened her out of sleep in the middle of the night. In any instances in which arterial obstruction is probable, this occurrence of sudden neuralgia is in itself a circumstance to awaken suspicion, and lead to a careful examination of the arteries of the limb. M. Legroux was attending a patient complaining of great pains in the lower extremities. The means of relief which he had employed having totally failed, several physicians were called into consultation. They all believed the pains to be rheumatic. After the consultation was finished, M. Legroux revisited the bedroom of his patient, examined for the first time the arteries of the affected limb, and discovered that both femoral arteries were hard, tender, and without pulsation. After the patient's death, the lower end of the aorta, the iliac and crural arteries were found obstructed by concretions.[1]

The proximate cause of the pain or neuralgia accompanying arterial obstruction in a limb, is a point which, in the present state of our knowledge, it is difficult or impossible to determine. It is not the result of a puerperal neuritis in the affected limb; for, in the right leg of the patient in Case II., the neuralgia was very great during life, but the nerves, on dissection after death, were found quite sound. Nor is it essentially the result of arteritis; for the pain has often developed itself before any arterial inflammation could be established, as immediately upon the artery being obstructed by a vegetation or clot; or, as has been seen in surgery, sometimes instantly after perchloride of iron, &c., was injected, and had produced coagulation in the blood in the vessel. Perhaps the proximate cause of the attendant pain may yet be traced to pressure in some form; or, to morbid over-distension of the arterial tube;

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[1] Cruveilhier's Traité d'Anatomie Pathologique, tom. ii. p. 299.

r, possibly, in some instances, to a spasmodic contraction of the alls of the vessel upon its contents.

5. *Gangrene below or beyond the Seat of Arterial Obstruction.*—In everal of the cases which we have previously detailed of puerperal rterial obstruction and inflammation, gangrene of the parts beyond he seat of obstruction followed to a greater or less extent. The erebral ramollissement and disintegration observed in Case V., in the parts of the brain supplied by the obstructed middle cerebral rtery, is in all probability of this pathological nature. The parts of the brain supplied by that artery are, as we have already stated, the more likely to become dead and gangrenous, in consequence of its occlusion, seeing there is little or no anastomosis between its branches and those of the other cerebral vessels. The occlusion of an artery going to some part may interfere with the function of these parts without leading to gangrene. We can easily suppose, for instance, the artery of the retina occluded, with the destruction of the function of vision, without the eyeball or its appendages running any risk of destruction or gangrene, because these parts are nourished by other vessels. When the chief artery or arteries of one of the extremities is obstructed, gangrene frequently comes on, not as a primary, but as an ultimate symptom and result; and its supervention appears to be regulated by various pathological circumstances. It is seldom that the local obstruction of the leading artery alone of a limb leads to gangrene in the extreme parts of that limb; the collateral circulation, if left free, being usually sufficient to prevent any mortification. In fact, the obliteration of the main artery of a limb seldom, apparently, produces gangrene, unless it be combined and coexist with conditions that more effectually prevent the circulation in the extremity; such as the simultaneous obstruction of the usual channels of the collateral circulation above, or of the smaller arteries of the limb below; or the simultaneous obstruction of the principal vein or veins, as in the left limb of Case II.

Gangrene, in one or other of the extremities of a puerperal female, is the only sign by which we know that arterial obstruction has in all probability occurred, in instances where there has been no opportunity of making an accurate post-mortem examination. I have notes of two or three unpublished cases of gangrene of the extremities in puerperal women, which I may here quote both as additional evidence of the frequency of arterial obstruction, and of his additional complication. The three following cases are all, esides, remarkable for the rapidity with which the mortification supervened. For notes of the two first I am indebted to Dr. Cowan; the third was seen by Dr. Reid of Kirkcaldy.

CASE XXII.—Mrs. G——, æt. 36, healthy and active, was delivered of her fourth child some time in the spring of 1826. She was visited regularly for three days, and during this time no bad symptoms were observed; and the lochial and lacteal secretions were natural. Dr. Cowan was sent for early in the morning on the fourth day, and found that his patient had suffered from a severe rigor in the night. The countenance was anxious and distressed, the face pale, the eyes sunk and languid, and she was screaming from excruciating pain in the left leg and foot, but referred principally to the upper and inner portion of the calf, which was found cold and tense, but not increased in size. This state extended to the foot, on the forepart of which a large spot of ecchymosis appeared most evident over the metatarsus, and creeping upwards to the ankle joint. The uterus was not perceptibly larger than usual, but there was some tenderness in the left hypogastrium on pressure, as also in the lumbar region. The pulse was small and rapid, the thirst urgent, and the tongue dry and coated. There was some nausea but no vomiting. The lochia had ceased and the milk was scanty. On returning in the evening, Dr. Cowan found that the pain was mitigated, but her appearance was worse in every respect. The pulse was now very feeble, the tongue parched and of a fiery red hue; her mind was sluggish and wandering; the discoloration had reached the rise of the calf of the leg (having a wavy margin), and still advancing upward with the same cold, tense, unyielding character, but with little increase of bulk. The thigh seemed natural but colder than the other. The bowels had been moved with oil, but little or no urine was secreted. At seven o'clock the following morning everything was found worse, and in addition vesication had begun on the spot first discolored; in short the foot was hopelessly gangrenous and the patient sinking fast. Death put an end to her sufferings early on the fourth day of disease, and the eighth from delivery. No decided margin of arrest in the livid or gangrenous limb could be observed, but the foot showed marks of decomposition, and the smell was characteristic both before and after death. The post-mortem examination was refused.

CASE XXIII.—A patient, aged 25 years, ten days after delivery with her first child, was seized with gangrene of one of the lower extremities. Dr. Cowan saw nothing of the case until he entered the room at the time of the operation for the removal of the limb. He had no opportunity of making an examination of the state of ulation in the heart and arteries. Above the site of the gan- ne the thigh seemed paler than natural, but little altered in size. e gangrene involved the foot and leg nearly to the knee joint; patient was greatly exhausted and anxious, but not suffering erely. The limb was amputated at the lower third of the thigh, t not a drop of blood followed the knife. The patient died next y, sinking gradually without suffering. An autopsy was refused, t the femoral artery from the groin to its extremities in the und were allowed to be examined. The artery was pervious and pty, except some soft clots, adhering slightly to its inner wall, ich, when scraped off, showed the inner coat of a bright roseate lor, gradually fading in its descent. The veins were not ob- ucted.

CASE XXIV.—This patient had previously borne a large family. er last labor was easy, for it came on prematurely, and the child s dead born. On the third or fourth day subsequent to delivery, er supervened, followed by swelling of the left leg and thigh, hich was attended with great pain and suffering. In the course two or three days gangrene showed itself, and when Dr. Reid s called in, the whole extremity was already dark-colored, and e mortification extensive. She died ten days after delivery. There s no dissection.

After gangrene in an extremity occurs in a puerperal mother, th has almost always followed, whether the disease was left to e effects of nature, or amputation had recourse to, as in Case XIII. An instance of puerperal gangrene in one of the legs was blished in the Lancet for 1845, by Mr. Bottomley of Croydon, d is interesting from the circumstance that the patient recovered ter amputation of the mortified parts. Mr. Bottomley writes me, at he has no doubt that the gangrene originated in arteritis, or terial obstruction. The history of the case is as follows:

CASE XXV.—The patient, æt. 39, was confined on the 8th of Feb- ary, 1845. Her pregnancy was favorable until within a month of elivery, when she suffered from cough and great debility. The bor, however, was expeditious; the lochia were small in quantity; d the after-pains slight. The woman continued doing well for n days, when symptoms of pleuritis, with considerable constitu- onal disturbance, presented themselves. These symptoms dis- peared under the treatment employed. Two or three days after r recovery from the pleuritic attack, she complained of pain in e heel, passing from thence to the great toe and ankle-joint. No normal appearance, however, could be found when these parts were examined. The pain was treated by Mr. Bottomley as neuralgic by veratrian ointment, morphia, &c., but with no good result. At length a livid spot appeared on one of the toes, the temperature of the foot and leg became gradually diminished, and there was impaired sensibility. This diminished temperature and sensibility continued to increase. The toes severally became black, and this appearance extended so as to involve the foot and ankle. At length a line of demarcation was formed about two inches above the ankle-joint, and amputation was thought of, but deferred until the patient's health should be somewhat improved. On the 3d of May, the leg was amputated above the knee. It was deemed advisable to operate as high up as this; for although the line of demarcation was just above the ankle-joint, and the skin had its normal color higher than this, still there was much swelling, some hardness and pain in the calf, and the cause of the gangrene was believed to be a constitutional one.

When the tourniquet was applied, it was noticed that the pulsation in the femoral artery was hardly perceptible. After the removal of the limb, both the femoral artery and vein were found plugged with a transparent semicartilaginous substance.

The recovery of this patient was complete, and, as I am lately informed by Mr. Bottomley, she has borne two children since the occurrence of this attack of puerperal gangrene.

The preceding contribution gives a view of puerperal arterial obstruction and inflammation, that is in many respects altogether unfinished and imperfect. Of its many defects no one can be more deeply and sincerely aware than I myself am. But imperfect and defective as the sketch is, it will answer the principal purpose for which it is intended, provided only it be fortunate enough to direct the attention of my obstetrical brethren to the subject, and thus initiate a full investigation into this new form of puerperal disease.

TETANUS FOLLOWING LESIONS OF THE UTERUS, ABORTION, AND PARTURITION.

(From Edinburgh Monthly Journal of Medical Science, Feb. 1854, p. 97.)

DR. ROBERT REID, in his work "On the Nature and Treatment of etanus, &c.," states that "injuries or derangements of *internal gans* do not appear to have much tendency to produce this affec- on. Indeed," he adds, "I have not been able as yet to find any se recorded of true tetanus which arose from *internal injury*, hether in the alimentary canal or elsewhere."[1]

The uterus is certainly an "internal organ," in the sense in which at expression is used by Dr. Reid; and internal injuries or lesions f it, both in the unimpregnated and puerperal states, are, without oubt, sometimes followed by tetanus in an acute and fatal form.

This fearful complication is fortunately a rare occurrence in mid- ifery practice. But the following series of cases is calculated to how that traumatic tetanus does supervene occasionally as a secon- ary obstetrical disease, in the same way as all medical authorities cknowledge it to supervene occasionally—and still more frequently -as a secondary surgical disease.

Before, however, detailing the proofs which I wish to adduce of his last remark, let me premise that in ancient times tetanus was sually considered and described, for example, by Aretæus, as a isease more common among females than males. Modern statis- ics, however, have amply shown the reverse to be true.

Out of 128 cases of traumatic tetanus collected by Mr. Curling, 112 were males, and 16 only were females.[2] Out of 221 cases col- ected in an excellent paper by Professor Lawrie of Glasgow, 185 f the patients belonged to the male sex, and 36 to the female sex.[3]

The Registrar-General's Official Reports afford a still greater mount of evidence on the relative liability of the two sexes to etanus. The number of fatal cases of tetanus which have occurred nnually in England and Wales during the last fifteen or sixteen years has varied from about 120 to 150; in 1847 as many as 165 died of this disease. The number of deaths from tetanus, &c., has, unfortunately, not been published for every year of the Reports; in some of the years those occurring in the metropolis are not given; and occasionally the data as to the sexes of the patients cannot be discovered in the official returns. But from 1837 to 1842, I find 629 deaths from tetanus entered in the reports, with the numbers of each sex stated; and from 1847 to 1849, there are 430 deaths from tetanus similarly specified. Out of these 1069 fatal cases of tetanus, 829 belonged to the male sex, and 240 to the female sex. In other words, out of every 100 of these cases, 77 per cent. were males, and 23 per cent. females. Or the males attacked with fatal tetanus were to the females in the proportion of nearly 4 to 1.

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[1] Nature and Treatment of Tetanus, p. 52.

[2] Treatise on Tetanus, Table in Appendix.

[3] Glasgow Medical Journal for October, 1853, p. 352.

In how many of the above 240 instances of fatal tetanus in the female sex, the uterus was the seat of traumatic irritation or injury we have no means of judging. Doubtless, the proportion was but small. The following cases, however, will tend to prove that this dreadful malady may follow—(1) lesions of the unimpregnated uterus; as well as the lesions left in the uterus and maternal canals, (2) by abortion; and (3) by parturition at the full time:—

I.—TETANUS AFTER LESION OF THE UNIMPREGNATED UTERUS.

Lesions and injuries of the unimpregnated uterus seem to have little tendency to produce traumatic tetanus. But the following instance affords a remarkable example of this formidable complication:—

CASE I.—A widow, æt. 35, who had been suffering for many months under severe menorrhagia, came from a considerable distance for advice. The uterus was large and heavy, and evidently contained some roundish morbid mass either in its cavity or in its walls. The use, for a few hours, of a single sponge-tent, opened up the os uteri, and allowed a polypus to be felt in the interior of the uterus. Menstruation supervened in a profuse degree; and any further local treatment was in the meantime suspended. In a few days uterine contractions came on. They were severe in character, and gradually pushed down a large cellular polypus from the uterus into the vagina. No special operation was required to detach it, as the expulsive efforts of the uterus had, in a great measure, spontaneously broken up the adhesions of the tumor with the interior of the uterine cavity; and at last the polypous mass was found so loose as to be easily removed from the vagina by slight traction with the fingers. It proved to be a cellular or loose fibrous polypus, of the size of the closed fist. After the separation of this mass the patient felt comparatively well. She was free from fever and local pain, and appeared for a time quite convalescent. On the ninth evening, however, after the polypus was detached, she sent for me, to complain of some stiff and strange feelings about her face. At the time of my visit she was sitting up, drinking tea. She herself remarked that she was almost unable to swallow it, from pain and difficulty in opening her mouth, which she had first noticed a few hours previously. My suspicions of tetanus were not at the time excited. I believed, with the patient herself, that she was threatened with an attack of cynanche. But early next morning, all the symptoms of the disease were becoming far too marked to leave any doubts of its nature. The muscles of the neck were affected with tetanic spasms, and the jaws fixed. Deglutition was impossible. During the day, Professor Syme saw the patient with me. In spite of all the usual remedies, the tetanic paroxysms increased in extent, in frequency, and in severity. She bore the terrible spasms with wonderful equanimity, and her mind seemed to remain entire up to the very last. She died, exhausted by the disease, about fifty-five hours after the first symptoms of tetanus were observed.

On making a post-mortem examination, no special lesions were found in any organ examined, with the exception of the uterus; and there the principal, or, indeed, only morbid appearance, consisted of the shaggy and projecting attachment of the uterine extremity of the ruptured pedicle of the polypus, marking the spot where it had adhered to the interior of the body of the uterus.

I do not remember of having heard or read of any analogous case in which a lesion of the unimpregnated uterus was followed by traumatic tetanus. Mr. Curling refers to an American case, recorded by Dr. Smart, in which the disease was supposed to have been produced by an injury of the neighbouring mucous canals, viz., "by the passage of large, rough, angular pieces of clay from the intestinal canal into the vagina."[1] The case, however, is not an example exactly in point. A French author of the sixteenth century, Martinus Akakia,[2] states—apparently in the way of a general observation, from one or two individual cases—the occurrence of trismus, general tetanus, and opisthotonos, running on to a fatal termination by the fourth day, as an occasional sequence of menorrhagia, whether that menorrhagia, or hemorrhage, had been the result of diseased states of the unimpregnated uterus, or of abortion, and delivery.

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[1] Treatise on Tetanus, footnote, p. 197.

[2] De Morbis Mulieribus, lib. i. cap. 6, "De fluxu rubro." See Spachius' Gynæciorum Libri, p. 759.

II.—TETANUS AFTER ABORTION.

Several of our oldest medical authorities describe abortion as one of the occasional causes of tetanus. "Individuals," observes Archigenes, "are attacked with this disease in consequence of various causes, as from wounds, and in women from abortion (*in fæminis ex abortu*)."[1] When speaking of the causes of tetanus, Aretæus specially enumerates wounds of all kinds, with exposure to cold, and he adds that "women are sometimes, though rarely, attacked with the disease as a result of abortion."[2]

In none of our modern obstetric books, nor in any of the various essays or works devoted to the consideration of abortion or puerperal diseases, is any allusion made, so far as I know, to the possible supervention of tetanus after miscarriage. It is a complication, however, which does occasionally take place; and it is always one very formidable in its character, and generally very fatal in its issue.

Two deaths from tetanus after abortion occurred some years ago, almost simultaneously, in Edinburgh. I have the kind permission of Dr. Alexander Wood and Dr. Malcolm, in whose practice these cases occurred, to state the following particulars regarding them:—[3]

Case II.—A lady, aged about 36, and the mother of six children, after having had the catamenia obstructed for three months, was attacked, on 16th November, 1845, with symptoms of abortion. On the 17th, the os uteri was open, and large coagula were expelled. In three or four days she was so far recovered as to require no more medical care. On November 23d, the patient suffered under some degree of sore throat and stiffness of the jaws. Early next morning, when Dr. Wood visited her, he found the rigidity of the jaws so considerable, and the difficulty of opening the mouth so great, that it was impossible to get an inspection of the throat. The pulse was 80, soft and compressible. During the day the stiffness of the jaws increased, acute pain of the spine was complained of; the abdominal muscles were rigid; and swallowing became difficult. General tetanic spasms supervened in the evening, and notwithstanding the employment of turpentine, tobacco, Indian hemp, &c., the disease continued to increase, and she died on the evening of the 26th.

Case III.—In this case the tetanic attack was still longer in super-evening after the abortion. Nearly a fortnight after an early miscarriage, and when the patient was considered well, she was suddenly attacked with symptoms apparently of cynanche parotidea. On the second day, Dr. Malcolm found his patient unable to open the jaws. On the third day, the symptoms were all much aggravated; and on the fourth day, she was seized with general tetanic spasms, which recurred again in two hours with increased violence, and then proved fatal in the course of a few minutes. Dr. J. Duncan, and one or two other medical gentlemen saw this lady in the first of these general tetanic paroxysms, during Dr. Malcolm's unavoidable absence at an obstetric case.

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[1] See Aetius' Tetrabibl. ii. sec. II. cap. 39.

[2] De Causis Acutorum Morborum, lib. i. cap. 6; vol. i. p. 2, of the Latin edition in the "Medicæ Artis Principes."

[3] Notes of these two cases have already appeared in the published Proceedings of the Obstetrical Society. Edinburgh Monthly Journal, April, 1850, p. 387.

In the following instance the ovum was at least partially separated, as shown by the attendant hemorrhage and by a segment of it being felt protruding through the os uteri. In all probability it passed—as ova in early abortions sometimes do—when the bowels or bladder were evacuated, and unobserved by the patient's immediate attendants. If not, the case must be reckoned one of fatal tetanus, with the ovum partially separated, but not expelled. I have been favored with the notes of it by my friend, Dr. Hislop of East Linton, in whose practice the case occurred.

Case IV.—The mother of a family, who had never aborted previously, had not menstruated for nine weeks, when she was seized with a considerable loss of blood from the vagina, which left the pulse quick and feeble. The bleeding soon ceased, but next morning Dr. Hislop found the pulse at 120, the os uteri relaxed and open, and a soft mass protruding through it. He introduced a sponge into the vagina to restrain the hemorrhage, which had recurred to a slight extent, and gave the patient some small doses of ergot of rye, to expel the ovum. The membranes were not observed in the vaginal discharge, but the convalesence of the patient went on satisfactorily, till six days subsequently, when she first complained of "weakness of the lower jaw." Next morning the jaw felt stiff, and could not be opened more than half. The patient herself became anxious and alarmed. In the evening any attempt at swallowing produced a sensation of choking; and there was a copious secretion of viscid saliva. On the following day the jaw was still more fixed, swallowing became impossible, tetanic spasms began to affect the back of the neck, and subsequently they stretched to the chest, back, and extremities. By night the opisthotonos was complete, the spasms increased in severity, the pulse rose to 160, her mind, however, remaining quite clear; and she expired next morning, or on the third day after the tetanic symptoms began. The treatment principally consisted of large opiates, interrupted during the second day of the attack, till the bowels were acted on by a dose of croton oil, and by turpentine enemata.

Tetanus, when it has taken place in obstetrical practice, has apparently been repeatedly mistaken, in its earliest stages, for an attack of cynanche (as in the first case I have detailed); or even oftener, perhaps, for some irregular form of hysteria. It is only after the tonic spasms have passed to other muscles than those of the jaw and neck, that the actual presence of such a fearful and fatal malady has in general been fully realized by the mind of the medical practitioner. The notes of the following case, obtained for me by Dr. Symonds, and drawn up by a medical gentleman, a near relative of the patient, afford an instance in point. The case also offers another example of the fact, that amid the mortal and agonizing struggles of the patient, the reason and intellect often remain clear and unclouded up to the last fatal spasm—the disease being originally and truly an affection of the reflex spinal system and not of the brain.

Case V.—A lady, aged 41, of a delicate and nervous temperament, and the mother of six children, miscarried at an early period of pregnancy (17th Nov. 1849). So much hemorrhage occurred that it was necessary to use the tampon. During the following week she suffered severely from the effects of loss of blood, and was especially troubled with palpitation, headache, and a sensation in the throat resembling the hysterical globus. On the morning of November 25th, or seven days subsequent to abortion, she experienced a difficulty in deglutition and a stiffness in the jaws and neck, which gradually increased and became paroxysmal in their character, leaving eventually no doubt of the existence of true tetanus, though it had been hoped previously, that hysteria, with the addition of "a stiff neck" from catching cold, would explain all. The existence of complete trismus subsequently formed a leading feature throughout. Deglutition, too, was much impeded at an early period, and soon became almost impracticable. The muscles of the neck, chest, and abdomen, were fully affected with the tetanic spasms; but not those of the back or extremities. Towards the termination of the case the larynx became involved, and the paroxysms of strangulation, several of which occurred before death, were agonizing in the extreme. The spasmodic exacerbations, irrespective of these laryngeal paroxysms, were very severe. About sixty hours from the commencement of the tetanic symptoms, death relieved the poor patient from her sufferings, in a paroxysm of strangulation. The mind and reason were unaffected to the very last.

There is a case of fatal tetanus after abortion, mentioned by Velpeau in his Essay on Puerperal Convulsions,[1] where the first symptoms of the disease were overlooked as unimportant. The details given by Velpeau are as follows:—

Case VI.—At the commencement of the year 1834, a young woman entered as a patient at the Hôpital Cochin, in consequence of a miscarriage. After some little time she was seized with trismus, to which little attention was paid. It was thought even that she complained without reason, and that she suffered less than she professed. A change in the service about this time occurring, she was in some sort forgotten. Nevertheless, these convulsive movements spread little by little, although slowly, to all the other parts of the body. Several bleedings were employed, but nothing was able to arrest the progress of these symptoms, and the unhappy woman died in a complete state of opisthotonos. No manifest lesion was found upon the dead body. The uterus, the brain, the spinal cord, and the viscera, appeared in a normal condition.

The symptoms of tetanus are liable to considerable variations in different cases. The following case of fatal tetanus connected with abortion, for the notes of which I am indebted to a very accurate observer, Dr. Ritchie of Glasgow, is remarkable as one of the rare instances in which the spasmodic paroxysms affected, during one period of the disease, the muscles that open the lower jaw, instead of, as usual, the muscles that close it:—

Case VII.—Mrs. ——, aged 40; the mother of seven children; eccentric and nervous; when in the third month of pregnancy was exposed to sudden mental agitation. On the instant, she had a flow of blood from the vagina. This ceased on lying down, when the os uteri was found shut, but the cervix bulky. Next day, on removing a tampon which had been left at the previous visit in the vagina, it was found that the os uteri could easily admit the finger, that the membranes were tense, and that there was no bleeding. The plug was reintroduced, and, soon afterwards, the membranes gave way, the rupture being accompanied by a loud noise and a discharge of watery blood and some clots. The tampon was again had recourse to, but about ten in the evening, much suffering being caused by it, the patient withdrew it. This was succeeded by profuse hemorrhage and alarming faintings till two A.M. There was no evidence of the expulsion of the ovum, excepting the appearance of some shreds of decidual membrane, although it was possible that such might have been passed unseen in the clots.

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[1] Loc. cit. Observation 21, p. 232.

From this day, the discharge ceased, the os uteri closed, and there was every symptom of convalescence, except that the neck of the womb continued to be bulky. Some clots passed from the vagina on the 11th day; and next day, while about to chastise a child, she was seized with locked jaw, succeeded by a sense of suffocation in the throat, and, two days after, by tetanic spasms or cramps in the muscles of the neck and back, and by acute shooting pains in the articulations of the lower jaw.

She was put on a course of calomel and opium with croton oil. The mouth became sore on the 3d day of the tetanic attack, and dysentery also set in, apparently from the oil. The spasms of the neck and face continued, although rather less intensely. Some fetid clots escaped from the uterus. She was now put on 25 drops of laudanum every three hours, the former medicine being omitted. On the 4th and 5th day of the disease the tetanic spasms ceased, and the dysentery also had disappeared. The opium was discontinued.

But during the course of the 6th day after the commencement of the tetanus, she was again accidentally excited, and in consequence seized with general rigidity of the muscles of the body. The mouth was thrown violently and widely open, the face became lived, spasmodic movements of the limbs succeeded, with complete insensibility, lasting for about half an hour. These paroxysms were succeeded by intervals in which the jaw became firmly closed, and she was affected with convulsive jerking movements, occasioned apparently by contraction of the extensor muscles of the back.

During the ensuing twenty-four hours, she suffered from about four or five of the first-mentioned seizures; and in the intervals her head was violently drawn backwards every few minutes, occasioning a sensation at the throat as if she were about to be strangulated. The severe fits usually began with a sense of constriction at the chest and of a want of air. She died at last in one of these fits, on the evening of the 7th day from the first commencement of the tetanic symptoms.

In some of the preceding instances of tetanus following early miscarriage, there was nothing particular in the phenomena of the abortion, and nothing special in the treatment which the cases required. In fact, in several of the examples adduced (as in Nos. II., III., and VI.), no medical or obstetric treatment seems to have been called for during or after the miscarriages—the cases being in all respects simple and uncomplicated. But in others (as in Nos.

IV., V., and VII.), there was one special complication present, viz., hemorrhage, and one special and effective mode of arresting it adopted, viz., plugging the vagina. I do not allude to these instances as any certain evidence that either this special symptom or special treatment were connected, in the way of causation, with the subsequent occurrence of the tetanus. The vagina is daily plugged by obstetric practitioners to restrain hemorrhage, both from the unimpregnated and pregnant uterus, without any such evil consequences following; but we have so far a certain amount of proof, in these instances, that tetanus, after abortion, is more particularly liable to supervene, either where the uterine surface is in such a state of injury and lesion as to lead to the protracted continuance of hemorrhage, or, where the maternal canals have been irritated by the tampon, when used as a mechanical means to diminish and arrest that hemorrhage. Dr. Adams of Lanark has furnished me with notes of a case of fatal tetanus after abortion that occurred in his practice where he was settled in Glasgow, which affords an illustration of the disease supervening upon the arrestment of the attendant degree of puerperal flooding. The patient was, in Dr. Adams' absence, attended by Mr. Crossken, who has drawn up notes of the symptoms and post-mortem examination.

CASE VIII.—Mrs. ——, the mother of several children, had a miscarriage at the third month. A slight degree of hemorrhage followed. On the eighth day after the abortion this discharge suddenly ceased, and a feeling of stiffness soon after supervened in the masseter muscles. Next day the jaws were quite locked, and the head was bent backwards with tetanic spasms, the muscles of the jaw and neck being fixed and rigid. The eyeballs were also sunk and the eyelids partially closed. The patient, however, was able to speak; but the deglutition of fluids was impossible. The pulse was 72; the bowels constipated and flatulent; and the urinary secretion natural. There was no discharge from the uterus, and no uneasiness or pain in that region. The pulse betimes became weaker, and the tetanic paroxysms more and more severe and frequent; and she sank and died in about seventy hours after the appearance of the first symptoms of tetanus.

The body was examined by Mr. Crossken and Dr. Fleming about thirty-six hours after death, and as the morbid appearances were in some respects peculiar, I will state them in Mr. Crossken's own words. "The uterus," he reports, "was about the ordinary size. Its substance and internal lining membrane were emphysematous throughout, full of air-vesicles, and crepitating under the fingers. In fact it was like a piece of lung, and resembled it also by floating in water. There was, however," he adds, "no appearance whatever of decomposition."

In surgical pathology, inquiries have been repeatedly instituted, with the view of ascertaining if there was any kind of settled connection between the existing state of the wound and the occurrence of tetanus, but without much success. For surgeons seem generally agreed upon the fact, that while the tetanic disease very frequently supervenes when the external wound appears in all respects perfectly healthy, in about an equal proportion of other instances, it comes on when the wound is unhealthy, or inflamed, or sloughing. In some of the preceding examples of obstetrical tetanus super vening after miscarriage, the lesion or wound, left on the interior of the uterus by the abortion, seems to have been in a "healthy state," as far as could be ascertained; in others it was so far unhealthy, as to have been a source of morbid sanguineous oozing and hemorrhage. In none of the examples of obstetrical tetanus, included in this or in the next division, does there appear to have been any tendency to inflammation of the uterus, as an antecedent to the occurrence of tetanus.

III.—TETANUS AFTER PARTURITION.

Puerperal tetanus, when following parturition at or near the full time of pregnancy, seems to be guided, in regard to the period of its supervention, by the same laws as regulate the occurrence of the disease after abortion, or after surgical operations and injuries. Under all of these conditions the tetanic attack usually does not commence till about a week after the occurrence of the exciting obstetrical or surgical lesion. According to some statistics published by Romberg,[1] in more than half of all instances of surgical tetanus—or in 112 out of 208 cases collected by him—the attacks set in between the 3d and 10th days after the receipt of the injury, or the occurrence of the operation. The same period includes the dates at which the tetanic seizure usually took place in the examples of obstetrical tetanus which I have already detailed, as well as in those which I have collected under the present head. In the following instance, which occurred in the practice of my friend, Dr. Lyell, of Dundee, the tetanic symptoms began on the sixth day after delivery at the full time, and when there was apparently nothing unusual in the labor itself, or in the puerperal state of the patient, to excite the disease.

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[1] Manual of Nervous Diseases, vol. ii. p. 105.

CASE IX.—A patient, aged 25, was delivered of her first child at the full term of utero-gestation, without any unusual complication, the labor being easy, and only of seven hours' duration. On the sixth day after her accouchement, stiffness of the muscles of the back came on, followed by symptoms of trismus, which daily increased. On the third day of the attack, on attempting to swallow, strong opisthotonos at length supervened, the head being drawn back between the shoulders. The tetanic symptoms went on increasing, till the patient sunk on the sixth day of the disease (the twelfth from delivery). The treatment consisted principally of large opiates; but they failed either in procuring sleep or in relieving the tetanic spasms. Venesection also, and turpentine in the form of enema, were tried. The child (a female) was living, and afterwards throve well. "There was," adds Dr. Lyell, "no perineal laceration or other injury to be ascertained."

In the following case of puerperal tetanus, reported to me by Dr. Lever of Guy's Hospital, in addition to the usual lesions left by delivery in the interior of the uterus, there existed also, as possible traumatic causes of the disease, lesions or injuries of the maternal passages and perineum:

CASE X.—A mother, about seventy-two hours after delivery, was attacked with tetanus. The disease presented all its most characteristic symptoms. In this patient laceration of the vagina and perineum had occurred during the process of labor. Opium was given in large quantities; but the disease proved fatal in three days. The child also died, affected with trismus and jaundice.

I have already alluded to the circumstance that an unusual degree of hemorrhage, and the use (sometimes prolonged) of the vaginal plug, seem to have been precedents to a considerable number of the instances in which tetanus appeared in connection with early abortion. In the two following cases of tetanus after parturition at the full time, the same or similar causes were also antecedent to the occurrence of the fatal attacks of this disease. For the notes of the first example I am indebted to Dr. Mackinlay of Barrhead; the second has already been placed on record by Dr. Storer of Boston.

CASE XI.—A mother, aged 27, was delivered, at the full time of pregnancy, of her second child, on the 2d February, 1852. She made for three or four weeks a fair recovery; but suddenly after some physical over-exertion and much mental disquietude, she was attacked with secondary uterine hemorrhage on the 27th February.

The flooding was so great as to require the vagina to be plugged by a surgeon, who was called in. He left the plug in the vagina for two or three days. On the 5th March, stiffness about the face and neck came on. Early next morning the trismus was very marked; and she continued to show, in an increasing degree, all the characteristic symptoms of tetanus, up to the 8th March, when death put an end to her sufferings. The tetanic spasms principally showed the opisthotonic form.

CASE XII.—Mrs. C——, aged 28, and the mother of two children, was delivered at the full time of an infant that weighed eight pounds. The umbilical cord broke off near its origin in endeavoring to extract the placenta. After some unsuccessful attempts to detach the afterbirth, it was considered proper to desist from further efforts. The attendant hemorrhage was slight. During the five following days the pulse remained good, and the patient free from fever or uterine pain. Towards the commencement of the sixth, a fragment of placenta was removed from the vagina, and after the use of ergot two other placental masses were expelled—decomposing, and offensive in smell. On the seventh day, the pulse was, for the first time, above 100, small and wiry; and the patient complained of pain in the head, considerable stiffness of the jaws, and a difficulty of swallowing. These symptoms rapidly increased during the day, and at night the tip of the tongue could scarcely be protruded between the teeth. The muscles of the neck and jaws had also become much more painful; the respiration was laborious; and, at irregular intervals, tetanic spasms were present. Next day, the eighth day after the birth of the child, the muscles of the face were so rigid, that the jaws could not be separated in the slightest degree. The merest touch seemed to distress the patient, and to hasten on the spasmodic attacks, which recurred every few minutes. The head was retroverted upon the pillow; and so firmly contracted were the muscles of the neck, that when the hand was placed behind her occiput, the whole body was brought forward, the neck not being flexed in the slightest degree. When the spasms were present, the patient's sufferings appeared to be extreme. The paroxysms increased in frequency until about midnight of this, the eighth day after parturition—when she sunk, exhausted by opisthotonos. Throughout, there were not any symptoms of uterine or peritoneal inflammation.[1]

External injuries and lesions form certainly the most common sources of tetanus. And next in frequency as an exciting cause of the disease, authors usually reckon exposure to cold, or rather to currents of cold and damp air—especially if the person has immediately before this exposure, been overheated or perspiring. Wounds may lead to tetanus without any exposure to cold; and exposure to cold sometimes leads to tetanus, without the presence in the body of any appreciable lesion. But though these two sources of tetanus may each confessedly lead to the production of the disease quite independently of the other, their *combined* action far more frequently leads to this result. Most of our surgical authorities, who have seen much of tetanus in practice, agree in stating that in surgical subjects the disease has very often been observed to occur immediately after those bearing surgical lesions upon their body have been accidentally exposed to cold currents, or to sudden changes of temperature. The same fact seems also to hold good with regard to puerperal tetanus. In one of the cases already detailed (Case II.), Dr. Wood found that, a few hours before the patient was attacked with tetanus, she had thrown her window open, and dressed her hair, exposed to the draught of cold air from without. In the three following cases of puerperal tetanus, all of which occurred in the practice of one physician, Dr. Aubinais of Nantes,[1] sudden exposure to cold seems to have been instrumental in lighting up the tetanic attack.

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[1] American Journal of the Medical Sciences for January, 1842, p. 97.

Case XIII.—In a primiparous patient, the milk-fever was high on the fourth day, when the woman, æt. 30, crossed a wet floor, with her feet naked, and while her skin was covered with perspiration. She was speedily seized with prolonged shivering, abdominal pain, and suppression of the lochia; and gradually the tetanic condition, commencing with difficult deglutition, markedly prevailed, inducing a slight degree of emprosthotonos. She died seven days after tetanic symptoms began.

Case XIV.—A woman, æt. 34, while in a state of perspiration, exposed herself to wet and cold four days after her confinement, with the effect of suppressing the lochia. Tetanic symptoms appeared twenty-four hours subsequently, and soon became strongly developed. The disease obstinately resisted the most active treatment until the tenth day, when it began to yield, and the patient ultimately recovered. She was bled six times, 100 leeches were applied, and musk and valerian were afterwards given.

Case XV.—A poor woman, æt. 28, having suffered from an attack of eclampsia, for which she was bled, was delivered a few days afterwards of a seven months' child. She went on very well until the fifth day, when, having drank a large quantity of cold water, she was seized with shivering, followed by trismus and difficult deglutition. These symptoms went on to complete tetanus. On account of her enfeebled state, blisters were alone resorted to, and the lochia were restored by warm baths. The spasmodic symptoms did not abate for twenty days; and forty elapsed before she was quite restored.

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[1] Revue Medico-Chirurgicale, tom. v. p. 149. British and Foreign Medical Review for July, 1849, p. 296.

The author of the article on Tetanus in the Dictionnaire des Sciences Médicales, M. Fournier-Pescay, mentions an instance, similar to the above, in which exposure to cold acted apparently as the more immediate excitant of an attack of this disease in a puerperal patient.

Case XVI.—A woman, sixteen days after her confinement, was for a short time exposed to cold, and cold draughts. She experienced at the time a sensation of great chilliness; and suppression of the lochia suddenly followed. Ten or twelve hours subsequent to this exposure, all the muscles of her body were powerfully contracted (*fortement contractés*). She was bled with the lancet, leeches were applied to the vulva and epigastrium, and emollient tepid drinks and baths were employed. Under their use the patient's symptoms disappeared, while the lochial discharge became re-established.[1]

In a case of puerperal tetanus, published from the notes of the late Mr. Colles, of Dublin, exposure to cold seems also to have been considered as an important link in the excitement of the attack.

Case XVII.—A woman who had been delivered a fortnight before of her sixth child, was exposed to còld. That same evening she felt tightness of her jaws, and was ordered opium and mercurial ointment. Next day she could open her mouth better, but during it had two or three tetanic paroxysms, and died.[2]

In a case which I have just cited from Mr. Colles, the disease occurred in June; and there seems to be a prevalent idea in the profession that tetanus is more common in our own climate in the warm than in the cold months of the year; in summer more than in winter. But the idea is of very doubtful accuracy. In one of the Registrar-General's Official Reports[3] there has been published a table showing the relative number of deaths produced by different special diseases in London during the four different quarters of the year, and from 1840 to 1847 inclusive. During these seven years, 179 individuals died in the English metropolis of tetanus. Of these 179 cases, 49 occurred during the spring quarter of the year ending with March; 37 during the summer quarter ending with June; 34 during the autumn quarter ending with September; and 39 during the winter quarter ending with December. Such facts show, beyond cavil, how very little influence, if any, season has upon the frequency of tetanus in our own climate.

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[1] Dictionnaire des Sciences Médicales, vol. lv. p. 15.

[2] Dublin Quarterly Journal, No. xxx. p. 288.

[3] Ninth Annual Report, p. 151, &c.

Traumatic tetanus is a disease which is generally alleged by pathologists to be more common in warm than in temperate climates, a result to which various causes may contribute, besides the mere amount of heat. And puerperal, like surgical tetanus, would appear to occur more frequently under the tropics than with us. Thus, in some remarks which Dr. Christie has published on tetanus in Ceylon, he casually observes, that he had occasion to treat a case of tetanus consequent on parturition, "which," he adds, "the native practitioners of Ceylon inform me is *not* an unfrequent occurrence here." Dr. Christie gives the following particulars regarding this instance of puerperal tetanus:

CASE XVIII.—"A healthy woman of the Portuguese caste was, two days after delivery of her first child, seized with rigidity of the jaw, and other symptoms of tetanus. The native doctors and midwives," continues Dr. Christie, "immediately pronounced her incurable, and their predictions were fulfilled. She was in vain treated with mercury, opium, and the warm bath."[1]

Individual examples of puerperal tetanus among the inhabitants of tropical climates are sometimes incidentally alluded to in the writings of those who have practised in these climates. Thus—

CASE XIX.—In a communication[2] upon tetanus, Mr. Dickenson, surgeon in Grenada, reports concisely the histories of thirty-three cases of the disease which he met during the time he was in practice in the West Indies. One of these cases of tetanus occurred in a female after delivery. The patient—a black—was seized with the disease soon after parturition, and died on the fifth day from the commencement of the malady. The treatment consisted of blisters, mercurial friction, and laudanum.

Puerperal tetanus has occasionally been observed to supervene after cases of operative midwifery. An instance in which tetanus succeeded delivery by turning, in a case of placental presentation, has been recorded by Mr. Finucane, of Nenagh.[1] The case is further remarkable in consequence of the extreme rapidity (if the reporter's data are given correctly), with which the tetanus ran through its brief and fatal course.

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[1] Edinburgh Medical and Surgical Journal, vol. viii. p. 415.

[2] London Medical Repository, vol. i. p. 192.

CASE XX.—A patient, in her fifth confinement, was attacked during labor with much flooding. On examining, Mr. Finucane found the placenta attached to the cervix uteri, and in consequence proceeded to extract the child, which was still-born, by the operation of turning. Four days afterwards, she had a violent rigor, followed by perspiration, which returned slightly on the following day. On the evening of this (the fifth day after delivery), the patient began to complain of stiffness about the articulations of the jaw, with pain in the back part of the head and neck. Early on the following morning the lower jaw was found completely fixed, and the patient was suffering under complete opisthotonos, the body resting on the heels and occiput. Every attempt to drink, excited violent spasms, and the patient died in a few hours. The tetanus ran throughout its fatal course in little more than fifteen hours.

The Cæsarean operation has, from the earliest annals of French surgery up to the present time, been performed many times in Paris. The present distinguished Professor P. Dubois has himself, I believe, operated on eight patients. But it is a remarkable fact, that not a single instance of recovery from the Cæsarean section is known to have ever occurred in the French capital. The case in which the patient survived for the longest time, at least of late years, was one operated upon in 1839, and where ultimately the woman died of tetanus, seventeen days after the performance of the Cæsarean section. The following are the principal points connected with this instance of the disease:

CASE XXI.—On the 22d January, 1839, Professor Dubois extracted a living child by the Cæsarean operation from a deformed dwarf. Febrile and inflammatory symptoms speedily set in; but by the 31st, these had, in a great measure, disappeared; the pulse was 110, and the patient's appetite had returned. The patient's state continued to improve still more up to the 5th February, or till the fourteenth day after the operation, when general uneasiness and stiffness in the movements of the jaw came on. The masseter muscles were soon affected with violent contractions; the poor ient was unable to drink; and the symptoms of tetanus became ırly established. The disease continued up to the 9th, the teta- affecting principally the muscles of the jaw and neck; fits of ocation supervened, and the patient at last sunk on the seven- ıth day after delivery, and the fourth after the first commence- ıt of the tetanic symptoms.

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[1] Lancet for June 2, 1838, p. 388.

t the time the tetanus began, the abdominal wound was already ıpletely healed, except at its lower angle. On dissection, the inal wound in the uterus was found closed, not by the union of edges of the cut uterus, but by their close adhesion to the ante- wall of the abdomen, to the posterior wall of the bladder, and fold of intestine. Several small abscesses existed among the toneal adhesions. In the spinal marrow nothing was found ept a very slight and doubtful softening at one point.[1]

n the following instance of tetanus supervening in a patient who been previously the subject both of a surgical operation and of turition, it is difficult to say whether the fatal attack of tetanic ase belongs to the class of surgical or of obstetric cases, or per- ıs to either. The disease set in at an unusual distance of time r parturition, and at a still longer date after the surgical opera- l.

ASE XXII.—In 1809, Mülder of Gröningen excised the knee- ıt of an adult female, in whom articular disease had been of long ıding. Great irritative fever followed at the end of nine days; l, later, she suffered intensely from the knee, and from hectic er. At the lapse of two months after the operation, she was ivered of twins, and ultimately she died from tetanus, seven eks after delivery and fifteen weeks after the operation for ision.[2]

et me make one further remark before closing this list of tances of puerperal tetanus. Occasionally cases of death from ınus after delivery are referred to in English official and tabular ords, without any special details in regard to them being given. e following two or three instances are examples of this remark, l afford additional evidence of the fact that puerperal tetanus is so very rare as the total silence of obstetric works on this sub- might lead us *a priori* to suppose.

ASE XXIII.—The late Dr. Merriman, in his Synopsis of Difficult Parturition, has published a table[1] of the cause of death in childbed, among 10,190 patients attended by one physician. There occurred 107 maternal deaths in this long list of deliveries. One of these puerperal deaths is noted as having been produced by "locked jaw." But no particulars are given.

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[1] Lancet, for 29th February, 1840, pp. 821–852.

[2] Wachter's Dissertatio de Articulis Extirpandis, Gröningen, 1810.

CASES XXIV., XXV.—From the Fourth Annual Report of the Registrar-General, it appears that, in the course of the year 1840, 140 of the inhabitants of England and Wales died of tetanus. The sex of the patients, in 116 of these cases, is given in the Report. Of these 116 patients, ninety-six were males, and twenty were females. The causes which led to the attacks of tetanus, in this long list of cases, are, of course, not specified. But, incidentally, it is stated in the Report[2] that two of the deaths from tetanus occurred after parturition, "lock-jaw caused by childbirth" being reported in two cases in which the patients were aged "34 and 35 years." Thus, out of the twenty cases of fatal tetanus among females during that year, two at least, or 1 in 10, were cases of puerperal tetanus.

The relative frequency or rarity with which tetanus occurs in obstetrical, as compared with surgical practice, could perhaps be properly ascertained by an analysis of the official returns given in for a few years to the Registrar-General of all deaths from this disease. And certainly the investigation would form an interesting point of inquiry in puerperal pathology.

NATURE OF PUERPERAL TETANUS.

It will be granted, I believe, by all pathologists that the existence of an injury or wound upon the external parts of the body is by far the most common cause of tetanus. After abortion and parturition we have the existence, upon the interior of the uterus, of a similar state of lesion. All authorities seem now generally agreed as to the facts—(1) that the human decidua is, as was maintained in the last century by Krummacher,[3] the thickened and hypertrophied mucous membrane of the uterus; (2) that the epithelial or superficial layer of it separates from its basement, or outer, layer, in abortion and after delivery; and (3) that this separation or solution of continuity of tissue, as well as the rupture of the organic attachments of the placenta from the uterus, leaves the interior of this organ so far in the condition of an external wound, or with a new aw surface for the time being exposed. Obstetrical tetanus has, this respect, an exciting cause essentially similar to surgical nus. And perhaps the great reason why this state of lesion of interior of the uterus does not more frequently give rise to nus is simply this, that the uterus is itself principally, or indeed ost entirely, supplied by nerves from the sympathetic system, le apparently, as stated by Mr. Curling and other pathologists, nus is an affection far more easily excited by lesions of parts plied with nerves from the cerebro-spinal system, than by lesions arts supplied with nerves from the sympathetic system.

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[1] Synopsis of Difficult Parturition, p. 339.

[2] Fourth Annual Report, p. 224.

[3] Membrana decidua "proprie est membrana uteri interna, quæ post conceptionem intumescit et crassescit, usque ad tertium circiter graviditatis mensem, &c."—See Krummacher's Dissertatio circa Velamenta Ovi Humani, 1790.

etanus is known to follow wounds very various in their degree severity. "Whether," says Professor Wood,[1] "the wound is ing or severe seems to be of little consequence," so far as re- ls the supervention of secondary tetanus. By what pathological hanism a wound or lesion of a part can, under any circum- ices, lead to an attack of tetanic disease, is an inquiry regarding ch we as yet possess little information; and in this respect the duction of obstetrical tetanus is not more obscure than the pro- tion of surgical tetanus.

he disease, when developed, essentially consists of an exalted or er-excited state of the reflex spinal system, or of some segment portion of that system. What circumstances in midwifery or gery might possibly, either singly or in combination, produce state, and so produce traumatic tetanus? In relation to this stion I will venture to make one or two hypothetical remarks.

.. We have in obstetric pathology evidence almost amounting to tainty that the analogous super-excitable state of the cerebro- nal system of nerves which gives rise to eclampsia or puerperal ivulsions is generally produced by the existence of a morbid son in the blood. And it seems not impossible that the genera- n of a special blood poison, at the site of the wound or else- ere, may sometimes in the same way give rise to obstetrical and gical tetanus. We know, indeed, that the introduction into the od, of particular vegetable poisons is capable of exciting an arti- ial disease quite analogous to tetanus.[2] Brucine and strychnine ve both of them, as is well known, this effect. Abundant expe- nents upon the lower animals, and cases of poisoning in the

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Practice of Medicine, vol. ii. p. 746.

Backer has shown that even after the spinal cord was divided in dogs, nux vomica, en by the mouth, produced convulsions in the paralyzed lower limbs, which it could y affect and reach through the blood.—Commentatio, &c., p. 139. On the other hand, anins found in the frog, that when the lower half of the spinal cord and its nerves was rely separated from all connection with the vascular system, no convulsions in the hind remities followed the use of strychnine, while the anterior extremities were affected as a.—Untersuchungen über die Rückenmark, &c., p. 50.

human body, have amply proved this.[1] Tetanus is a frequent disease from wounds, &c., in the horse and lower animals. Would it not, in relation to the possible humoral origin of the disease, be worthy of trial whether the blood of an animal dying of tetanus is ever capable by transfusion of infecting another animal with the same malady? The experiment is said to have succeeded with a disease having many analogies with tetanus, viz., hydrophobia. But—

2. We know further, with regard to the tetanizing effects of strychnine and brucine, that these agents do not necessarily require to circulate in the blood in order to produce their special effects. In the lower animals, when strychnine or brucine is applied directly to the spinal cord, tetanic effects speedily follow;[2] and in all probability, when they are introduced into the blood, they produce their tetanizing consequences, by being carried in the current of the circulation to the cord—thus toxicologically influencing it, as if they were primarily applied to it. Centric irritation, or centric morbid conditions of the cord (and, it may be, of the cerebro-spinal system), may lead, according to these experiments, to tetanic disease, independently of any morbid state of the blood. And, if the observations of Sims, Harrison, and others, are correct as to the occasional mechanical origin of *Trismus Nascentium* from pressure and displacement of the occiput upon the medulla oblongata, we have, in this obstetrical instance, tetanus resulting—as it certainly sometimes does under other circumstances—from direct injuries or affections of the nervous centres.

3. The appropriate and specific affection of the spinal cord, or cerebro-spinal system, constituting traumatic tetanus, would appear to be sometimes, if not always, a condition excited by some influence propagated upwards along the nerves, from the seat of the injury or wound to the central portions of the nervous system. In proof of this, we have the fact that occasionally, as in cases published by Hicks, Murray, Larrey, &c., but not often, the artificial division of the nervous communication between the seat of the wound and the

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[1] See details in the works on Toxicology. Thus, in a boy poisoned by false angustura bark, which contains both brucine and strychnine, Professor Emmert found that when "he touched the patient's arm, in order to feel his pulse, a sudden and violent *tetanic* spasm intervened; the eyelids opened wide; the eyes projected rigidly and immovably; the lower jaw was firmly compressed against the upper one; both lips separated from one another, so that the front teeth were exposed; the different muscles of the face were tense; the extremities were extended and rigid; and the spinal column and the head were violently drawn back."—Romberg's Manual, vol. ii. p. 129.

[2] In a number of frogs Stilling removed the heart, and, indeed, all the other viscera, and applied to the exposed spinal cord a drop of a solution of acetate of strychnine. All the animals thus treated were rendered tetanic in a few minutes. The tetanus was universal over the system.

nervous centres has arrested the disease, when performed in a very early stage of the attack. What the nature of the transmitted influence may be, we have at present no means of judging. But we have analogies for the transmission itself in some of the phenomena of electrical induction and propagation. And, let me further add, that the symptoms of tetanus assimilate themselves very greatly to a rapid succession of electrical or galvanic currents transmitted peripherally from a nervous trunk or centre to the sets of muscles affected.

At the present time various physiologists are busy with recondite inquiries into the laws pertaining to the electro-motive powers of the nervous and muscular systems of the body. When our knowledge of these laws is more advanced, physicians will, perhaps, be able to deduce from them a more correct and true pathology of tetanus and other convulsive diseases.

TREATMENT OF PUERPERAL TETANUS.

According to Mr. Curling, tetanus is not only less frequent in women than in men, but it is also, "*less fatal* in the female than in the male sex."[1]

Out, however, of the preceding list of twenty-five cases of obstetrical tetanus, only three recovered (Nos. XIV., XV., and XVI.); and these under opposite modes of management. There are two other cases on record of the successful termination of puerperal tetanus. The two instances I allude to have been reported by Dr. Symonds and Dr. Currie. The true tetanic character of both cases may perhaps admit of some doubt.

In his article on Tetanus, in the Cyclopædia of Practical Medicine,[2] Dr. Symonds of Bristol speaks of the first of these cases in the following words:

CASE XXVI.—"We remember," says Dr. Symonds, "attending a young woman, who was seized with lock-jaw soon after delivery of a still-born and premature fœtus; but the affection readily gave way to a turpentine injection, and we looked upon it as a mere sympathetic accident."

The other case of recovery from puerperal tetanus, which I have adverted to above, is mentioned by the late Dr. Currie of Liverpool in an essay on the treatment of tetanus and other convulsive diseases by the cold bath and cold effusion, which he published in the third volume of the "Memoirs of the Medical Society of London."

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[1] Treatise, p. 29.

[2] Cyclopædia of Practical Medicine, vol. iv. p. 874.

In this essay he details six instances of tetanus. He gives the few following particulars regarding the puerperal case:

CASE XXVII.—A poor woman, "in consequence of difficult labor, and, as she imagined, of local injury in some part of the uterus, was seized with the *spasmus cynicus*, locked jaw, and other symptoms of tetanus. She was immediately taken to the cold bath, and thrown into it, with good effects. The spasms disappeared, and though they afterwards returned in a slight degree, they gave way entirely to a second immersion."

Supposing these two instances of recovery to have been instances of genuine tetanus, it would still appear, from the evidence of all the cases which I have collected, that puerperal tetanus is nearly as fatal as surgical tetanus, for, out of the whole twenty-seven examples collected, twenty-two died and five recovered; or, the disease was fatal in the proportion of four out of every five persons attacked. Dr. Lawrie has calculated surgical tetanus to be fatal in seven out of every eight who are seized; but, like Mr. Curling, he believes surgical tetanus in females to be less fatal than this.

In such a communication as the present, it would be out of place to dilate on all the means usually recommended for the treatment of tetanus. Let me merely remark that, in obstetrical tetanus, no kind of local treatment to the seat of the original uterine lesion could be well applied, or would probably be of any avail, if applied. And, as to *constitutional* means, perhaps the most important are—

1*st*, The greatest possible quietude and isolation of the patient from all irritation, corporeal or mental, during the course, and for some time even after the resolution, of the disease.

2*d*, The special avoidance of painful and generally impracticable attempts at opening the mouth in order to swallow; but sustaining the strength of the patient, and allaying thirst by enemata, or by fluids applied to the general surface of the body.

3*d*, If there is any well-grounded hope of irritating matters lodged in the bowels, acting as an exciting or aggravating cause, to sweep out the intestinal canal at the commencement of the disease with an appropriate enema.

4*th*, To relax the tonic spasms of the affected muscles, and diminish the exalted reflex excitability of the spinal system by sedatives, or antispasmodics, with the prospect of either directly subduing this morbid reflex excitability, or of warding off the immediate dangers of the disease, and allowing the case to pass on, from an acute and dangerous attack, to a subacute, and far more hopeful and tractable form of the malady.

Various sedatives and antispasmodics have been recommended to lfil this last most vital and important indication in the treatment tetanus—as belladonna, stramonium, hemlock, henbane, musk, mphor, Indian hemp, hydrocyanic acid, valerian, camphor, &c. rhaps the two drugs of this class that have hitherto been most ed, and most relied upon, are opium by the mouth and tobacco enema. But certainly we have no decided evidence of the bene- ial effects of opium, even in the most heroic doses; and it seems ubtful even if this and other such medicines are readily or at all sorbed from the stomach and upper part of the intestinal canal cases of acute tetanus. Tobacco in the form of enema has ubtless often acted most favorably in arresting the spasms; but is a drug, the action of which is not easily or safely kept up with at degree of constancy which is required in acute tetanus. Lat- dly, the antispasmodic action of sulphuric ether and chloroform is been repeatedly employed to allay that exalted state of the flex nervous system, and to relax that resulting tonic contraction the maxillary and other muscles, which constitutes the essence tetanus. Medical men may yet discover therapeutic agents, to introduced into the body by inhalation or otherwise, the action which will be as directly anti-tetanic in their effects, as strych- ine is directly tetanic in its properties; and such agents, if they ere otherwise innocuous, would form the proper remedies for tanus. Here, as elsewhere, in future, physicians will probably ek for therapeutic remedies in the same way, and upon the same rinciples, as toxicologists search for antidotes to poisons. Chloro- rm in sufficient doses acts as a direct sedative upon the reflex ervous system, and upon exalted muscular contractility. In con- quence of this action, it affords us one of the surest and most anageable means of allaying common convulsive attacks; and it as now also, according to the reports in periodical medical litera- re. been repeatedly successful in the treatment of traumatic tanus,[1] whilst it has apparently also repeatedly failed in subduing e more acute forms of the disease. Perhaps some of the failures ave arisen from the patient not being kept sufficiently deeply and ontinuously under the action of the drug. If used in tetanus, its ction will require to be sustained for many hours, or oftener, per- aps, for many days. And there is abundant proof of the safety ith which its continuous action may be kept up under proper care nd watching. For instance, a few months ago I saw, with Dr. ombe, a case of convulsions of the most severe and apparently opeless kind in an infant of six weeks. The disease at once yielded, and ultimately altogether disappeared, under the action of chloroform, which required to be used almost continuously for thirteen days; as much as 100 ounces of the drug being used during the period. After all tendency to convulsions at last ceased, the little patient rapidly grew, and is at the present moment a very strong, healthy child. In a case of the successful treatment of traumatic tetanus, by Dr. Dusch, above sixty ounces of chloroform were employed.[1] Let me close these remarks with a case of puerperal tetanus treated with chloroform.

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[1] See, for example, Dr. Ranking's Abstract, vol. ix. p. 239 (three successful cases); British Foreign Medical Review for 1851, p. 464 (two successful cases), &c.

Case XXVIII.—In the following letter, dated 20th January, 1854, Professor Lawrie, of Glasgow, has kindly communicated to me the encouraging results, so far as they go, of an example of the disease which is at present under his care:

"Mrs. B——, a fine young woman, æt. 24, in the third month of her third pregnancy, miscarried on the 4th of this month (January, 1854). She lost a considerable quantity of blood, and required plugging, cold, and pressure, but was so well on Sunday, the 8th, that I ceased my attendance. On Thursday, the 12th, she complained of stiffness about the neck and lower jaw, but not suspecting the nature of her illness, she did not send for me till late on Saturday, the 14th. I found trismus well-marked—the spasm not extending beyond the neck—and the pulse nearly natural. Every attempt to swallow gave great pain, and produced a spasm in the muscles of the neck and larynx, which threatened instant suffocation. I forbade all attempts at swallowing, ordered nutritive enemata with 50 or 100 drops of laudanum every six hours, and pectra to the neck, with aconite and chloroform. There was little change till the night of Monday, the 16th, when the pulse had risen to 120, and the spasms had greatly increased, but had hardly extended beyond the neck; deglutition was impossible. I immediately exhibited chloroform, which acted admirably, and gave instant relief. I taught her husband and mother how to use it, and she has since been more or less constantly, and nearly continuously, under its influence. Thursday, 18th, the pulse was 96, and she swallowed with comparative ease. To-day, Friday, 20th, she is not quite so well; the pulse is 108, the abdominal muscles rather tense, and the rectum will not retain the enemata. For this last occurrence I was of course prepared; and, since the 15th, she has been carefully rubbed with oil, butter, and cream. She still swallows tolerably well. My *prognosis* in this case was from the first serious, for although the pulse was natural, and the spasms had not extended beyond the neck, deglutition was nearly impossible, and suffocation was nminent. One symptom I have forgotten, which is often :he most distressing—a constant cough from accumulated which cannot be raised or got rid of. In two days it has ıred. I now anticipate recovery. The *treatment* has been ͽ, with the exception of the chloroform, which has been ıeful. I attribute the improvement on the 17th and 18th to it; and if Mrs. B. recovers, she will owe her life to your ɔle discovery."[1]

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[1] Ranking's Abstract, vol. xvii. p. 63.

when not curative of tetanus, the use of chloroform has of no small service in relieving the sufferings and agonies of ient. Thus, in a fatal case of puerperal tetanus already in a preceding page (Case V.), the medical gentleman who ı the case observes, "We had the just comfort of finding the ms materially alleviated by chloroform, which was used the last sixteen hours of the patient's life. 'Chloroform! ırm!' was the poor sufferer's anxious exclamation whenever the spasms impending, and comparative tranquillity speedily l its application each time." Our practical power over the pid and fatal forms of tetanus, perhaps amounts yet to little ıan a "meditatio mortis." But even to alleviate the suffer-endant upon such a dreadful disease, should we effect no ınd to produce a state of euthanasia, is surely an object of the best directed efforts of the medical art. "Physi-says Bacon, "do make a kind of scruple and religion to th the patient after the disease is deplored; whereas, in my nt, they ought both to inquire the skill, and to give the at-:es for the facilitating and the assuaging of the pains and of death."[2]

DEGENERATION OF THE MUSCULAR STRUCTURE OF THE UTERUS AFTER DELIVERY.

(Monthly Journal of Medical Science, March, 1854.)

meeting of the Edin. Med. Chirurg. Society, 1st Feb. 1854, ıpson exhibited the uterus of a woman who had died some ter delivery, to demonstrate its fatty degeneration under the :ope, and remarked that in the uterus after delivery this r change occurred as a normal condition; the decrease of

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quently to the publication of this essay in the Monthly Medical Journal the patient r the continuance of the disease.

[2] gu's Edit. of Bacon's Works, vol. ii. p. 166.

the organ in size being consequent on the fatty degeneration of the muscular tissue, and its subsequent absorption in the form of fat. This involution of the uterus commenced, he thought, at the mucous surface, and radiated outwards, from the inner to the outer layers of the organ, being at first most marked internally.

Fatty degeneration of the uterine walls, or rather of a limited part or layer of them, from previous inflammatory or irritative disease—or from its mistimed occurrence before delivery—seems a condition leading in some cases to rupture or laceration during labor.

PERINEAL FISTULA

LEFT BY THE TRANSIT OF THE INFANT THROUGH THE PERINEUM.

(From Monthly Journal of Medical Science for July, 1855.)

The length of the perineum in the adult virgin female is usually stated by anatomists to vary from one inch to one inch and a half; but it is very frequently found shortened in women who have borne a family, from the liability of its anterior portion to become more or less fissured and lacerated during the passage of the child's head and shoulders.[1]

In consequence of the natural shortness of the perineum, it looks a priori almost impossible that the child and its appendages should ever be propelled directly *through* it during labor, while the orifices of the vulva and anus were left entire. But the state of the perineum is very different in the stage of labor immediately preceding the expulsion of the child, from what it is in the non-parturient condition. By the time the child's head at last fully dilates the external parts in labor, the perineum is so stretched, that it has become thin and lacerable, while, at the same time, it is enormously increased and expanded in all its superficial dimensions. The perineum when thus distended by the child's head has been found to measure six inches in breadth, or from one tuberosity of the ischium to the other; and in length three inches or more from the posterior commissure of the vaginal orifice backward to the anterior commissure of the dilated and elongated orifice of the rectum; or as much sometimes as seven inches from the fourchette to the point of the coccyx. When the perineum is thus attenuated and expanded, like a thin cap placed upon the head of the child previously to its expulsion, it is easy to conceive that a fissure occurring in the centre of e stretched perineal structures would readily enough tear and tend under the strong expulsive efforts of parturition, so as to low the head of the fœtus to pass through the accidental opening; d if the resulting lacerations assume, however irregularly, the rms of X, Y, or V, with their diverging lines passing somewhat either side of, but without rupturing *into*, the orifices of the rec- m behind, or the vulva in front, we may have the infant, cord, and acenta traversing the perineum, while the rectal and vaginal nals remain entire and intact.

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[1] See remarks on the frequency of its rupture, in vol. i. p. 367.

Instances in which the infant and its appendages were thus born d expelled through a central aperture in the perineum, have been ablished by Nedey, Coutouly, Merriman, and various other accou- eurs; and references to most of the recorded cases of this singu- r lesion are to be found in the writings of Moreau,[1] Duparcque,[2] d Dr. Churchill,[3] upon the subject.

None of these authors specially allude to any examples of a fistu- us opening remaining subsequently in the perineum, as a result d a proof of the perforation of it in the process of parturition. Such result indeed appears to be very rare, in consequence of the edges f the lacerated wound almost always perfectly uniting, under com- on surgical care and treatment, subsequently to delivery. A pre- ration in the Obstetric Museum of the University of Edinburgh resents, however, an exception to this general rule, and is a well- arked instance of that rare lesion, viz., perineal fistula in the male.

CASE I.—The patient was attended during her first labor by a ractitioner in the west of Scotland. The labor—as he subsequently formed me—was tedious, particularly during the advance of the ead through the lower part of the pelvis. After the perineum had ecome much stretched and distended by the child's head, and when he artificial support of it by the hand happened to be for a short ime withdrawn, a very strong expulsive effort supervened, and the ractitioner was recalled to the patient in consequence of her loud ries. To his surprise he found the head passing, or in fact, already early entirely passed through a rent in the perineum; and the next ain expelled the body of the child through the same opening. hrough this same perineal perforation, the cord and placenta were elivered. The sphincter ani and the anterior edge or fourchette of he perineum remained untouched. The sides of the laceration did ot entirely unite. A year subsequently to her delivery, I saw her, along with the late Dr. Dawson of Bathgate, under whose care she had come with symptoms of phthisis. The perineal fistula still remained, and, as we found on a post-mortem examination some months subsequently, it was about the size of the barrel of a goose-quill. The opening was situated about half an inch behind the posterior commissure of the vulva. The perineum was very thin at the site of the fistula, and converging lines of old cicatrices were still visible on its mucous surface. But its anterior edge, or fourchette, was strong and dense, and placed unusually far forwards over the vaginal orifice.

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[1] Revue Medicale, for June, 1830; or, Moreau's Traité des Accouchemens, vol. ii. p. 462, &c.

[2] Histoire des Ruptures de l'Uterus, p. 368, &c.

[3] Diseases of Pregnancy and Childbed, p. 403.

I have only been able to find on record two other cases in any way analogous, of perineal fistula originating in perineal perforation during labor. They are described by Marter of Kœningsberg[1] and Halmagrand[2] of Paris.

Case II.—In a primiparous woman, to whose assistance Marter was summoned by a midwife, he found the head of the child already passing through a central laceration in the perineum. The child was speedily pushed by the strong pains that were present through the abnormal aperture, and the placenta afterwards followed through the same crucial-shaped laceration. Inflammation of the lips of the wound subsequently occurred, and, despite of the use of ligatures, a perineal fistula remained, by which the menses escaped during the two subsequent years. She then again became pregnant; and this second child was born naturally by the vulva.

Case III.—In 1838, a patient applied to Halmagrand some time after delivery, with a perineal perforation not yet cicatrized, and forming a communication with the vagina. He cut the anterior bridle of the perineum, which was slender, made raw the edges of the fistula, and brought them together by the apposition of the thighs alone. Reunion and cicatrization took place in a few days. The patient was subsequently confined without any renewal of the perineal lesion.

To the preceding remarks, let me merely add that, as a means of preventing central perineal laceration, and the chance, consequently, of perineal fistula as a result, we have to trust to—1st. The common methodic manual support of the perineum, so as to save excess of pressure upon it, while at the same time we push the head forward to the vaginal opening—a means which, in the practice of Denman and Lachapelle, succeeded in preventing the child's head from passing through the perineum, after its central structures were split and burst; 2d. Delivery of the head, and its proper guidance through the vulva by the forceps, as has been effected by D'Outrepont, Hüter, and Braun, in cases in which this accident was impending; and, 3dly. Lateral incisions, if absolutely necessary, of the anterior edge of the perineum; for in this, as in the more common longitudinal forms of ruptured perineum, it is, I believe, better practice to make one or two slight cuts on either side of the fourchette, so as to regulate the site and direction of the lacerations that must occur, rather than leave their form and their character to mere chance alone. It is always an infinitely more important matter to save the sphincter of the anus than the sphincter of the vagina.

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[1] Siebold's Journal für Geburtskunde, vol. ix. p. 726.

[2] Demonstrations des Accouchements, p. 577.

COLLODION AS AN APPLICATION TO SORE NIPPLES.

(From Edinburgh Monthly Journal of Medical Science, July, 1848, p. 49.)

THERE is one extremely painful and unmanageable form of ulcer in which I have applied collodion with perfect success. I allude to fissures at the base of the nipple. Most practitioners know well the agony that some mothers undergo in consequence of this apparently slight disease: the ulcer or fissure being renewed and torn open with each application of the child. In two such cases I lately united the edges of the fissures, and covered them over with the solution of gun-cotton, making the layer pretty strong. It acted successfully, by maintaining the edges so firmly together that they were not again re-opened by the suction of the infant. The gun-cotton dressing is not like other dressings, affected by the moisture of the child's mouth; and, at the same time, by securing rest to the part, it allows complete adhesion and cicatrization speedily to take place. It is improved by the addition of a little olive oil.

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[1] Extracted from Proceedings of Edin. Medico-Chirurgical Society, May, 1848.

PART V.

PHYSIOLOGY AND PATHOLOGY OF THE PRODUCTS OF CONCEPTION.

ON THE ATTITUDE AND POSITIONS, NATURAL AND PRETERNATURAL, OF THE FŒTUS IN UTERO.[1]

(From Edinburgh Monthly Journal of Medical Science, January, 1849, p. 423.)

In numerous important points, the physiology of the human fœtus varies from the physiology of the human adult. The mechanism of some of the highest functions in the economy is different in the one from what it is in the other. The mode, for instance, in which nutrition is effected in the infant, before it leaves the uterus, differs greatly from the mode in which nutrition is conducted in the individual after birth. The means by which respiration is accomplished during intra-uterine life are very different from the means by which it is accomplished during extra-uterine life. The mechanism and course of the circulation is not the same in these two states of existence. After birth the circulation is double, systemic, and pulmonic; before birth it is carried on as a single circulation, by an apparatus intended afterwards, and ultimately, to serve for a double circulation. The common attitude and position of the fœtus and adult are equally different. The adult in his waking state, and whether walking or standing, carries the body vertically, with the head uppermost. The fœtus in utero, for some time before birth, usually also carries the body vertically, but with the head aced undermost. The position of the head in relation to the body ffers in the two states of being—in the adult it is placed higher an the body, in the fœtus it is placed lower than the body.

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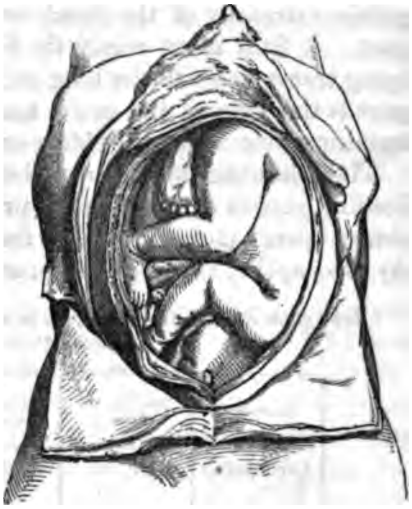
[1] Read before Edinburgh Medico-Chirurgical Society, December 13, 1848.

Dr. Simpson made a previous communication on this subject to the Edinburgh Obstetric Society in 1843. The above memoir contains the views then expressed, in a more matured form. Similar opinions were supported by him at the Medico-Chirurgical Society, December 20th, 1851, and are reported in the Edinburgh Journal for February, 1855.—(Ed.)

At, and for some time before, e full period of utero-gestation, e mode in which the child is tuated in utero is generally as llows:[1]—The trunk and spine e usually slightly curved, and ie head is bent forwards, with ie chin approaching towards ie sternum. The thighs are rawn upwards, and flexed upon ie abdomen; and the legs are exed and laid along the posrior surfaces of the thighs. he feet, which are sometimes rossed, are thus placed near the ates, and in front of them. In he interspace between the knees nd face the upper extremities are ituated, the arms being laid cross the sides of the thorax, and the forearms flexed and crossed i front of the chest.[2] The whole fœtus is thus flexed and rolled up ito an *ovoid mass*, of such a form as to occupy the least possible pace. One end of the ovoid is formed by the head, the other by the reech of the child.[3]

Fig. 3.



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[1] Through the kindness of Professor Goodsir, I am enabled to give, in Fig. 3, a sketch of e attitude of the fœtus in a subject brought into the dissecting rooms during the present inter. She had died of cholera, near the full time of utero-gestation. The placenta is seen tuated on the right side of the uterus, opposite the right foot of the infant. In injecting the essels, some wax escaped in the interspace between the two feet, and probably slightly ltered their position.

[2] "The most common situation of the extremities is not to be determined, as they are und to be a little different in different dissections; and in the living body they vary almost ery moment: thence the hands are seen indiscriminately on the head or face, or across ne another, or around the knees or legs, or the legs are sometimes extended, and the feet re placed by the face, or one in that position, and the other contracted and the foot downards."—Dr. William Hunter's Anatomical Description of the Gravid Uterus, p. 62.

[3] "The fœtus in utero," says Hunter, "is naturally contracted into an oval form, adapted o the figure and circumstances of its habitation. The vertex of the head makes one end of he oval and the nates the other. One side or edge of the oval is formed by the occiput, the ack part of the neck, and the incurvated trunk; the other is made by the forehead, and the mass of contracted and conglomerated limbs. The chin is close to the breast, the trunk is bent forwards, the knees are close to the fore parts of the hypochondria, the legs drawn to the back parts of the thighs; the feet, or lower parts of the legs, decussating each other; and the upper extremities contracted into the vacant space betwixt the forehead and knees."—An Anatomical Description of the Human Gravid Uterus, 1794, p. 62.

This ovoid is, in obstetric practice, found to be placed over or to present at, the os uteri at the time of labor, in three different modes. 1. In a large majority of cases, the head or cephalic end of this ovoid mass is placed lowest in the uterus, and consequently presents at the time of birth. 2. Sometimes, however, the nates or pelvic extremity of the fœtal ovoid is the lowest and presenting part. 3. Still more rarely the fœtus is placed with its long axis lying transversely to the long axis of the uterus, and the presenting part is the side of the ovoid mass of the fœtus; or, more strictly speaking, one of the shoulders or arms.

The following table shows the proportion in which these three leading genera of positions or presentations of the fœtus at the full time of pregnancy occurred in four large reports of cases furnished by Lachapelle, Boivin, Clarke, and Collins.

Table of the Relative Number of Cases in which the Presentation of the Child was found Cephalic, Pelvic, and Transverse.

Reporter.	Total No. of Cases.	No. of Head Presentations.	No. of Pelvic Presentations.	No. of Transverse Presentations.
Lachapelle,	37,126	35,550	1390	186
Boivin,	20.517	19,810	611	96
Clarke,	10,387	10,094	245	48
Collins,	16,654	16,102	504	48
Total,	84,684	81,556	2750	378
Proportions,		96 in 100	1 in 31	1 in 224

The preceding table shows the immense proportion of cases in which the head of the child is placed over the os uteri, and presents at the full term of pregnancy. In the above table, head presentations are in frequency, in proportion to all other presentations, as 26 to 1. The presentation of the pelvic extremity of the ovoid of the fœtus is, to that of the cephalic extremity, as 1 to 32. The presentation of the pelvic extremity, and its frequency in comparison to that of the head, is as 1 to 32, according to Desormeaux; as 1 to 33, according to Osiander; as 1 to 34, according to Carus; as 1 to 35 according to Meckel.[1] The transverse position of the fœtus, with its long axis across, or at right angles to the long axis of, the uterus, is by far the rarest of the three forms of presentation. In the preceding table of cases it occurred only once in every 224 labors. Rigby[2] estimates it as occurring in about 1 in every 230 cases; Churchill[3] estimates it as occurring in 1 out of every 261 labors.

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[1] See Adelon's Physiologie, tom. iv. p. 160, and Burdach's Physiologie, tom. iv. p. 224.

[2] System of Midwifery, p. 167.

[3] Theory and Practice of Midwifery, p. 356.

Various opinions, and modifications of opinion, have been suggested in order to explain why the head of the infant is thus, before irth, usually placed lowest and over the os uteri, and why some pecial circumstances should occasionally change this law, and produce malpositions. None of the doctrines hitherto proposed seem o have at all proved satisfactory. Indeed, so much so is this the ase, that latterly some authors have looked upon any successful nquiry into the causation of the attitude of the fœtus in utero, as a natter beyond the legitimate limits of physiological investigation. Thus, M. Virey[1] has strongly argued that we ought to content ourelves with considering the fact of the position of the head of the fœtus opposite the os uteri, simply as an ultimate law in animal physiology, and a law quite general to the whole animal kingdom. The law is perhaps not so general as M. Virey supposes.[2] But granting that it were even universal, still this admission could form no kind of logical reason against the propriety of investigating how n man, or in any other species, the effect in question is accomplished, and what mechanism is employed by nature for its production. At all events, if such an investigation be improper, then almost all our other physiological investigations are equally so. Every animal, from the lowest zoophyte up to man, respires, is nourished, and is propagated by generation. In other words, respiration, nutrition, and reproduction, are all of them ultimate and general laws in the animal kingdom; but their being so surely furnishes no reason for discarding all physiological inquiry regarding them, and would not entitle us to argue, as M. Virey does, that it is hence hopeless and improper to attempt to trace, for example, regarding reproduction or respiration, the variety of means and mechanism by which these ultimate laws are brought about and accomplished in man, or in the different series and classes of the animal kingdom.

At the present time, two opinions are principally or alone held by physiologists and obstetricians with regard to the mode of causation of the attitude of the fœtus in utero with the head lowest. It is regarded by some as a result of the mere physical gravitation of the head. Others look upon it as a result of the action of vital or mental influences of an instinctive and voluntary character. In the sequel I shall first speak of these two opinions, and of the objections to which they are open. Afterwards, I shall endeavor to show that the natural attitude of the human fœtus before birth is the result of excito-motory movements on its part. I shall then consider the circumstances which lead to preternatural positions of the fœtus, with a view of demonstrating that they are not only explicable by, but at the same time illustrative of, this new doctrine of the causation of the attitude of the fœtus. And, lastly, I may take an opportunity of offering some brief practical inferences and remarks.

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[1] Mémoire sur une loi de l'economie animale rélative à la position des embryons et des fœtus dans l'uterus.—Révue Medicale, July, 1833.

[2] I have more than once had an opportunity of dissecting the female of the common blue shark (Squalus cæruleus), sometimes caught by the fishermen on our coasts. The animal is viviparous; and in the distended uterus or oviduct, I found the enclosed fœtus placed in nearly equal frequency with the head and tail directed towards the uterine opening.

Section I.—Gravitation of the Head as the Cause of the Attitude of the Fœtus.

The head is, proportionably to the body, far larger and heavier in the fœtus than in the adult.[1] Most authorities have supposed that the position of the fœtus in utero, with the head undermost, was owing to the greater specific gravity of the head determining this part, in the erect position of the mother's body, to fall to the lowest part of the uterine cavity, or toward the os uteri.

The *period* of utero-gestation at which the preponderating gravity of the head is supposed to make it the lowest part of the fœtus, has been differently stated by different authors. Some, as Baudelocque,[2] Termanini,[3] Capuron,[4] Meigs,[5] and others,[6] maintain that, from the

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[1] The encephalon of a new-born child, according to Tiedemann, is so large as to be, relatively to the size of the body, as 1 to 6; in a full-grown man it is as about 1 to 40.—London Philosophical Transactions, vol. xxvi. p. 503.

[2] System of Midwifery. Heath's Translation, vol. i. p. 260.

[3] "During the first six months of pregnancy," says Termanini, "it is certain that the fœtus enjoys so much mobility, that its position cannot but be determined by the respective weights of its constituent parts. The head of the fœtus is, in fact, the heaviest part relatively to all the others after the second month, and it results from this, that it tends constantly to direct itself inferiorly towards the orifice of the uterus."—Archives Générales de Médecine, vol. vi. p. 288.

[4] Journal Universel et Hébdomadaire, tom. x. p. 437.

[5] "The natural presentation is that of the head, which is turned towards the os uteri from the earliest period of pregnancy. The navel-string is nearer to the pelvis than to the head of the child, the head therefore hangs downwards; but when the cord, by the growth of the ovum, has become of a very considerable length, the child ceases to be dependent from it, for the cord is not unfrequently from twenty to thirty inches long."—Meigs, Philadelphia Practice of Midwifery, p. 229.

[6] See Onymos, Dissertatio de Naturali Fœtûs in Utero Materno situ; in Schlegel's Sylloge ad Artem Obstetricam, vol. i. p. 525; Gehler, De Situ Fœtûs in Utero; in ibid., p. 537.

"It is not," says Burdach, "as has been hitherto believed, towards the last period alone of intra-uterine life, and by a sort of culbute, but from the second month of this same life, that the human embryo brings its head towards the inferior part of the uterus, or near its orifice. This phenomenon is the mechanical result of the erect position of the mother, since the umbilical cord is inserted at the inferior extremity of the trunk, and the upper half of the body, being the heavier, finds itself hanging."—Traité de Physiologie, vol. iv. p. 5.

"All the observations," says Hunter, "that I have been able to make in dissections, and in the practice of midwifery, would persuade me that the child's head is naturally downwards, through all the later months of utero-gestation; and that neither reason nor instinct teaches it, at any particular time, any trick of a tumbler or rope-dancer."—Dr. William Hunter's Anatomical Description of the Human Gravid Uterus, p. 64.

rliest periods of pregnancy, this is the common and natural atti-
de of the fœtus. Others uphold, as Mauriceau,[1] Roederer,[2] Levret,[3]
amotte,[4] and others,[5] that the position with the head downward,
d directed to the os uteri, is only taken towards the latter months;
d that it is assumed either gradually, or at last by a sudden move-
ent and toppling on the part of the fœtus (the movement of *culbute*
the older authors). Sir Fielding Ould[6] and Burton[7] supposed
at this downward movement of the fœtal head did not occur till
e commencement of labor.

Whatever explanation may be given of the position of the infant
utero, one fact has been sufficiently established in regard to it

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[1] "The spine of the child's back is placed towards the mother's; the head uppermost
d the feet downwards. It keeps usually this posture till the seventh or eighth month, at
hich, the head being grown very big, is carried downwards by its weight towards the in-
ard orifice of the womb, tumbling, as it were, over its head, so that then the feet are
ppermost, and the face towards the mother's great gut."—Mauriceau, Maladies des Femmes
osses, Chamberlen's Translation, p. 147.

[2] "I conclude," says Rœderer, "1. That the head of the embryo, which is extremely
ender. and supported upon the chest, occupies the top of the uterus after conception; that
inclines towards the abdomen; and that the face is turned towards this part. 2. That it
mains in this situation when some obstacle prevents it from descending; for example, a
o small quantity of liquor amnii. 3. That it descends gradually, according as it becomes
avier. 4. That at last, having become very large, it is carried downwards by its own
oper weight. 5. Several causes may, however, prevent its descending, or derange its
irection. I place in this class, defect of the liquor amnii, obliquity of the uterus, tumors
the uterus, premature efforts, and external violence."—L'Art des Accouchemens, p. 41.

[3] "The fœtus, after the fourth month of pregnancy, has the head above, the breech below,
e belly in front; but when it *approaches* the period of its birth, it is the back which is in
ont, the head below, and the breech above. This observation proves that the infant, at the
ter periods of pregnancy, brings down its head to where its breech was before, and that it
kes so by bending forwards, whether gradually or all at once, and this is what the ancients
ave called the *culbute*."—Levret, L'Art des Accouchemens, p. 74.

[4] "All authors," says Lamotte, "agree that the child in the uterus has its back towards
at of the mother, the heels backwards, the hands upon the knees, with the head resting
n them *till the seventh month*; that at this time the head, growing heavier by the increase
f its bulk, draws the body down, making it topple, and that then the head is below and the
et above, the face towards the mother's back; as to anything else, remaining in the same
osture it was in before, which is the situation in which it remains to the ninth month, and
n which it comes into the world."—Lamotte, Treatise of Midwifery, Translation.

[5] See Heister's Compendium Medicinæ, p. 111; Manningham's Compendium Artis Ob-
tetricariæ, p. 10; Haller's Elementa Physiologiæ, vol. viii. p. 412.

[6] Treatise of Midwifery, p. 25; and Preface, p. xiv.

[7] "We must therefore," says Burton, "assign some other cause, which, according to Ould,
eems to be as follows, viz., that the whole spine is curved, and its head looks down, so that
he fontanelle is just opposite the fore part of the mother's belly; and therefore as the first
nd greatest efforts for the expulsion of the child are in the bottom of the womb, which
resses directly on the back of the head, as is evident from the posture of the fœtus in utero,
nd from the foregoing description of the womb, they must of course immediately turn the
ead downwards towards the vagina, and its face to the mother's back, especially as it is at
his time floating in the waters. Hence it happens, that the change of the posture of the
tus does not happen in a natural way till the first labor pains begin."—Essay towards a
ew System of Midwifery, p. 100.

by modern investigations; namely, that the position with the head lowest is not assumed till the latter months of utero-gestation. Before the beginning of the sixth or seventh month, the ovoid mass of the fœtus lies in utero, in nearly an equal number of cases, with its cephalic and its pelvic extremities lowest, or over the os uteri, and in a considerable proportion of instances it is still placed across or transversely, so that the trunk is opposite the os uteri. Hence in abortions, presentations of the feet or pelvis are nearly as frequent as presentations of the head; and presentations of the arm are by no means uncommon. In the Maternity Hospital of Paris, Professor Paul Dubois found, that out of 121 fœtuses, born alive or dead before the end of the sixth month, 65 presented by the head; 51 by the pelvis; and 5 were cross-births, or presented by the arm or shoulder. Further, the position of the head towards the os uteri appears to be taken up more and more frequently and certainly from the end of the sixth month onwards. Out of 73 instances in which the child was born prematurely but alive, during the currency of the seventh month, Dubois found it presenting by the head in 61 cases, by the pelvis in 10 cases, and by the shoulder in 2 cases. Hence, while before the end of the sixth month the proportion of head presentations amounted to 55 in the 100, during the course of the next month they were found already amounting to 82 in the 100; at the full time they amount, as we have previously stated, to 96 or 97 in the 100.

But at whatever period of the pregnancy the attitude of the fœtus is supposed to be taken, the whole idea of its head being placed lowest and opposite the os uteri, by physical gravitation *merely*, is a doctrine to the adoption of which there are the following insurmountable objections:

1. The doctrine presupposes that the mother's body is in the vertical or upright position, in order that the gravitation of the fœtal head may have the effect attributed to it. But during her hours of sleep and rest her body is placed not vertically, but horizontally upon the back, or more frequently perhaps upon the right and left side, and ought to afford many more chances than the statistical results show, of the head falling by mere gravitation into other positions and localities than its usual and normal locality in the cavity of the cervix uteri. Besides, most practitioners have repeatedly seen patients restrained to the horizontal position for months before labor came on, without this position of the mother producing any deviation from the common position of the fœtus.

2. The doctrine further presupposes that the child is suspended

n the uterine cavity by the umbilical cord.[1] But this is not in acordance with fact, at least it certainly does not hold good at that dvanced period of pregnancy when, as we have just stated, the hild no doubt assumes the position with the head downward, viz., n the latter months. In order to suspend the fœtus by the cord, he placenta, from which the cord springs, would, in the upright osition of the mother's body, require to be fixed at the fundus uteri. The placenta, however, is almost always implanted on one side of the uterus,[2] rarely at the fundus. Further, in order to suspend the fœtus, the cord itself, as the medium of suspension, would require to be at least considerably shorter than the length of the uterine cavity, in which it is supposed to perform this function of suspension. But it is far too long to effect such a purpose. It is in general from eighteen to twenty inches in length, while the whole depth of the pregnant uterus is not above twelve or fourteen inches. The physical conditions, in short, necessary for the supposed physical suspension of the fœtus by the cord, assuredly do not exist in the latter periods of pregnancy, when the fœtal head first comes to assume the position downwards.

3. When physical conditions exist, the very opposite of those that have been supposed necessary to effect the suspension of the fœtus by the umbilical cord, these conditions do not make any corresponding deviation in the position of the child. When the cord is twisted and fixed around the neck of the infant, it makes the lower half of the fœtus, from the neck downwards, by far the longest and heaviest end of the supposed balance, and hence ought, according to the doctrine of gravitation, to make, in these circumstances, the pelvic instead of the cephalic end of the child the presenting part. Here, however, fact does not accord with theory. In about one in every

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[1] "The fœtus," says Capuron, "is generally suspended from the uterus by the umbilical cord, and represents a kind of lever or hydrostatic balance with two arms immersed in the liquor amnii. These two arms commence at the umbilicus, and terminate, the one at the apex of the head, the other at the heels or feet of the fœtus. . . . . The head, which is placed at the extremity of the super-umbilical arm of the balance, must obey the gravitation which forces it down to the neck of the uterus."—Mémoire sur la Situation du Fœtus pendant la Grossesse;—Journal Universel Hébdomadaire, &c., tom. x. p. 437. "The reason," says Hunter, "why the child's head is commonly downwards may be supposed to be this: the child is specifically heavier than the liquor amnii, and therefore, in the various attitudes of the mother, is always in contact with and supported upon the depending part of the uterus. This, in the more common attitudes, is the cervix uteri. The child's head and upper part of the trunk contain more matter, in proportion to their surface, than the lower part of the body; thence the head will more generally fall down to the lower part of the uterus."—Anatomical Description of the Human Gravid Uterus, p. 66.

[2] See Naegele on Auscultation, Dr. West's Translation, p. 79; Velpeau's Traité des Accouchemens, tom. i. p. 297.

seven cases, according to Dr. Churchill[1] (in 174 cases out of 1271 labors), the cord is found coiled around the child's neck or limbs, and yet without the usual position of the head being altered by it. Out of 1417 labors among the patients of the Edinburgh Maternity Hospital,[2] the cord was twisted around the child's neck once, twice, or oftener, in 164 instances, or in one in every eight cases; and in seven other instances, it was coiled both around the neck and limbs; but without changing in any of the cases the position of the infant from its normal direction with the head downwards.

4. If the physical gravitation of the head of the child were the cause of the normal position with the head lowest, then this position ought to be found with more frequency and certainty when the gravitation of the head from any cause was rendered proportionally greater than natural; with less frequency and certainty, when from other causes the gravitation of the cephalic extremity of the infant was rendered proportionately less than natural. The very contrary, however, of all this is the truth. In cases of intra-uterine hydrocephalus, the child's head is larger and heavier than usual; and sometimes it is so to an excessive degree. But this condition of the head, this increased preponderance and gravitation of it, does not render head presentations in these cases more common than usual, but the very reverse. In an excellent thesis on hydrocephalus of the fœtus as a cause of rupture of the uterus, my young friend, Dr. Thomas Keith, last year collected the histories of 69 cases of intra-uterine hydrocephalus at birth. Of these 69 hydrocephalic fœtuses, 11 presented preternaturally, or 1 in 6. When the head and fœtus are normal, preternatural presentations occur, in proportion to other presentations, as 1 to 26. Hence, preternatural presentation was four times more frequent in hydrocephalic than in natural cases; while it ought to be more frequent in the former than in the latter, if the doctrine of gravitation were true. Again, anencephalic fœtuses, with the whole brain and arch of the cranium wanting, are still often found presenting naturally. I have been present at the birth of three anencephali that had reached the full term of pregnancy. All of the three presented with the deformed and diminished cephalic extremity over the os uteri.

5. When a human fœtus of the latter months is placed experimentally in fluid, in descending through the fluid the head does not always turn and fall first to the bottom of the containing vessel, as the theory of gravitation takes for granted that it would. Thus, if a dead new-born fœtus be plunged into water, contained either in a

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[1] Researches on Operative Midwifery. p. 281.

[2] See vol. i. of this work, p. 754.

l shaped like the uterus, or in a large bath, the part which itates, and strikes the bottom of the vessel or bath first, is often ack or scapula, and not the head. M. Dubois first pointed out fact as the result of numerous experiments, which he had made etuses from the fourth to the ninth month; and in some of the nces in which I have repeated the experiment, I have found ame result.

When the child dies in utero, it still continues to be subjected e same physical laws as when it is alive. The mere death of œtus does not in any way remove it from the agency of gravin. When all its vital actions have ceased, the body should in be more subject than heretofore to all influences, such as gravin, which are merely physical in their character. The dead nt ought therefore, as frequently as the living, to have its head ed as the presenting part over the os uteri, provided the mere sical gravitation of the head were the reason and cause of that tion. Experience, however, amply proves that this is not the . In other words, experience proves that malpositions of the d, or the presence of other parts than the head at the cervix and teri, is in the latter months a far more frequent occurrence when child is dead, than when it is still alive.

uring the seven years that Dr. Collins had charge of the Dublin ng-in Hospital, 16,654 children were born within the house. Of e 16,654 children, 15,533 were born alive, and 1121 dead. Of 1121 dead children, many no doubt died during labor; but 527 hem were in a putrid state, and consequently may be correctly rded as having perished in utero, and before labor commenced. ong the 15,533 children born alive, 278 presented preternatuy, or 1 in 57; 250 presented by the pelvic extremity, or 1 in 62; resented by the upper extremity, or 1 in 555; 15,255 presented the head, or 98 in 100. On the other hand, among the 527 child born putrid, as many as 94 presented preternaturally, or 1 in 88 presented by the pelvic extremity, or 1 in 6; 6 presented by upper extremity, 1 in 88; 433 presented by the head, or 83 in . This contrast between the proportions of preternatural and ral positions among the living and dead children, may be exsed in a tabular form as follows:

Relative Proportion of Natural and Preternatural Presentations among Living and Putrid Children.

Presentation.	With 15,533 Living Children.	With 527 Putrid Children.
Cephalic Presentations, . . .	15,255, or 98 in 100	433, or 83 in 100
Pelvic Presentations, ...	250, or 1 in 62	88, or 1 in 6
Transverse Presentations, . . .	28, or 1 in 555	6, or 1 in 88

7. The position of the fœtus in utero, with the head lowest, is a vital action, and dependent upon the existence and continuance of life in the infant; and consequently it is not simply a mechanical result, and dependent upon the mere preponderating weight of the head of the child. The last paragraph (6), proves that the position of the head at the os uteri is by no means so frequent when the fœtus is dead, as when it is alive; that, in other words, its *vitality* is in some way or another connected with the existence of this particular position, with the head-undermost and over the os uteri. One objection may be urged against this deduction, as derived from the preceding data regarding putrid children, viz., that as "putrid" fœtuses are often born prematurely, the frequency of their malposition may depend upon the prematurity of their birth, and not upon the fact of their death. In some of the data drawn from the Maternity Hospital of Paris, and published by Professor Dubois, there is an answer to this objection. I have already stated, that out of 73 children born prematurely, but alive, during the currency of the seventh month of pregnancy, Dubois found that 83 per cent., or 61 in the 73 cases, were head presentations; the pelvic extremity presented in 10 instances out of the 73, or in one case in 7; and the shoulder presented in two instances. These children were, I repeat, born *alive*. But when the children born during the course of the same seventh month of utero-gestation were already *dead*, the results were very different. Out of 46 children thus born prematurely, but dead, during the currency of the seventh month, 45 per cent. only, or 21 in the 46 cases, were head presentations; the pelvic extremity presented in 21 instances out of the 46, or in about one case in two; and the shoulder presented in four instances. The following arrangement expresses these curious facts in a tabular form:

Table of the Relative Presentations among Children born prematurely during the Seventh Month, as affected by the Life or Death of the Child.

State of Child.	Total Cases.	Shoulder.	Pelvis.	Head.	Proportion of Head Cases.
Alive, . .	73	2	10	61	83 in 100
Dead, . .	46	4	21	21	46 in 100

The data which I have already adduced in a preceding page, prove that the fœtus does not take any very determinate position till nearly the commencement of the seventh month; and that during the course of the seventh month, the position with the head downwards begins to be assumed with considerable frequency and certainty. The data in the present table further prove, that the posiion with the head downwards is lost, or not taken, provided the nfant be dead. Or, in other words, the table shows what I have tated at the commencement of the present paragraph, that the assumption and maintenance of the position of the fœtus with the head undermost, and over the os uteri, is dependent upon the existence and continuance of *life* in the child, and consequently is not a physical but a *vital* action.

The next question, therefore, which meets us is this—What is the nature and character of the *vital* action by which the fœtus obtains and keeps up its position in utero with the head lowest? The assumption and maintenance of any position of the body, if dependent not upon physical but upon vital causes, can result from no other known vital action than a muscular action. Human physiology presents us with no other known action or power whatever, except muscular action, that can produce motions capable of altering or regulating the position either of the whole body or of any of its parts. And the real question, therefore, resolves itself into this—What is the nature and character of the *muscular* action by which the fœtus assumes and maintains its position in utero with the head lowest?

SECTION II.—INSTINCTIVE AND VOLUNTARY MUSCULAR ACTIONS AS THE CAUSE OF THE ATTITUDE OF THE FŒTUS.

Some physiologists, as Cabanis, Ennemoser,[1] &c., have upheld that the unborn fœtus is already endowed with physical powers, and performs acts referable only to the existence and exercise of mind. They maintain that metaphysicians have as yet one department of their science to investigate, viz.: the state and degree of development of psychical life in the intra-uterine fœtus. Cabanis[2] holds that the unborn infant already possesses the consciousness of its own existence with the first traces of fundamental ideas, and has already wants and desires, and both the will and the power of exciting volitional muscular movements.

Long ago, Ambrose Paré[3] and Chamberlen[4] attributed the position of the head of the infant at the os uteri in labor, to a psychical rather than a physical cause. But the author who has principally maintained and developed the idea, that the position of the child is a psychical result, is Professor Paul Dubois of Paris.

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[1] Historisch-physiologische Untersuchungen über den Ursprung und das Wesen der menschlichen Seele. Bonn, 1824.

[2] Rapports du Physique et du Moral de l'Homme, tom. ii. p. 431.

[3] After speaking of the infant, at the full term of pregnancy, requiring more food than it can now obtain through the vessels of the cord, Paré describes it as endeavoring violently to escape from the uterus to supply its wants, and thus, by its strong violence (*grande impetuosité*), breaking the membranes. When the womb then begins to open, "the childe," says he, "pursuing the aire which hee *feeleth* to enter in at the mouth of the womb, is carried with its head downwards."—Paré's Works, English translation, p. 899.

[4] Of the celebrated obstetric family of the Chamberlens, the inventors of the forceps, we have few or no literary remains. Hugh Chamberlen, one of the sons, translated the work

In 1832, this distinguished obstetrician communicated to the Academy of Medicine of Paris, an elegant and remarkable essay on the subject. M. Dubois' essay is published in the second volume of the Academy's memoirs, under the title of "Memoire sur la cause des Présentations de la Tête pendant l'accouchement et sur les determinations instinctives ou volontaires du fœtus humain."

After showing, by some of the arguments that I have stated in the preceding section, that the position of the fœtus, with the head lowest and over the os uteri, is certainly not the result of gravitation, M. Dubois, by a kind of reasoning by exclusion, comes to the conclusion, that the position in question must be the result of instinctive and voluntary determinations on the part of the fœtus, for he confesses himself unable to conceive any other influence by which it could be effected.[1]

The terms, however, used by M. Dubois, "instinctive and voluntary," are so much of a conventional character, and are liable to have such different significations attached to them by different writers and readers, that it is necessary for us to understand the meaning which M. Dubois himself affixes to them, in order that we may justly appreciate his views and reasoning on the subject. M. Dubois enables us to do this by an illustrative explanation, which he himself gives of the terms, in a note appended to his essay; while, at the same time, he confesses how difficult it is, in relation to some motions and actions, to separate and limit the effects of instinct from the effects of volition and individual mental intelligence.

A bird builds a nest. The building of the nest is, says M. Dubois

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of Mauriceau, and has added a few sparse notes of his own. To the passage which I have already quoted in a preceding page from Mauriceau, regarding his belief in the preponderating weight of the head of the fœtus being the cause of its common position, Chamberlen affixes as an annotation, that the fœtal head comes lowest, and to the os "rather by a natural *propensity* than any weight of the head."—Translation of Mauriceau on Diseases of Women with Child, &c., p. 147.

[1] "M. Dubois," says Dr. Ramsbotham, "has ascribed the general situation to an instinctive impulse implanted in the fœtus, which inclines it to take the most favorable position for its escape, as the needle points mysteriously to the pole. But such a mode of reasoning or illustration cannot be considered either as argumentative or conclusive; it is, in fact, completely evading the question, after attempting to elucidate it; and the method he has taken can only be regarded as a cloak for human ignorance. It would, in my opinion, be much better not to endeavor to explain the secrets of nature, so deeply hidden, but to content ourselves with referring this also to a general, though not invariable law—a part of the great system which shows the design and exemplifies the harmony that reigns throughout the whole works of Providence."—Obstetric Medicine and Surgery, p. 309.

inctive act, and the object of an irresistible impulse; as well
the form or general plan and materials of its construction in
articular species. But, in his opinion, the means by which
stinctive determination is attained, such as the choice of an
ent place, and the search for, seizure, and carriage of the
als appropriate for the edifice, so far involve and imply, in
nt shades, acts of individual mental activity and volition—
ntary determination"—on the part of the bird.

he word "instinctive," therefore, as applied to the attitude
fœtus, M. Dubois implies the fact, that the situation of the
ver the os uteri is an act performed by the infant, indepen-
of all experience and all imitation; and with the object and
f placing its head in the position which is most favorable for
ty and protection during labor. The word, in short, signifies
e *final cause* of the position is one of wisdom and foresight;
ery person will, I believe, readily grant this part of the pro-
n. Any other position would endanger far more greatly the
of the child, and consequently the whole object of the func-
reproduction, viz.: the continuance of the life of the species.
rincipal peril which the fœtus undergoes during labor is the
of asphyxia from compression of the umbilical cord. In
resentations, however, the risk of this accident is immeasura-
s than it is in pelvic or transverse presentations. The whole
of the child is that of a cone—the head forming the basis of
ne, and the feet its apex. When the head or basis of the
resents, and dilates the passages first, then, after the birth of
ad, a single pain generally expels the body, and the chances
pression of the cord come thus to be rendered exceedingly
But when the apex of the cone, the feet or pelvis, is placed
os uteri, and advances first into the passages, the cord is sub-
much greater chances of compression between the mother
fant; for the higher and broader end of the cone then slowly,
ith gradually increasing difficulty, makes its transit through
lvis, the cord becoming more and more liable to compression
this transit. Many more children are consequently born
under transverse and pelvic, than under cephalic presenta-

acknowledge then, willingly, in this sense, the truth of M.
s' expression of the position of the child being an "instinctive
nination," in so far as the final cause or object of the position
erned. But we believe that he takes an erroneous view of
bject when he adds, that the mechanism by which nature
this instinctive determination of the head downwards to the
ri, consists of an almost constant series of movements, pro-

duced by "intentional determinations," and "small volitions" ("*petites volontés*,") on the part of the fœtus. The position of the fœtus is, I believe, certainly the result of the movements which it itself performs in utero; but these movements are not of a volitional, and consequently mental origin, but simply of a reflex or excito-motory character. Before pointing out the proofs of this view, let us glance at what the intra-uterine movements of the fœtus really are; and in doing so, I shall principally follow and abridge the lucid statements of M. Dubois himself.

When we apply and press our hand over the pregnant uterus, in the latter months of pregnancy, movements of the fœtus are often produced, and these movements are simultaneously perceived by us and by the mother. Touching and irritating the fœtal head through the os uteri during labor, generally leads to the same result. The pressure of the end of the stethoscope upon a projecting part of the body of the fœtus, almost always produces fœtal movements. Sometimes it can be felt striking its limbs, as it were, against the instrument, or against the uterine parietes, when we have occasion to use auscultation during pregnancy or labor. "Pregnant females," says M. Dubois, "can generally make and provoke fœtal movements at will; for there are few mothers who, in attempting to seize or tickle through the abdominal walls, the small fœtal feet which sometimes press painfully against and protrude these walls, have not felt the infant immediately draw them back, in order to take up another position, or answer by a rapid blow to the impression which it perceives." Warm or cold bodies when swallowed, or when applied to the abdomen externally, are thought sometimes to exert a marked influence upon the movements of the fœtus in the way either of increasing or suspending them.[1]

"Thus," adds M. Dubois,[2] "not only the susceptibility and the power of locomotion are exercised during fœtal life, but even the regular and almost constant succession of impressions perceived, and of movements resulting from these impressions, indicates sufficiently t in the fœtus there is the same connection between these two ctions as exists in the adult. The fœtus in the uterine cavity s the feeling of external impressions, and these command, so to eak, the exercise of its powers of locomotion; and this faculty is und nowhere in the animal kingdom without being associated with ontaneous and intentional determinations, and consequently with ternal perceptions, which ordain them."

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[1] In this instance, viz.: the apparent fœtal movements following the impressions of co or heat applied to the surfaces of the abdomen or stomach—the uterine walls are probabl *first*, if not alone, affected through their reflex system of nerves. All the different varie of movements mentioned in the text have been maintained by Eggert (see Rust's Magazi vol. xvii. p. 62) to be movements in the uterine or abdominal walls alone, and not in t fœtus. Every physician and mother can easily prove, by their own daily experience, t unsoundness of such a view. In speaking of the fœtal movements apparently followi sudden external impressions of heat and cold, M. Dubois states, that "in rural economy th take advantage of this fact to ascertain if chickens have perished in their shells before t end of incubation. With this view they plunge the suspected eggs into cold water, a immediately the movements of some show the enclosed fœtuses to be still alive; the imm bility of others show that in them the chicks have perished."

[2] Mémoires de l'Académie Royale de Médecine, tom. ii. p. 280.

These fœtal movements, M. Dubois further shows, are generally xerted and repeated under certain determined conditions, as, for stance, under great changes of position on the part of the mother, when she suddenly sits up or lies down. In some mothers, par- cular positions on the side, back, &c., always produce them. When e mother's stomach is empty, the fœtal movements sometimes ecome excessive. And he adds, "When it happens accidentally, uring pregnancy or labor, that the umbilical cord of the child be- omes compressed, the fœtus testifies by repeated movements both e sentiment of restraint which it feels, and its wish to be relieved om it."

From these facts, M. Dubois draws the following conclusions: The human fœtus, therefore, possesses the faculties of sensation and otion, and these faculties of sensation and locomotion are already elated the one to the other in the same manner as they are destined o be during the whole life of the individual. In the narrow sphere f its existence, it has wants, the feeling of well-being and perhaps f pain; habits, and perhaps desires, and volitions; its movements re often the decided indications of these feelings and desires, as its ries will be after its birth. Further, if any of its movements appear o us to be altogether automatic, it is only because we have not tudied them so profoundly as to discover their cause, or guess at heir intention."

"Having demonstrated that the fœtal movements have an object, ometimes fixed, sometimes only to be guessed at, and that they ay, consequently, be looked upon as true instinctive determina- ions, we have only further to prove that it is in consequence of a etermination of this sort that the head of the fœtus in the mamma- ia is generally found situated at the pelvic end of the uterus. This oint we shall demonstrate in very few words, for the proof itself esults from the very facts which we have already adduced. If by ome of these facts we have demonstrated that it is not by the laws f gravitation that the head of the fœtus is drawn towards the uterine rifice—if by other facts we have shown that, during intra-uterine ife, some acts depend upon an instinctive or voluntary action, then t appears to us altogether impossible not to class among these last

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[1] Ibid. p. 284.

the almost constant position of the head at the os uteri; for it i only possible to choose between this cause and mere chance (l *hazard*); the choice of intelligent men in this case cannot b doubted."

These conclusions of M. Dubois are, I believe, not what the fac themselves warrant and point to. We fully admit the truth of hi premises regarding the extent and variety of muscular movement performed by the fœtus; we further believe, as will be subsequentl shown, that these movements constitute the immediate cause an mechanism of the peculiar attitude and positions of the fœtus; bu we altogether dissent from the deductions which he has drawn fro his data with regard to the actual physiological nature or characte of these movements. They are not the result of the "spontaneo determinations" of the fœtus: they do not prove as he suppos that "the faculties of sensation and motion are developed and exe cised long before birth," already connected together, as in the adul by intermediate mental operations, such as sensation and volitio they are not *psychical* nervous actions, but *physical* nervous action they are not, in short, volitional or sensorio-volitional movement but movements entirely of a reflex or excito-motory character; move ments not dependent on the cerebral system, but referable entirel to the "true spinal system."

It is well known that modern physiology and medicine stand i debted to the masterly and beautiful investigations of Dr. Marsh Hall, for systematizing and elaborating our knowledge of a larg and most important class of animal muscular movements, that a fundamentally altogether independent of sensation, consciousne or volition, and consequently are not mental in their origin working; that are connected with the spinal cord as their nervo centre or centres, and not with the brain, and may hence be pe formed when all influence from the brain is entirely removed;—a that originate in impressions, made principally upon mucous a cutaneous surfaces, which impressions being conveyed from the surfaces by afferent, excitor, or incident nerves (as they are variou termed) to the spinal centre, lead thus indirectly to many and oft complex muscular movements, through some of the motory nerv connected with that centre becoming in consequence secondar excited. The resulting movements are termed reflex or excit motory, because they are induced by excitations transferred reflected from one set of nerves to another, that is, from the excit to the motory nerves, through an intermediate nervous centre. T movements of the fœtus in utero are of this reflex or excito-moto character. That they are so is, I think, proved by the fact, that all important points they correspond with the most fixed charact istics of other undoubted forms and varieties of reflex or excito-motory muscular actions. The principal characteristics of true reflex movements consist—1. In the anatomical conditions under which they are capable of being performed; 2. In the origin and character of the movements themselves; and, 3. In the physiological object or objects with which they may be performed. I proceed to show briefly, that in these various particulars the movements of the fœtus in utero correspond to the proper characteristics of true reflex or excito-motory movements.

1. *The Anatomical Conditions under which the Movements may be performed.*—Dr. Hall and others have shown, that true reflex or excito-motory movements persist in decapitated animals, when all influence of the brain, and consequently of sensation and volition, is necessarily removed. In instances of paraplegia in the human subject, dependent upon complete destruction and division of the spinal cord in the dorsal region, and hence when the agency of the brain is entirely cut off, reflex muscular movements sometimes remain in the muscles of the lower extremities, and are capable of being excited, sometimes violently, by slight irritations applied to the soles of the feet, &c.—the patient *seeing*, but not feeling, or being otherwise aware of, the resulting movements in his own limbs.[1] That the fœtal movements are of the same character, is so far proved by the anatomical fact, that they may occur and go on during the period of intra-uterine life, and even for a day or two after birth, when, from malformation, the brain is entirely absent. The histories of cases of anencephalous monsters show that the intra-uterine movements are the same with them as with other children; and I have seen one survive for thirty-six hours after birth, performing various excito-motory movements during that time—swallowing, crying, and moving the limbs when the skin was pinched or irritated.[2] These cases afford us evidence of this amount—that all the usual fœtal movements are capable of being performed without a brain, and consequently without the agency of mind, and hence are not of a sensational or volitional character, but fall within the class of reflex or excito-motory movements.

2. *The Origin and Character of the Movements.*—If *after* birth we irritate the sole or palm of the new-born infant (whether awake or asleep), muscular movements are excited in the limb. These are acknowledged by all physiologists to be excito-motory move-

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[1] Cases of the same kind have been recorded by Lallemand, St. Hilaire, &c. &c. See Dr. Hall's Work, p. 134; or St. Hilaire's Histoire des Monstruosités, tom. ii.

[2] See the interesting cases recorded by Dr. Burlow, Dr. Budd, &c.; or Dr. Marshall Hall's own work, On the Diseases and Derangements of the Nervous System, p. 234, &c.

ments, as much so as the closure of the lips around the finge
nipple. If, *during* labor, the foot or hand present, and we irr
in the same way the protruded limb, it will excite motion in
the same kind, as I have in several cases taken an opportunit
ascertaining. During labor, irritation of the scalp of the child
the finger, in common head presentations, is usually followe
the same way by some movements of the head. Go a step fu
back in the life of the infant, and apply similar irritations t
limbs or surface of the infant through the abdominal walls
birth, or during pregnancy, and, as we have seen, the same re
are obtained; movements of a similar kind are excited, and the
tated limb is moved away. But these movements are to the fee
of the mother exactly the same in character as those which
usually and constantly perceived by her as made by the un
fœtus within her. Their independence of the brain, and
excito-motory character, is proved by their existence in anenc
lous fœtuses *after* birth, as stated in the last paragraph. The
ginate in excitations or impressions made upon the cutan
surfaces, as many other reflex movements do. We shall afterw
see that the almost incessant and persistent character of the
ments in some fœtuses during their intra-uterine life is an
evidence of their excito-motory nature. For the "true spinal
tem never sleeps."

8. *The Physiological Object of the Movements.*—The acts and ph
logical objects of the reflex or excito-motary system are state
Dr. Marshall Hall in the following terms: "These acts," say
"are found to preside over two important classes of functions,
those of the *preservation* of *the individual*, and of the *propagat*
the species."[1] It would be difficult to describe in more terse an
posite words the objects aimed at and attained by the positi
the head of the fœtus over the os uteri at the time of labor.
excito-motory character of the fœtal movements, as leading t
position, is so far proved by the result of the movements fulf
both the ends for which reflex motions seem specially designe

But *how* do excito-motory or reflex motions on the part o
fœtus place the head of the infant below its body, and over t
uteri? In the next sections I shall attempt to answer this que
by showing by what means and mechanism these fœtal refl
excito-motory actions make the child assume and maintain
common position with the head directed downwards; or force
place itself in malpositions or preternatural presentations
various preternatural circumstances.

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[1] New Memoir on Nervous System, 1843, p. 51.

ɔN III.—REFLEX OR EXCITO-MOTORY MUSCULAR MOVEMENTS THE CAUSE OF THE ATTITUDE OF THE FŒTUS.[1]

us now attempt to point out the means or mechanism by
the special position of the infant in the latter months, with
ad downwards, is assumed and maintained through the in-
entality of the fœtal reflex or excito-motory movements. In
ng this object, we shall speak first of the position of the fœtus
full term of pregnancy, and the mode in which it is *maintained*
lex action; secondly, we shall inquire how and when reflex
enables the fœtus to *assume* this position; and, lastly, we
ffer a few observations on the appropriate stimuli calling the
motions of the fœtus into action, the parts of its surface most
tible of the external impressions leading to its reflex motions,
e period of life at which reflex muscular motions are first
rincipally remarked.

Mode of Maintenance of the Position of the Fœtus towards the end of Pregnancy.

and, as we have already seen, for some time before parturi-
he human fœtus is rolled up into an ovoid-formed mass, with
phalic extremity placed lowest or over the os uteri. When
laced, it stands, so to speak, upon its head, when the mother
the erect posture. To comprehend the mode in which this
ar position is maintained during the latter period of utero-
ion, it is necessary to attend to the relative shapes or forms of
erus and of the fœtus at that period. For it is the relation in
of the fœtus to the uterus—of the contained to the containing
-that regulates this position. And the maintenance and re-
on of the position is effected by reflex movements, when its
uance is from time to time threatened to be interrupted by
nents of the mother's body, or by other circumstances affect-
e conditions and relations of either the uterus or infant.

form of the uterus at the full term of utero-gestation is
See the outline of it from Hunter's most accurate drawing,
ure 4. "The general figure of the uterus at this time is,"
r. Hunter,[2] "oviform; the fundus answering to the largest
nity of the egg, and the cervix and os uteri to the small end;
e fundus is larger and more flat, or less pointed, in proportion
lower extremity of the uterus, than one end of an egg is to
her; and the whole uterus seems more or less compressed, so

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[1] From Edinburgh Monthly Journal, April, 1849, p. 639.

[2] Anatomical Description of the Human Gravid Uterus, p. 3.

as to be broader from right to left than it is from the forepart backwards.

In the pregnant uterus represented in Figure 3, and belonging to Professor Goodsir, the whole length of the cavity of the organ is twelve inches and a half. The broadest part of the cavity is four and a half inches from the fundus, where it measures eight inches across. From this point the organ gradually diminishes in breadth, and tapers downwards towards the cervix. Across the cervix, about three inches above the os, it is about four inches in breadth.

Fig. 4.

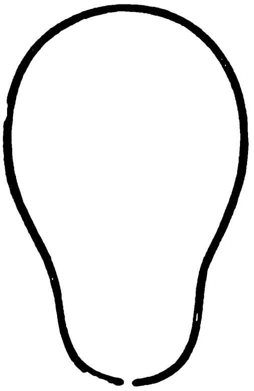
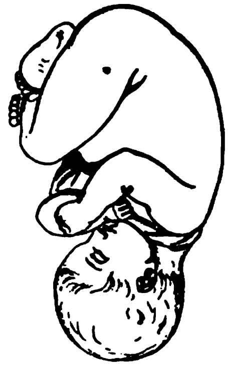


Fig. 5.



The form of the fœtus as it lies rolled up in utero at the full term of pregnancy, is ovoid, like that of the uterus itself. "The fœtus," says Velpeau,[1] "forms an ovoid mass, whose large end, turned towards the fundus of the uterus, is represented by the pelvic extremity of the trunk, and the summit (small end of the ovoid) is represented by the cephalic extremity, which rests upon the cervix of the uterus." In Mr. Goodsir's preparation, the broadest part of the larger or pelvic end of the ovoid mass of the fœtus measured nearly eight inches, and ran across in a line from the lumbar region of the child nearly to the point where the sole of the foot was applied to the placental surface. The breadth of the smaller or cephalic end of the ovoid (formed by the occipito-frontal diameter of the head) measured about four inches. In other words, the lower end was nearly a half narrower than the upper and broader end of the fœtal ovoid.

Figures 4 and 5 represent in outline the ovoid form of the uterus, and the ovoid form of the fœtus, at the full term of pregnancy, according to the preceding description of their relative shape and configuration at that time.

Towards the end of pregnancy, the fœtus is situated in the uterus, so that the two ovoids which the fœtus and uterus form are relatively adapted to each other; that is, the broad or pelvic end of the ovoid of the fœtus is placed towards the broad or upper end of the ovoid of the uterus; while the narrow or cephalic end of the ovoid of the fœtus is placed towards the narrow or lower end of the ovoid of the uterus. The narrow and broad ends of the ovoid mass of the fœtus are relatively adapted to the narrow and broad ends of the ovoid cavity of the uterus. The figure of the contained body (the fœtus) thus comes to correspond with the figure of the containing body (the cavity of the uterus), and is, as it were, fitted into it. Figure 6 represents the one ovoid (Fig. 4) thus adapted to the other ovoid (Fig. 5).

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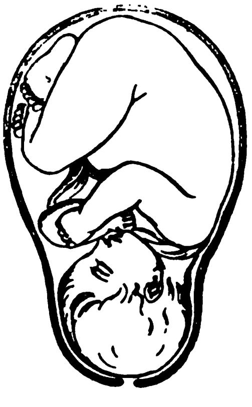
[1] Traité des Accouchemens, tom. i. p. 333.

At the advanced time of pregnancy of which we are speaking, when the uterus and fœtus are normal in shape and size, the fœtus cannot be placed transversely in the uterine cavity, so as to form a transverse or arm presentation, as there is not room for it to lie in that position. For the long axis of the ovoid mass of the fœtus is about twelve inches; the greatest breadth of the uterus is only eight inches: so that the latter could not receive the former. For the same reason, when once the position with the head downwards is assumed, it comes to be difficult or impossible for the child near the full time to move round in utero, so as to turn its pelvic, instead of its cephalic extremity downwards, and towards the os. In fact, a compressible body of twelve inches in length, such as the rolled-up fœtus is, cannot readily move through a space of only eight or nine inches, even though that space, like the transverse axis of the uterine cavity, be capable of dilatation and forcible alteration of shape.

At and towards the full term of utero-gestation, the position of the fœtus with the head lowest is thus greatly maintained by the relative *physical* adaptation of the ovoid shape of the rolled-up mass of the fœtus, to the ovoid shape of the interior of the cavity of the uterus. But this particular adaptation and position of the fœtus in the uterine cavity would be often lost if no other additional and *vital* means were in operation; as we see indeed often happen when the child dies. The other additional vital means, by whose influence this special position is still further rigorously and carefully sustained, consists of the restoring influence of reflex motions on the part of the fœtus itself. For, let the relative adaptation and position of the fœtus be partially disturbed (as readily happens by any movements of the mother, as, for

Fig. 6.



example, by her suddenly assuming the upright or supine postures, or other such causes), and immediately the fœtus, by a series of reflex movements of its extremities, particularly of its lower extremities, acts till it regains its former perfect position. It moves, in other words, till it has retaken that position in which it is most free from any marked excitations or compressions upon the external surface of its own body.

It is perhaps unnecessary to add, that seeing the fœtus is placed in the liquor amnii, a fluid medium of high specific gravity, and is consequently subjected to a uniform amount on all sides of hydrostatic pressure (considerably neutralizing the force of gravity upon its mass), its existing position is easily altered by changes of position and motions on the part of the mother, and consequently on the part of the containing body—the uterus. But for exactly the same physical reason by which the position of the fœtus is thus rendered easily capable of change and alteration, the physical powers and means necessary for the restoration of it to its proper position are correspondingly simple. Floating as it does in a liquid medium, the motions of its extremities in that medium, and against the uterine walls, are easily and perfectly adequate as a mechanical means of motion, to enable it to swim back to, and resume that normal and adaptive position in which it lies most free from extraneous excitations, and from the irritation of disturbing impressions or stimuli upon its own surface. In some mothers the position is apparently easily maintained; the infant and uterus are perfectly adapted to support each other, and consequently the child moves little. In other mothers, or in the same mother in other pregnancies, the movements are nearly incessant. In other cases in which the movements of the infant are great and continuous, the umbilical cord is generally found long, and hence liable to compression; or the cavity of the uterus is large and distended from an excess of liquor amnii, and almost constant movements are required on the part of the fœtus to prevent its almost constant tendency to displacement or compression. The nearly uninterrupted constancy of the fœtal movements in some mothers exists to a degree greater than is observed in any voluntary muscular movements. But the excess of fœtal movements corresponds in this respect with the law of constancy, which regulates other corresponding excito-motory movements in the animal economy. Voluntary muscular movements always become temporarily arrested and interrupted from time to time; because, from time to time, volition, like other cerebral acts, must temporarily cease for the purpose of rest and renovation. Fatigue, and its antidotes repose and sleep, necessarily belong to the cerebral system; but the spinal system never sleeps, and the reflex or excito-motory muscular movements over which it presides, are not susceptible of fatigue, and do not necessarily require any intermission in their action. It is on this principle that respiration in man is unattended by fatigue, and that "the long flight of birds in their migrations, and of flies, which are all the day long on the wing, is sustained."[1] Some of the reflex or excito-motory movements of animals which show this law of constancy, have the same object and effect as the reflex movements of the fœtus, viz., the maintenance of position. Thus, the fish holds and maintains its position in the stream by constant reflex movements of the fins and tails.

2.—*Mode of Assumption of the Natural Position of the Fœtus with the Head downwards.*

I have already stated that the position of the fœtus, with the head over the os uteri, is not observed till the latter months of utero-gestation; that in fœtuses expelled before the end of the sixth month, presentations of the feet or pelvis are as frequent as presentations of the head, and presentations of the shoulder by no means unfrequent; and that the position of the head lowest and towards the os uteri comes to be taken with more and more certainty from the end of the sixth month, onward to the full term.

From the observations collected by Professor Dubois at the Maternity Hospital of Paris, I have constructed the following table in evidence of the preceding remarks. The respective results of the presentations among children born before the end of the sixth month, during the course of the seventh month, and at the full time, include, it will be observed, all cases, whether the child was alive or dead at the time of its expulsion. The return for the eighth and ninth months refers only to children born dead. He does not give any return relative to the children born alive during these two months.

Table of the Relative Proportion of Head Presentations, as varied by the date of the Pregnancy or Labor.

Period of Pregnancy.	Total Cases.	Presentations of			Percentage of Head Presentations.
		Shoulder.	Breech.	Head.	
Before end of sixth month,	121	5	51	65	52 in 100
During seventh month,	119	6	31	82	68 in 100
During eighth and ninth months,	96	2	22	72	76 in 100
At full term of gestation,	100	1	3	96	96 in 100

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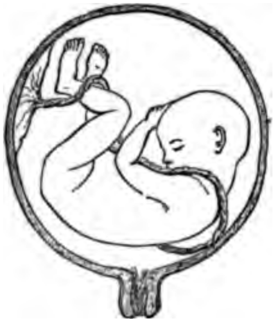
[1] Hall, Diseases of the Nervous System, p. 26: "I suspect, indeed, that the migratory traveller is frequently actually visited by nature's sweet rest, even during its aerial transit," New Memoir, p. 32. The young infant often evidently continues the act of sucking, an act of the spinal or reflex system, while the cerebral system is asleep.

The facts in this table sufficiently prove that the position of the fœtus with the head lowest and over the os uteri, does not begin to be assumed till about the end of the sixth month, and that it is taken up with increasing frequency and certainty from that period onward to the full term of pregnancy.

The double fact of the position not being assumed before the period in question, and of its being more and more positively and steadily assumed after this period, is simply, I believe, ascribable to the circumstance, that before the sixth month there is no adjusting correspondence of figure between the uterus and fœtus; while, after the sixth month, the figure of the one comes to correspond more and more to the figure of the other, and consequently requires more and more the contained body to be placed in the containing cavity in that kind of relative adaptation which has been already described.

Fig. 7.



All anatomical authorities allow that the uterine cavity before the sixth month[1] is spheroidal, and not ovoid, the neck of the organ not yet being developed and taken up to form part of the cavity. The fœtus, up to this time, is not folded up so compactly as afterwards, or moulded into its ultimate ovoid form. Further, at the same period, the uterine cavity is not only roundish in form, but is relatively much larger in regard to the fœtus—for it contains a large quantity of liquor amnii.[2] After the sixth month these various circumstances are gradually altered. The cavity of the uterus becomes more and more ovoid in form, from the development of the cervix; the quantity of liquor amnii becomes less and less, relatively to the increasing size of the uterus and infant; the infant itself becomes in a greater and greater corresponding degree deprived of space—is forced to alter its shape and position according to the shape and form of the uterine cavity—becomes more folded and compacted together because its containing space is less—and ultimately ecomes comparatively fixed in the position which it is thus forced to sume. "In the last two or three months of gestation, the child," bserves Dr. Hunter,[1] "is commonly so much straitened for room, nd so compactly adapted to the oblong figure of the uterus, that it annot change its general position either by its own efforts, or even y accidents happening to the mother." "When," he elsewhere emarks,[2] "there is a considerable quantity of liquor amnii, the child akes the advantage of room, and the composition of its parts is not o close or globular. In proportion as there is less room, its figure s more compacted and moulded to the shape of the cavity of the terus."

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[1] See Fig. 7, sketched and drawn from a case of five or six months' pregnancy delineated by Breschet and Velpeau.

[2] "The relative proportions," says Ramsbotham, "between the quantity of fluid and size of the embryo, differs much at different stages of pregnancy, being considerably greater at the early periods, and less at the advanced stage. Thus when the embryo is scarcely visible to the naked eye, there is from half a drachm to a drachm of water collected within the membranes. Where the embryo is not so large as a small kidney bean, there would be an ounce or more of the liquor amnii; while at the end of gestation, when the fœtus weighs on an average, nearly seven pounds, the amount of fluid seldom exceeds a pint. The quantity, therefore, though positively increasing with the growth of the ovum throughout the whole of gestation, is relatively to the size of the fœtus gradually diminishing."—Obstetric Medicine and Surgery, p. 69.

These changes, however, in the physical shape and size of the terine cavity, relatively to the physical form of the fœtus, would not produce of themselves the position of the head over the os uteri. The dead child does not take this position. If the child has been alive up to the seventh, or eighth, or ninth months, and has in consequence assumed the position in question, but then dies—the position is liable to be lost from the death of the child. I have already given two tables proving this fact, and showing how much the assumption, as well as the maintenance of the position, with the head downwards, is a vital act, and connected with the existence of life in the child. The regulating vital power guiding it to the assumption of that normal position in which its figure corresponds as exactly as possible to the figure of the uterine cavity, consists of a succession of reflex or excito-motory movements of an adaptive kind on the part of the fœtus, excited by impressions made on its external surface. In order fully to understand their action, we shall next briefly consider the origin and effects of those adaptive reflex movements, which thus contribute not only to the original assumption, but to the continued maintenance of the position of the fœtus —and force it to take up, retain, and restore itself to, that special position in the uterine cavity in which it can lie with its form more perfectly adjusted than in any other position, to the form of the uterus, and with its own excitor surfaces more removed than in any other position from the action of external impressions and stimuli.

3.—*Nature of the Physical Impressions, or Stimuli, exciting Reflex Movements on the part of the Fœtus.*

"All animals," says Harvey, "while they are at rest or asleep, fold up their limbs in such a way as to form an oval or globular figure. . . . . So, too, the infant in utero is generally disposed after this manner. In such a position is the embryo usually found, as that which we naturally take in sleep."[1] This folded-up state of the limbs and body of the fœtus is primarily the effect of the well-known greater muscular tone and contraction of the flexor than of the extensor muscles. Of the power and preponderance of the flexor over the extensor set of muscles, we have evidence remaining for some time after birth, in the almost constant state of flexion which may be observed in the extremities, and particularly in the lower extremities of the new-born child, when unirritated and at rest. It is not in general for some considerable time after birth that the child acquires the full power of extending the lower extremities. And no doubt, this superior tone and contraction of the flexor muscles is one of the means by which the necessary ovoid form of the fœtus in utero is maintained. But while this physiological principle[2] accounts for that form and figure of the fœtus which best adapts it for the form and figure of the uterus, we must have recourse to the acts of the reflex system in another view, in order to account for the adaptive position, or rather adaptive movements of the fœtus. These movements are reflex. And it is laid down as an established physiological law, that "the reflex nerves are in every case, without an exception, excited to action by the impressions of *physical* agents acting on the peripheral extremities of incident nerves;"[3] and "that the reflex power is never exercised without the excitement caused by the application of a physical agent either to the external or internal *surfaces* of the body."[4]

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[1] Anatomical Description of the Gravid Uterus, p. 67.

[2] Ibid. p. 63.

In relation to the movements of the fœtus in utero, let us next, then, inquire what physical agents produce the required excitor impressions, and on what surface, or the peripheral extremities of what incident nerves, these impressions act?

Various physiologists have pointed out that, when in the lower animals the head is removed, or the spinal cord is divided, and sensation thus abolished, the mere touching or irritation of the *skin* is followed by more or less complex movements in the *muscles* of the extremities and trunk. Whytt, for example, long ago remarked "that after decapitation, frogs and serpents performed movements of the limbs or entire body, consequent upon the application of a stimulus to the skin."[5] "In the frog," says Mr. Grainger,[6] "in the rabbit, kitten, &c., after all feeling and volition is lost, the *limbs* are moved when the *skin* is touched." The movements thus excited under these conditions, decapitation and division of the cord, are evidently reflex or excito-motory, and the facts prove how powerful an excitant surface *the skin* is, and how easily irritations of it act in calling up or producing reflex muscular movements. "No part," says Müller, "equals the skin in the property of exciting reflex motions; the slightest touch applied to the surface, in animals in a state of narcotization, is frequently sufficient to give rise to strong spasms, while the reflex actions excited by irritation of the nerves themselves are much slighter."[1]

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[1] Harvey's Works, Sydenham Edition, p. 522.

[2] Dr. Hall, it is well known, holds that the reflex function, in its natural state, constitutes the principle of equilibrium and tone of the whole muscular system.—Memoirs on the Nervous System, 1837, p. 38.

[3] Grainger's Observations on the Structure and Functions of the Spinal Cord, p. 50.

[4] Ibid. pp. 119, 129.

[5] Ibid. p. 3.

[6] Ibid. p. 49.—"Every physiologist," says Mr. Grainger, "who has divided the spinal cord in living animals, and has then pricked or otherwise irritated the *skin*, has remarked the free motion which is thereby caused in the muscles which are called voluntary."—P. 100. When the spinal cord is divided, "the foot of the rabbit is not only drawn away from

I have already stated that movements of the limbs of the child are readily produced *after* the period of its birth, by irritation of the sole of the foot and other parts of the cutaneous surface; that *during* birth, and while labor is going on, similar movements can be produced by similarly irritating the presenting part of the child; that *before* birth, or, in other words, during pregnancy, the same kind of movements is excited by irritating the limbs or body of the fœtus through the abdominal or uterine walls by the hand, stethoscope, &c.; and that in all these cases the movements thus produced are reflex or excito-motory, and not voluntary, inasmuch as they occur in anencephalous fœtuses, entirely wanting a brain, as distinctly as in those that are normal in structure.[2]

Cutaneous impressions, or physical irritations applied to the peripheral extremities of the incident nerves distributed to the surface of the skin, are thus capable of producing reflex-motory actions on the part of the infant, and are, perhaps, the most common cause of excitement of the muscular movements of its extremities, and consequently of its changes of position. If the fœtus be temporarily

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the irritation, but the whole limb, occasionally even both legs, are thrown backwards, with all the complicated motions that occur when the creature runs. In the frog, if the cord be divided sufficiently high, both hind legs often strike out when one is touched, not in the manner of the rabbit, but with the very action that occurs in swimming. The chicken, in the experiment of Flourens, when deprived of all volition, flew when thrown into the air. Lastly, in the human body, the legs may be incited to motion, when all voluntary power is destroyed, by tickling the sole of the foot. It is thus proved, beyond the possibility of a doubt, that motions similar to those performed in the progression of the animal may be excited by touching *the skin*, when volition and sensation are destroyed."—P. 150.

[1] Müller's Physiology, Baly's Translation, vol. i. p. 322; see also Longet, Anatomie et Physiologie du Système Nerveux, tom. i. p. 318.

[2] Unzer, whose remarkable works contain such clear and comprehensive views, in many points of the physiology of the nervous system, makes a remark upon this subject which is too striking to omit in evidence. "A living anencephalous monster, when born, draws itself up, if you prick or burn its limbs, and that plainly through a reflex operation of the nerves—[durch eine blosse Nervenwirkungzurück]—as a natural new-born child would do, if you made the same external impressions upon it."—Erste Gründe einer Physiologie, p. 552.

thrown out of position by any movements on the part of the mother and if the surface of its body thus become irritated at any point or points by mere pressure from or against the opposing uterine parietes, or other resistant substances, this pressure, like the pressure of the hand or stethoscope, alluded to in the preceding paragraph, will act as an excitor stimulus, calling up reflex motions calculated and empowered to move away the irritated and compressed limb or body from the agent or source of the irritation. Different postures of the mother, when suddenly assumed, by both displacing the fœtus and altering the shape of the uterus so as to make its parietes press upon the fœtus, are followed for a time by a succession of movements on the part of the infant.[1] Thus the acts of suddenly rising or suddenly lying down excite almost constantly, in certain women, motions on the part of the child. When some mothers stoop or lean forward, they alter the shape of the uterus, and compress the child so greatly as to call up instantly such strong fœtal motions as to oblige them immediately to assume the erect posture. And the fœtus sometimes throws itself into positions which excite new movements on its own part. In the latter months, "it is frequently evident," says Dr. Hunter,[2] "that some awkward straightened position of the child being produced by its own motion, and pressing or stretching the uterus unequally, gives the mother much uneasiness for a time; and then, upon the child's stirring in some remarkable way, it gets to be more at its ease, and the mother feels instant relief from the pain, the stretching, or whatever the complaint was. This will happen to the same woman again and again."

In the various cases and conditions which I have mentioned,[3] the

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[1] "That the different attitudes of the mother's body should produce alterations in the figure of the uterus, needs not now a particular explanation or proof. The weight of the uterus itself, and of the adjacent viscera, being differently directed, must produce some change of posture, especially as the parts against which the uterus will rest its weight in the different postures of the body are of such different natures."—Hunter's Anatomical Description of the Gravid Uterus, p. 4.

[2] Anatomical Description of the Gravid Uterus, p. 67.

[3] All the conditions I have spoken of in the text, relate to cutaneous irritations and impressions acting as stimuli to reflex motions. In cases of prolapsus of the umbilical cord, compression of the prolapsed funis by the fingers or otherwise, seems generally to lead to a series of reflex movements of the body and limbs of the fœtus. During pregnancy this may be one of the means employed by nature to prevent, more frequently than otherwise would happen, the position of the cord between the child's head and the interior of the uterine cervix. It is certainly remarkable that we do not meet with this position of the cord more frequently, when we consider its length and its greater specific gravity than the liquor amnii. If, when so placed, and consequently compressed, reflex fœtal movements were always excited, this circumstance may possibly be the means of leading to, or producing, one other result, viz., the ultimate course and twisting of the cord around the body, neck, or limbs of the fœtus; in other words, such convolution of the cord around these parts

cutaneous impressions and stimulations call up reflex *motions* in the fœtus, according to the laws laid down by all those physiologists who have written upon the subject of excito-motory phenomena. "An impression is made," to use the language of Dr. Marshall Hall, "upon the extremity of a nerve, or nerves; this impression is conveyed not to the cerebrum, but to some part of the medulla oblongata or medulla spinalis, whence it is reflected upon certain muscles destined to be excited into consentaneous action."[1] "The external impressions," as Prochaska[2] writes it, "that are made upon the sensorial nerves are most swiftly propagated along the whole length of the nerves to their origin, where, when they have arrived, they are reflected according to a certain law, and pass on to certain and corresponding motor nerves, through which, being again very swiftly propagated to muscles, they excite certain and determinate movements." The views which Prochaska took of the general object and aim of such reflex or excito-motory movements are precisely those which are attained by the aforesaid movements of the fœtus in utero. "The general law," he observes, "by which the common sensorium reflects sensorial impressions into motory, is the preservation of the individual (*conservatio nostri*); so that when external impressions act that are hurtful to our body, there follow certain motory impressions producing motions, combined for the purpose of removing and driving away this irritation from our body; and, on the contrary, when external or sensory impressions act that are grateful to us, there follow internal or motory impressions productive of motions, having for their object the further maintenance of this grateful condition."[3]

All the so-called voluntary or motory muscles of the fœtus are not excited to an equal degree, or with equal facility, by the action of the physical impressions or stimuli to which it is subjected. The muscles of the trunk and upper extremities are much less moved under these stimuli than the muscles of the lower extremities are. Various authors have remarked incidentally, and without a view to any theory, that apparently the lower extremities move in utero much more than the upper.[4] Indeed, for some time after birth the movements of the legs continue to be greater in degree and extent than the movements of the arms. In the fluid medium in which it lies and swims in the uterine cavity, the motions of the lower extremities of the

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may be the result of sanitary reflex movements on the part of the fœtus—these sanitary reflex movements originating in, or being excited by, some unsafe or uneasy compression of the cord itself.

[1] Memoirs on the Nervous System, p. 70.

[2] Annotationes Academicæ, fasc. iii. p. 114.

[3] Ibid. p. 117.

[4] Thus, for example, Professor Dubois observes, "Il ne faut pas oublier que les grands mouvements du fœtus, et les plus ordinaires sont ceux qu'executent les extrémités abdominales."—Mémoires de l'Academie, tom. ii. p. 285.

fœtus are more useful and powerful in enabling it to maintain and resume its position than any movements of the upper extremities could be. And even in the adult, when the influence of the mind and cerebral system is withdrawn, reflex movements are far more easily excited as in the fœtus, in the lower than in the upper extremities. When a man receives an injury, producing fracture or displacement of one or more of his cervical vertebræ, and paraplegia in consequence is produced, "we find," says Dr. Todd, "the patient presenting the following phenomena. His trunk and extremities appear as if dead (except the movements of the diaphragm), while the head lives. In full possession of his mental faculties and powers, he is nevertheless unconscious, save from the exercise of his sight, of any changes which may affect the parts below his head, nor is the utmost efforts of his will sufficient to produce a movement of any, even the smallest, of these parts. If the stunning effect of the accident have passed off, tickling the soles of the feet will be found to cause movements, of which, as well as of the application of the stimulus, the patient is unconscious. The limbs may be irritated in various ways, but without exciting any effects which the patient can perceive, excepting movements, and these he is aware of only from his happening to see them. It is *important*," adds Dr. Todd, "to notice that, in cases of this kind, movements are difficult of excitation in the *upper* extremities, *while they are aroused with great facility in the lower.*[1]

4.—*Are there any parts of the Cutaneous Surface peculiarly susceptible of the appropriate physical impressions or stimuli leading to the Reflex Motions of the Fœtus?*

In the fœtus after birth, some parts of the cutaneous surface, when stimulated, give rise much more readily than others to reflex muscular motions. In the adult, the skin of the soles of the feet, of the knees, and of the sides, are thus specially susceptible. In other words, these parts are more sensitive than other parts to the irritation of touching or tickling. This over-susceptibility in the cutaneous nerves of these special parts is of no apparent use in the adult; and yet there is no property without its utility. It becomes generally more and more diminished and lost as life advances, and is greater in degree the younger the person, and greatest of all in the infant. Has this property not *its* special utility in the infant before birth? The parts of the fœtal surface most exposed to external irritation, the soles of the feet, knees, and sides, as seen in the woodcuts, are exactly those parts which are super-sensitive after birth to cutaneous irritation, and which most readily produce reflex move-ents of the limbs, in consequence of their cutaneous irritation. In new-born infant, irritation of the sides of the chest is followed by more or less strong movements of both lower extremities. Rubbing r tickling the soles of the feet immediately excites a movement or change of position and retraction. "Certain localities," says Dr. Hall, "are more susceptible than others to the effects of the excito-motory stimulus—the sole of the foot is especially one of these. Dr. Little," he continues, "has published an interesting case of distortion of the foot, which only, but uniformly, occurred when it was placed upon the ground. Dr. Dieffenbach met with a similar case."[1]

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[1] Physiology of the Nervous System (Cyclopædia of Anatomy), p. 19. See also Todd and Bowman's Physiological Anatomy, vol. i. p. 334.

In anencephalous infants the same special parts of the cutaneous surface are principally or solely those, through the irritation of which, muscular movements are excited, when such infants survive after birth. In his Croonian Lecture on Muscular Motion, Sir Gilbert Blane describes some experiments upon a kitten which he decapitated, and upon another in which he divided the cord; and he states that after these injuries of the nervous centres, when the hind paws were irritated, "the muscles belonging to the posterior extremities were thrown into contraction, so as to produce the motion of shrinking from injury." He subsequently adds, "In an acephalous monster the like phenomena were observable. It moved up its knees when the *soles* of its feet were tickled," &c.

The results of pathology strikingly prove, that there are some portions of the cutaneous surface which are much more susceptible than others, of producing reflex muscular movements, under the application of appropriate excitants. This is particularly seen in some cases of complete paraplegia depending on disease or division of the spinal cord, and where all power of sensation and volition is abolished, as far as the paralyzed limbs are concerned. In instances of this kind, described by Drs. Hall, Barlow, Budd, and others, the muscles of the paralyzed limb, though in no way obedient to volition, were capable of being excited into temporary action by various irritations of the cutaneous surface of the limb, but particularly by tickling the sole of the foot.[3] Thus, in a case described by Mr. Grainger, "a girl about fifteen years of age was affected with angular curvature of the spine, producing insensibility and paralysis of the lower extremities. On tickling the *soles* of her feet, which, as an experiment, was often done, the legs were immediately slightly retracted, although the patient said she felt nothing; it was further remarked, that on touching the *other* parts of the feet or the legs in the same manner, no effect was produced."[1] "The results," observes Mr. Grainger, "noticed in these cases are full of interest. They prove, first, that in parts of the body indisputably deprived of all feeling, and power of voluntary motion, contraction may be excited in the so-called voluntary muscles, by impressions made on the skin; secondly, that this capacity of exciting muscular contractions, is not equally possessed by all parts of the external surface of the body, but that the *sole* of the foot is that precise part, in which the action is excited in the most energetic manner."[2]

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[1] Diseases and Derangements of the Nervous System, p. 117.

[2] Select Dissertations on Medical Science, p. 262.

[3] See Diseases of the Nervous System, p. 233, &c. &c.

In cases of hemiplegia, particularly when the paralysis is complete, the same phenomena may be frequently observed. "In such cases," observes Dr. Todd, "it is wonderful how easily movements may be excited in the palsied leg—very rarely in the arm—by the application of stimuli to the *sole* of the foot, or elsewhere with less facility. The patient, who has acknowledged his utter inability to move even one of his toes, is astonished at the rapidity and extent to which the *whole* lower extremity may be moved by *touching* the sole of the foot, even with a feather."[3] The development of these excito-motory phenomena in paralytic cases is frequently, adds Dr. Todd, "in the inverse proportion of the withdrawal of the power of the will."[4] And in the same way, and on the same principle, the *excito-motory reflex* muscular movements of the fœtus are probably only the more marked in degree and extent, in consequence of the absence during intra-uterine life, of all volitional and other mental influences. This remark leads us to a new subject, viz.:

5.—*The Degree of Reflex or Excito-Motory Movements manifested by the Fœtus in Utero, and the Period of their Commencement.*

In adult life the influence of, and susceptibility to, reflex movements in the so-called voluntary muscles of the trunk and extremities, is masked by the predominating power and effects of the mind and cerebral system over these same sets of muscles. Their excito-motory activity, however, is only obscured, not obliterated. Disease, by sometimes separating the influence of the cerebral system from the influence of the spinal system over these muscles, proves to us the persistence of their excito-motory activity, as in the cases of paraplegia and hemiplegia, adverted to in the two preceding paragraphs. These cases show us by a kind of pathological analysis, at even in the adult, when the motory muscles of the trunk and tremities are withdrawn from the psychical nervous influence of e cerebral system, they are still found remaining subject to the hysical nervous influence of the reflex or spinal system. "The ruth," remarks Dr. Hall, "is that the intellectual functions are daily developed during the first years of life, and obscure those of the excito-motory; but the latter are not enfeebled during this change, which is one of superaddition, not of substitution."[1]

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[1] Observations on the Spinal Cord, p. 94.

[2] Ibid. p. 94. See also Dr. Budd's paper in Med. Chir. Trans. vol. xxii. p. 187: "Impressions on the *soles* of the feet were more efficient than any that were tried on other parts of the skin."

[3] Physiology of the Nervous System, p. 19.

[4] See also Dr. Budd, in Medico-Chir. Trans. vol. xxii. p. 186: "The reflex movements varied in extent and force, inversely with the degree of voluntary power."

All modern physiologists, however, admit that reflex or excito-motory movements are more distinctly and more powerfully developed, and are intrusted with the performance of more and more important functions in the animal economy, according as we descend in the scale of animal life, and consequently also in the scale of individual life. In other words, the younger the individual is, the more evident and marked, as a general rule, is the power and influence of the reflex movements. "It is worthy," says Dr. Hall,[2] "of special observation, that, in the very young animal, and in the cold-blooded animal, the phenomena of the excito-motor power are far more vividly manifested than in the older and the warm-blooded. In the very young kitten, even when asphyxiated to insensibility, every touch, contact, or slight blow—every jar of the table, any sudden impression of the external air, or that of a few drops of cold water, induces at once energetic reflex movements and acts of inspiration. The nostrils, the tail, the soles of the feet, the general surface, are all extremely susceptible, and, in degree, in the order I have mentioned." "Phenomena of this nature," observes Professor Todd, "may be produced in all vertebrate animals. They are, however, especially marked in the cold-blooded classes. In the young of warm-blooded animals they are more manifest than in the adults of the same class."[3]

In accordance with the principle stated in the preceding paragraph, we ought to expect the manifestations of reflex movements to be well marked in the fœtus. And the whole of the present inquiry is intended to show that they are so, in particular sets of muscles, viz., in those connected with the general movements of the body. Other sets and combinations of muscles subject to excito-motory action after birth, as those of inspiration, suction, deglutition, defecation, &c. &c., are still in the state of absolute quietude during intra-uterine life, because the appropriate physical stimuli calculated to excite these reflex movements, are never applied till extra-uterine life is commenced, and because their action during intra-uterine life would be dangerous and even fatal to the fœ On the other hand, we have seen that the appropriate stimuli, c ing into exercise the motory muscles of the trunk and extremit are applied during intra-uterine existence; and the action of th muscles is necessary on the part of the fœtus, in order to enabl to assume and maintain its position with the head downwards, action which is in itself characterized by being intimately connec as other excito-motory actions are, "with the preservation of individual, and the propagation of the species."[1]

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[1] Diseases and Derangements of the Nervous System, p. 138.

[2] New Memoir on the Nervous System, p. 29.

[3] Physiology of the Nervous System, p. 18.

In several parts of his works, Dr. Hall speaks of the extent of phenomena referable to the nervous system that are manifeste fœtal life. But in consequence of not adverting to some of points which have been attempted to be stated in the preced pages, he takes a too limited, and consequently, as I am incline believe, an erroneous view of the extent of the excito-motory reflex muscular movements and actions that occur during in uterine life. "The fœtus in utero seems," says he, "restraine a sort of ganglionic life. Everything consists in nutrition growth, or development. The contact of the liquor amnii is s cient to keep the eyelids, the lips, and the sphincters close "The fœtus in utero is," he again observes, "so little expose the influence of stimulants, or excitants applied to the cutaneou mucous surfaces, that the excito-motory property is comparati little called into action in the form of the reflex function. contact of the liquor amnii may preserve the lips or the larynx, the sphincter ani, closed. In this manner, the reflex function, or function of exclusion and of retention, is in activity; but, as agent in ingestion and egestion, it is as if it did not exist."[3]

In these, and the other passages in which he alludes to the s of the nervous system of the fœtus, Dr. Hall nowhere, so far as I aware, adverts or alludes to the strong and well-marked re movements and actions performed during intra-uterine life by motory muscles of the trunk, and particularly of the limbs of fœtus, and which constitute, as I have endeavored to show, mechanism by whose instrumentality nature ultimately effects produces in the human uterus, the normal and salutary positio the child with the head downwards and over the os. At w period of intra-uterine life reflex movements first commence these so-called voluntary muscles, we have no power of preci determining. The exercise of these reflex movements is not essential for the principal object which they seem intended to accomplish (namely, the proper position of the fœtus), till the latter period of pregnancy. But the fœtus is usually felt by the mother moving as early as the middle term of utero-gestation; and stethoscopic observations show that they are often present to some extent before they come to be perceived at the time of quickening. "During the early stage of pregnancy," says Naegele, "while the fœtus is very small in proportion to the size of the cavity which contains it, and while the free movements of its limbs are consequently unrestrained, sounds produced by the movements of the fœtus may be occasionally distinguished as gentle taps repeated at intervals, and continued uninterruptedly for a considerable time. These sounds may sometimes be distinguished *several weeks before* the mother becomes conscious of the motion of the child, and also earlier than the pulsations of the fœtal heart, or the uterine souffle."[1]

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[1] The "functions" of the "true spinal excito-motory system" are, as repeatedly expl and laid down by Dr. M. Hall, those functions of the economy "on which depend— preservation of the individual; and, 2, the continuance of the species."—Diseases o Nervous System, p. 39, &c.; New Memoir, p. 51, &c.

[2] Diseases and Derangements of the Nervous System, p. 131.

[3] Ibid. p. 243; see also p. 113, &c.

In comparative physiology, we may have even *ocular* proof of the early commencement of reflex or excito-motory movements in the limbs, &c., of the embryo. In marsupial generation, the embryo, as is well known, leaves the uterus very early, and becomes immediately affixed to the nipple protruding in the interior of the marsupial sac, where it continues its development and growth. In the kangaroo, the embryo is expelled from the uterus as early, according to the observations made by Professor Owen, as the thirty-ninth day. An embryo of the kangaroo expelled at that period was watched and examined by Mr. Owen, and to use his own words, it "resembled an earth-worm in its color and semi-transparent integument," and, in the degree of development, its brain corresponded to that of the human embryo at the ninth week. Yet, in this early stage of development, the existence in it of reflex actions and movements was most distinct, for it breathed, *moved its limbs when touched*, and fixed upon the marsupial nipple. "It adhered freely," says Mr. Owen, "to the point of the nipple; breathed strongly but slowly, and moved its fore legs when disturbed.'

SECTION IV.—MALPRESENTATIONS OF THE FŒTUS; THEIR CAUSES AND MODES OF PRODUCTION.[3]

Hitherto we have spoken only of the usual and common position of the fœtus with the head downwards, and presenting over the os uteri. But other parts of the fœtus than its head, sometimes present over the uterine orifice in the latter months of utero-gestation, and during labor. When any other region than the head of the fœtus presents, the resulting "malpresentation," or "preternatural presentation" (as it is generally termed), is one of two kinds, viz., either the presentation of some part of the pelvic end of the ovoid mass of the fœtus, or the presentation of the lateral surface of the trunk, but particularly of the shoulder or arm. In other words, the full-grown infant, at the time of labor, is practically found to present over the os uteri in one of three different modes, namely,—1. With its head (or cephalic extremity); 2. With the nates, knees, feet (or pelvic extremity); or, 3. It may be situated transversely across the uterine cavity, and hence present a side, shoulder, or arm, at the uterine orifice. In a previous part (p. 90), I have already given a table which shows the relative proportion in which these three leading orders or genera of presentations of the fœtus are observed to occur in practice, as deducible from the extensive reports on the subject, published by Lachapelle, Boivin, Clarke, and Collins.

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[1] Treatise on Obstetric Auscultation, p. 50.

[2] Cyclopædia of Anatomy, article Marsupialia, vol. iii. p. 322.

[3] From Edin. Monthly Journal of Med. Science, July, 1849, p. 863.

In the preceding sections we have seen the difficulties with which all theories of the natural and common attitude of the fœtus with the head lowest, have heretofore been confessedly beset. And it can easily be conceived, that if the rationale of the cause of the natural position of the fœtus has been a matter of doubt and uncertainty, the explanation of any deviations of the fœtus from that natural position has necessarily been involved in still greater dubiety and difficulty.

Indeed, though most systematic writers in midwifery have theorized more or less fully upon the subject of the natural position of the fœtus, and its supposed cause or causes, few of them have, in any degree, entered into the consideration of the cause or causes of its malpositions or malpresentations; and of these few, none, so far as I am aware, have discussed the question at any length. They have vaguely referred to the subject rather than considered it. Those who believed that the child's head was turned downwards to the os uteri by a kind of somerset in the latter periods of pregnancy, or at the beginning of labor, have, when they mention the subject at all, usually contented themselves by simply attributing its malpositions to some derangement in this supposed mechanism.[1]

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[1] Thus Roederer, after speaking of the physical gravitation of the head downwards, adds —"Several causes, however, may prevent its descent, or derange its direction. I place in this class, defect of the liquor amnii, obliquity of the uterus, tumors in the uterus, premature efforts, and external violence."—L'Art des Accouchemens, p. 41.

"The cause," observes Ould, "why the feet sometimes present, is certainly from the body being in a more than ordinary erect posture in the womb, at the commencement of labor, whereby the compression of the parts designed for that purpose thrusts the body forward in

Other authors have mentioned one or more individual causes, as probably capable of producing malposition of the fœtus, but without attempting any generalization of the mechanism and operation of these causes.[1]

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a direct line, and consequently the feet against the orifice of the womb."—Treatise of Midwifery, Preface, p. 15.

The following is the only passage in his Memoir in which I find Professor Dubois referring to the probable cause or explanation of the malpositions or malpresentations of the fœtus:—"If," he remarks, "the ordinary relations of the fœtus to the uterus in the later periods of gestation are the result of its own efforts, and consequently of a spontaneous determination, this ought to have an internal cause, and internal sensation, which provokes it. What, then, is the nature of this cause—of this sensation? The abnormal situation of the infant in which the pelvic extremity corresponds to the small extremity of the ovum, might be uncomfortable, or painful even, for the fœtus; and the spontaneous movements by which it would change its position might be classed among those produced by a state of suffering;—or else each of the extremities of the trunk of the fœtus fitting better to the form of the ovum, when the pelvis of the fœtus lies superiorly, and the head inferiorly, might it be the easiness of this position which determines the fœtus to seek it, and return to it."—Mémoirs de l'Acad. Roy. de Méd. tom. ii. p. 264.

[1] For example, in speaking of this subject, Dr. Denman observes, "It seems doubtful whether we ought not to exclude accidents as the common causes of these presentations, and search for the real cause in some more intricate circumstances; such as the manner after which the ovum may pass out of the ovarium into the uterus; some peculiarity in the form of the cavity of the uterus, abdomen, or pelvis; in the quantity of the waters of the ovum at some certain time of pregnancy; in the circumvolution of the funis round the haunches or lower part of the back of the child; or perhaps in the insertion of the funis into the abdomen of the child, which is not in all cases confined to one precise part, but admits of considerable variety."—Introduction to Midwifery, p. 467.

"We know little," remarks Dr. Clark, "of the cause of preternatural labor; perhaps it depends upon a peculiarity of form, either in the uterus or pelvis. It is said to arise from accidents; but preternatural births are most likely the effects of peculiarity of shape in the parts."—London Practice of Midwifery, p. 328.

"It is not easy," says Spence, "to ascertain the cause of the different situations of children in the womb, as in some women it may be owing to too great an inactivity during pregnancy, in others to external violence or over-exertion of strength. Again, the want of a sufficient quantity of the liquor amnii, or the child being entangled by the umbilical cord, may occasion this."—System of Midwifery, p. 220.

"The causes," observes M. Chailly, "of presentations of the trunk, cannot be very well appreciated. But it is generally supposed that small size and mobility of the fœtus, and obliquity of the womb, may occasion them."—Treatise on Midwifery, Dr. Bedford's Translation, p. 443.

"The smallness and mobility of the fœtus," M. Cazeaux states, "obliquity of the uterus and of the passages of the pelvis, deformities of the pelvis, are generally considered as predisposing causes."—Traité des Accouchemens, p. 363.

Some authors, in expressing their opinions on the probable causes of the malpositions of the fœtus, admit one or more of these individual causes, but reject others. Thus Dr. Rigby, when discussing this point, observes—

"The question naturally suggests itself: by what means is the long diameter of the child, in so large a majority of cases, kept parallel with that of the uterus? This depends in a great measure upon the form and size of the uterus. Where the uterus is not unduly distended with liquor amnii, and where it preserves its natural oval figure, it is scarcely possible that the child should present in any other way than with its cephalic or pelvic extremity foremost. There can be no doubt that the first early contractions of the uterus, in the commencement of labor, have a great effect in regulating the position of the child. . . . . . We may state that the causes of arm or shoulder presentations are of two kinds, viz., where

And some of our latest and best systematic authors on midwifery have declared as their opinion, that all past suggestions on the subject have failed in affording any sufficient explanation of the phenomena. Thus in speaking of pelvic presentations, Dr. Tucker states, "no satisfactory reason has as yet been given in explanation for their occurrence."[1] When treating of transverse presentations, Dr. Churchill, in the same spirit, remarks, "I think all the explanations as yet offered are insufficient."[2] Dr. Ramsbotham conceives that there are not "any evident causes to which we can assign them."[3] "To explain why the head of the child does not invariably present in labor is," observes Dr. Lee,[4] "very difficult or impossible."

The present state of obstetric knowledge then, with regard to the production of malpresentations of the fœtus, is confessedly very defective. All the doctrines that have been previously proposed with regard to the causation of the natural position of the fœtus have been imperfect in this important respect, that they have failed to afford any clue to the solution of the mode of causation of its occasional preternatural positions. But the new explanation, which I have offered, regarding the production of the normal position of the child, derives additional strength and confirmation from its affording at the same time a perfect rationale of the mode, or modes of production of its anormal presentations and positions.

In the observations which I have offered in the preceding section fo the present communication, I have attempted to establish the following, among other propositions, relative to the natural or common position of the fœtus:

1. The usual position of the fœtus, with the head lowest, and presenting over the os uteri, is not assumed till about the sixth month of intra-uterine life, and becomes more frequent and more certain from that time onwards to the full term of utero-gestation.

2. Both the assumption and maintenance of this position are vital and not physical acts, for they are found to be dependent on the existence and continuance of vitality in the child; concurring with its life, but being lost by its death.

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the uterus has been distended by an unusual quantity of liquor amnii, or where, from a faulty condition of the early pains of labor, its form has been altered, and with it, the position of the child. . . . . Shortness of the umbilical cord, or its being twisted round the child, insertion of the placenta to one side of the uterus, faulty form or inclination of the pelvis, obliquity of the uterus, as above mentioned, violent exertions or concussions of the body, plurality of children: of all these we do not believe that there is one which can exert the slightest influence in determining the position of the child."—System of Midwifery, p. 168.

[1] Principles and Practice of Midwifery, 1848, p. 212.

[2] Theory and Practice of Midwifery, p. 258.

[3] Obstetric Medicine and Surgery, p. 333.

[4] Lectures on Midwifery, p. 327.

3. In human physiology, we do not know or recognize any vital power or action, except muscular action, capable of producing motions calculated to alter or regulate the position, either of the whole body, or of any of its parts; and further, the motory muscular actions of the fœtus are not spontaneous or voluntary, but reflex or excito-motory in their nature, causation, and effects.

4. The position of the fœtus, with the head placed over the os uteri, is that position in which the physical shape of the normal and fully developed fœtus is best adapted to the physical shape of the normal and fully developed cavity of the uterus.

5. This adaptive position of the contained body to the containing cavity is the aggregate result of reflex or excito-motory movements on the part of the fœtus, by which it keeps its cutaneous surface withdrawn as far as possible from the causes of irritation that may act upon it as excitants, or that happen to restrain its freedom of position or motion.

In proceeding to discuss the production of the malpresentations and malpositions of the fœtus, I shall endeavor to show that its preternatural presentations originate in the derangement of one or other of these normal and necessary conditions; and that they are referable to the following series of causes, viz.—to

First.—Prematurity of the labor; parturition occurring before the natural position of the fœtus is established.

Secondly.—Death of the child in utero; or, in other words, the loss of the adaptive vital reflex actions of the fœtus.

Thirdly.—Causes altering the normal shape of the fœtus or contained body, or causes altering the normal shape of the uterus or containing body, and thus forcing the fœtus to assume, in its reflex movements, an unusual position, in order to adapt itself to the unusual circumstances in which it happens to be placed.

And *lastly.*—Preternatural presentations are occasionally the result of causes physically displacing either the whole fœtus or its presenting part, during the latter periods of utero-gestation, or at the commencement of labor.

Let us consider in succession and detail, the special causes of malpresentation of the fœtus, referable to each of these general heads.

1.—*Prematurity of Parturition.*

We have seen that the common or normal position of the fœtus, with the head placed lowest, and presenting over the os uteri, is not in general assumed till the sixth month; and that from this period onward to the full time, this special position is taken and kept with a certainty and frequency which gradually increase in proportion to the advancement of the pregnancy. The data published in the table given at p. 111, show, in evidence of this law, that, while the percentage of head presentations among children born at the end of the sixth month is only about 52 in 100, or nearly 1 in 2, this percentage increases to 68 in 100 during the seventh month; to 76 in 100 during the eighth and ninth months; and at last, at the full term of utero-gestation, the proportion of head to other presentations rises as high as 96 or 97 in every hundred births.

It is hence evident, that if, from any causes, parturition happens to come on prematurely, the child is much more liable to present preternaturally, than if pregnancy had gone on to the full time; or, in other words, the prematurity of the labor is, in this sense, a cause of the malpresentation of the fœtus.

The following table shows the variety of natural and preternatural presentations of the child met with by different observers in various series of cases of premature labor. The returns of Drs. Collins,[1] M'Clintock, and Hardy,[2] refer to the Dublin Hospital, and those of Professor Dubois[3] refer to the Maternity Hospital of Paris. The returns of Hoffman,[4] Hamilton,[5] and Ramsbotham,[6] are practically interesting in one respect, namely, that they are the results obtained on the same point in a large number of cases, in which the induction of premature labor has been artificially performed.

Table of the Presentations of the Fœtus in 1087 Premature Labors, supervening spontaneously, or induced artificially.

Reporters.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Collins,	498	393	102	3
M'Clintock and Hardy,	108	70	29	9
Dubois,	240	147	82	11
Hoffman,	120	45	56	19
Hamilton,	59	54	4	1
Ramsbotham,	62	53	6	3
Total,	1087	762	279	46
Proportions among premature labors,		70 in 100	1 in 4	1 in 23
Proportions among common labors at full time,		96 in 100	1 in 31	1 in 224

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[1] Practical Treatise, p. 461, &c., and Dublin Quarterly Journal for 1836, p. 198.

[2] On Midwifery and Puerperal Diseases, p. 4.

[3] Mémoires de l'Académie Royale de Médecine, tom. ii. p. 287.

[4] British and Foreign Medico-Chirurgical Review, No. iv. 1848, p. 551.

[5] Practical Observations in Midwifery, p. 289.

[6] Obstetric Medicine and Surgery, p. 299.

The last line in the preceding table, and in several of those that llow, gives the relative number of cephalic, pelvic, and transverse resentations seen among the large series of 84,000 labors reported the hospital returns of Lachapelle, Boivin, Clarke, and Collins, d which I have already arranged in a tabular form in a previous ge.[1] The comparison of the two last lines in the present table ows—1. That while presentations of the head occur in 96 per nt. in common obstetric practice at the full time, the same presentions occur only in 70 per cent. among premature labors; 2. That lvic presentations are nearly eight times more frequent among emature labors than among labors at the full time; and 3. That nsverse presentations are nearly ten times more frequent among emature labors than among labors at the end of the usual term pregnancy.

2.—*Death of the Child.*

The child not unfrequently dies in utero, and before labor bens. In cases in which the death of the fœtus is induced from any use, during its intra-uterine life, the child, when labor at last pervenes, is apt to be found presenting preternaturally. The exanation of this circumstance has been already given in a previous ction. The maintenance as well as the assumption of the usual sition of the fœtus with the head downwards and over the os eri, is an excito-motory, and consequently a vital act; and hence, hen the vitality of the fœtus is lost, its position, as a result of that tality, is liable to be lost also. In other words, the death of the ild thus becomes a cause of its malpresentation at the time of rth.

Table of the Presentations of the Fœtus in 669 Cases in which the Child had died in Utero.

Reporter.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Collins,	527	438	83	6
Hardy and M'Clintock,	142	115	21	6
Total,	669	553	104	12
Proportions among putrid children,		82 in 100	1 in 6	1 in 55
Proportions among common labors at full time,		96 in 100	1 in 31	1 in 224

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[1] See Table, p. 90.

Among the Dublin Hospital returns published by Drs. Colli Hardy, and M'Clintock, the presentations are noted in 669 cases which the child was expelled in a "*putrid*," state, and where, co sequently, it may be justly assumed that death had occurred so time before the supervention of labor. The preceding table sho the relative number of different presentations among these 6 uptrid children.

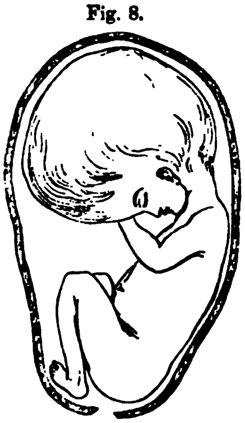
As far, then, as these data go, the difference between the liability cephalic, pelvic, and transverse presentations between children w have died before labor, and those born alive, may be expressed follows:—1. Head presentations are 16 per cent. less frequent amo dead than among living infants; 2. Pelvic presentations are fi times more frequent among dead than among living children; a 3. Transverse presentations are four times more frequent among t former than the latter.

Under the first of the preceding heads, we have traced the m presentation of the fœtus to the occurrence of labor before the refl movements of the fœtus had sufficiently fixed and established i normal position with the head lowest. Under the second head, have seen the absence or loss of the fœtal reflex movements fro the intra-uterine death of the child, leading to the same result. W have now to speak of a series of other causes leading to preternatu presentations, by directly changing the relative forms, either of t fœtus or of the uterine cavity, and thus indirectly forcing the co tained fœtus to place itself, by its reflex motions, within the co taining uterine cavity, in some anormal position, in order so far adapt itself to its anormal circumstances, that it may avoid and ave as much as possible, the irritation of those external excitants, th we have seen capable of acting upon its cutaneous surface a excito-motory system. Under this third head, and as referable it, we shall consider the influence, in the production of preternatu presentations, of diseases of the fœtus, its states of malformation a monstrosity, the existence of twins, of hydramnios, of spasmod contractions, and organic disease in the uterine parietes, of placen prævia, of deformity of the pelvis, and of original anomalous co figuration of the uterus.

3.—*Intra-uterine Diseases of the Fœtus altering its form.*

Various affections of the fœtus in utero terminate in alteration, a greater or less degree, of the form and shape of the infa Ascites, for instance, spina bifida, hydrocephalus, &c., have t fect. Of these intra-uterine diseases the most mmon is hydrocephalus (Fig. 8); and taking as the most frequent and noted pathological ample, I shall limit to it the proofs that I duce of the effects of diseases altering the m of the fœtus, having the effect of altering o its position or presentation.

Fig. 8.



Among sixty-nine cases of intra-uterine hy- cephalus, previously referred to as collated Dr. Thomas Keith, in fifty-nine the cepha- , and in ten the pelvic extremity of the in- t presented. One of the sixty-nine cases s a transverse presentation.

Table of Proportions of different Presentations of the Fœtus in 69 Cases of Intra-Uterine Hydrocephalus, and in 84,000 Cases of Common Labor.

Conditions.	No. of Head Presentations.	No. of Pelvic Presentations.	No. of Transverse Presentations.
Hydrocephalus cases, . .	59 in 69	1 in 7	1 in 69
Common cases, ...	96 in 100	1 in 31	1 in 224

Other diseases of the fœtus and its appendages may perhaps act causes of its malpresentation—such as inflammatory adhesions the surface of the fœtus to the surface of the amnion; morbid hesions and fixation of the umbilical cord upon the body or mbs of the fœtus, &c.; but I am not aware of any recorded ries of facts sufficient to prove or disprove this supposition. If ch causes act in producing malpresentations, they will probably found to lead to this effect, by restraining, within fixed limits, e adaptive reflex movements of the fœtus, and by thus preventing ese movements from placing the fœtus in its usual position ithin the uterine cavity.

4.—*Malformations and Monstrosities altering the Form of the Fœtus.*

In the recorded histories of cases of malformation of the fœtus, e details of the birth and presentation of the child are very rarely iven. The teratological and anatomical facts connected with such alformed infants, have hitherto principally engaged the attention f medical men, and their obstetric history has been almost entirely eglected. But the data that we do possess prove sufficiently, in lation to such malformations and monstrosities, that the resulting teration which they produce in the shape of the fœtus is, as we hould a priori expect, a frequent cause of their preternatural pre- ntation in utero and at the time of birth.

In cases of abdominal and thoracic eventration of the fœtus (the "Monstres Cesolomiens," in the classification of St. Hilaire), the malformed infant usually presents, according to Klein, not by the head, but by the abdomen, the mass of the displaced viscera lying in front.

From Fried, Harhold, and Klein, Burdach cites cases of imperfect development of the lower extremities in which the fœtus presented, not by the head, but by the malformed parts.[2]

In his work on Teratology, Isidore St. Hilaire states it as a general and remarkable fact (*fait remarquable*), "that the presentation by the head is much less common among fœtuses affected with monstrosities and malformations, than among normal fœtuses. I have," he adds, "collected cases, published and unpublished, in sufficient number, to leave no doubt in relation to the truth of this result; but I only add, with dubiety, and as an opinion requiring the confirmation of future observers, that the part which most frequently, and as it were by preference, presents, is the region affected with the malformation."[3]

An immense number of cases of double monstrosity have been placed upon record; but the presentations of the children at birth are stated in very few instances. The following table contains the results of the presentation in fifteen such cases, including, in fact, all, which a cursory examination of this point in teratology has enabled me to ascertain.

Table of the Presentations in Fifteen Cases of Double Monstrosity.

Reporter.	Nature of Presentation.	Reference.
Huron, . . .	Feet,	Monthly Journal of Med. Sc. 1847–48, p. 67.
Derien, . . .	Feet,	Gazette Médicale de Paris, No. 23, 1848.
Bromilon, . .	Head,	Edin. Med. and Surg. Journal. vol. lv. p. 435.
Lyell, . . .	Feet,	Monthly Journal of Med. Sc., August, 1848.
Moreau,. . .	Feet,	Traité des Accouch., tom. ii. p. 309.
Duverney, . .	Feet,	Mémoires de l'Acad. Roy. de Méd., tom. i. p. 357.
Peu, ...	Head,	Do. do. do. p. 358.
Evrat, . . .	Head,	Do. do. do. p. 358.
Brez d'Angers,	Feet,	Do. do. do. p. 359.
Regnoli,. . .	Feet,	Do. do. do. p. 359.
Ratel, . . .	Head,	Do. do. do. p. 360.
Molas, . . .	Head,	Do. do. do. p. 363.
St. Hilaire,. .	Feet,	Des Anomalies &c., &c., tom. iii. p. 114.
Letonze, . .	Head,	Archives Générales de Medecine, Dec. 1848.
Askham, . .	Feet,	Lancet, vol. ii. 1848, p. 235.

It may be observed that out of these fifteen cases, the presentation was footling in nine, and cephalic in six. The instances of

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[1] Deutsches Archiv für Physiologie, Bd. iii. 1817, p. 39, &c.

[2] Traité ed Physiologie, tom. iv. p. 223.

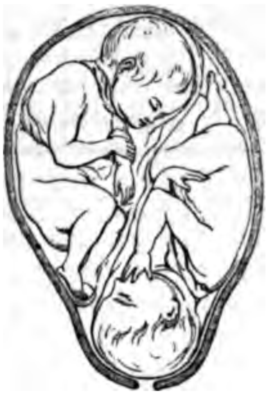
[3] Histoire des Anomalies, &c., tom. iii. p. 570.

footling presentation were consequently extremely prevalent, and in fact preponderated over the other. In none did the arm present.

5.—*Twins.*

When two or more children are contained simultaneously within he uterine cavity, the form of the individual cavity or loculament ccupied by each child, varies more or less from the regular ovoid rm of the expanded uterus. The cavities which contain the œtuses in multiparous pregnancy are more or less relatively different in shape from the cavity which contains the fœtus in uniparous pregnancy. A glance at the sketch of the form and contents f the uterus in plural pregnancy, given by Smellie, and copied in igure 9, sufficiently confirms this remark. And as the reflex irritations and movements of the fœtus force it to adapt itself to the form of the cavity containing it, malpositions and malpresentations among twin children are more common than among single births. The following table in proof of this statement is constructed from the returns of presentations among twin children, as observed in the Dublin[1] and Edinburgh[2] Lying-in Hospitals, and among the patients of the London Maternity Charity.[3]

Fig 9.



In pregnancies with triplets, as with twins, there is the same tendency to malpresentations of the child, and from the same cause. Amongst eighteen children born in cases of triplet pregnancy, and detailed in the reports referred to in the footnote,[1] the following form the modes in which the children presented at birth. Of the eighteen children, thirteen presented by the head, or seventy per cent.; four children presented by the pelvis, and one among the eighteen was a transverse presentation.

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[1] Clarke, Transact. King's and Queen's Coll. of Physic., Ireland, vol. i. p. 403. Collins, Practical Treatise, p. 314. Hardy and M'Clintock's Report, p. 329.

[2] Monthly Journal of Medical Science, 1848, p. 335.

[3] Ramsbotham's Obstetric Medicine and Surgery, p. 495. Reid, in London Medical Gazette, Nov. 1835.

Table of the Presentations of the Fœtus in 808 *Labors with Twin Children.*

Reporter.	Total Number of Cases.	Number of Head Presentations.	Number of Pelvic Presentations.	Number of Transverse Presentations.
Clarke,	126	73	53	—
Collins,	449	309	133	7
Hardy and M'Clintock,	190	122	62	6
Ramsbotham,	772	532	221	19
Simpson,	30	23	7	—
Reid,	48	25	22	1
Total,	1615	1084	498	33
Proportions among twin children,		67 in 100	1 in 3	1 in 49
Proportions among all births,		96 in 100	1 in 31	1 in 224

6.—*Hydramnios, or Excess of Liquor Amnii.*

Before the sixth month, the quantity of liquor amnii is very much greater, in relation to the size of the fœtus, than at the full time. In fact the quantity surrounding the fœtus is up to that period relatively so large, and the uterine cavity so distended with it, that the fœtus can move about in it readily and rapidly. (See Fig. 7.) This perfect freedom, from large room, is, as we have already seen, the principal reason why the fœtus does not require to assume any specific position before that time. But when the liquor amnii remains in great quantity up to the full time, or when the fœtus remains small, the pregnancy of these earlier and temporary conditions produces anormally at the full time, the same result which they normally produced at the fifth and six months—namely, the presentation is often preternatural.

I have no statistical proofs to offer in evidence of this statement, as no collection of cases of hydramnios at the full time, and of its effects upon the presentation, has, so far as I know, been yet made by any author. But the fact itself, that malpresentations are often found coexisting with a preternatural collection of liquor amnii, is admitted by most obstetric writers. "The passive motions of the fœtus," observes a late German author,[1] "are easily performed when there is an excess of liquor amnii, and we not unfrequently find with it circumvolutions of the cord around the child, and preternatural positions of the fœtus."

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[1] Collins, Practical Treatise, p. 340 ; Hardy and M'Clintock's Report, p. 330 ; Simpson in Monthly Journal of Medical Science, 1848, p. 339 (see also vol. i. of this work, p. 753) ; Davis Ibid. 1841, p. 448.

7.—Spasmodic Contractions in the Uterine Parietes.

In the latter months of utero-gestation, patients often feel and suffer from spasmodic contractions in the uterine parietes. When these spasmodic contractions are severe, limited to particular portions of the uterine walls, and more permanent than usual, they sometimes so alter the normal form of the uterine cavity as to force the fœtus to assume a new and anormal position. "It is," say Dr. Rigby,[2] "a well-known fact that cross-births, as they have been called, are frequently preceded by severe spasmodic pains in the abdomen, from which the patient suffers for some days or even weeks before labor has commenced; the uterus is more or less the seat of these attacks, which usually come on towards night-time; and, in some instances, it is felt for the time hard and uneven from irregular contraction. It was the circumstance of this symptom having preceded five successive labors of a patient, in all of which the child had presented with the arm or shoulder, which induced Professor Naegele, when attending her in her sixth pregnancy, to endeavor to allay these cramp-like pains, which had begun to show themselves, as severely as on former occasions. Having tried opium by itself, and also in combination with ipecacuanha or valerian without effect, he ordered her a starch injection with twelve drops of tinct. opii every night, so long as she continued to suffer from these attacks; the spasms soon ceased, nor did they appear again during the remainder of the pregnancy, and he had the satisfaction of delivering her at the proper time, of a living child, which presented in the natural manner."

8.—Organic Diseases of the Uterine Parietes, &c.

By far the most common form of morbid growth in the parietes of the body and fundus of the uterus, is the common and well-known fibrous or fleshy tumor. Pregnancy not unfrequently occurs when the uterine walls are the seat of this diseased structure. Fibrous tumors of the uterus are seldom found in connection with pregnancy so large in size, or so placed towards the internal surface of the uterus, as to alter its cavity from its normal form and make the fœtus assume a preternatural position. But occasionally pregnancy does take place even when the tumor encroaches much upon the uterine cavity; and the resulting form of that cavity may be such as to cause a preternatural presentation. In one case, where a large fibrous tumor existed at the fundus uteri, I found the breech of the infant form the presenting part in labor. Preternatural presentations of the infant in connection with large fibrous tumors in the uterine parietes have been recorded by Dr. Beatty,[1] Dr. Ashwell,[2] &c. But I am not acquainted with any series of data sufficient to show the degree of liability to preternatural presentations resulting from the presence of fibrous tumors, or such other organic diseases in the uterine parietes, or in the ovaries or neighboring viscera, as may have the effect of altering the shape of the uterine cavity during pregnancy. This defect, however, is capable of being so far supplied by appealing to the next two causes which I have to enumerate, viz.: placenta prævia, and great deformity of the brim of the pelvis. Under both these circumstances, the normal form of the uterus is altered by preternatural organic conditions existing in the one (placenta prævia) within the interior of the uterus, and in the other (deformity of the brim) existing without the uterus,—but both leading to the same result—a liability to preternatural presentation from the same cause, viz., a change in the physical shape of the uterine cavity.

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[1] Busch and Moser's Handbuch der Geburtskunde, Bd. iii. p. 361. See also the works of Wigand. Carus, Rigby, &c.

[2] System of Midwifery, p. 169.

9.—*Placenta Prævia.*

The lower end of the ovoid cavity of the uterus at the full term of pregnancy, is principally formed after the sixth month, and from the development of the cervix uteri. In placental presentations this lower or narrow end of the uterine ovoid does not acquire its normal size and form in consequence of the implantation of the placental mass upon its interior. Hence, in these cases, the whole normal ovoid figure of the cavity of the uterus is altered in shape, and in adapting, through its reflex movements, its form to the altered form of the uterus, the fœtus is liable to assume and maintain a preternatural position.

The data in the following table afford ample evidence of the great frequency of preternatural presentations of the fœtus in cases of implantation of the placenta over the os and cervix uteri, a fact scarcely, if indeed at all, alluded to by obstetric writers. The data are derived—1. From the published reports of Madame Lachapelle, and Dr. Collins, the only two authors who seemed to have stated the presentations of the fœtus in *all* the cases of placenta prævia

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[1] Dublin Journal of Medical Science, vol. xvii. p. 414.

[2] Treatise on Diseases of Women, p. 338.

:h they record; and, 2. From a long series of unpublished s of placenta prævia in the practices of Dr. Wilson of Glasgow, of Drs. John and Francis H. Ramsbotham of London, with manuscript notes of which I have been very kindly favored.

Table of the Presentations of the Fœtus in 366 *Cases of Placenta Prævia.*

Reporter.	Total No. of Cases.	No. of Head Presentations.	No. of Pelvic Presentations.	No. of Transverse Presentations.
ıchapelle,	17	14	2	1
·llins,	12	8	3	1
ilson,	29	25	4	—
Ramsbotham,	124	108	11	5
H. Ramsbotham	184	155	19	10
Total,	366	310	39	17
?roportions in cases of Placenta Prævia,		85 in 100	1 in 9	1 in 21
?roportions among common labors at full time, [illegible]		96 in 100 [illegible]	1 in 31 [illegible]	1 in 224 [illegible]

he comparison of the two last lines in the present table, shows That while presentations of the head occur as 96 per cent. in mon obstetric practice at the full time, the same presentations ır as only 85 per cent. among cases complicated with placental entation; 2. That pelvic presentations are more than thrice as uent among cases of placenta prævia as among ordinary labors ıe full time; and 3. That transverse presentations are ten times e frequent in cases of placental presentation than among ordi- ' labors at the full term of pregnancy.

10.—*Distortion and Contraction of the Brim of the Pelvis.*

ew obstetric authors have adverted to pelvic contraction and ɔrtion, as a cause of preternatural presentations. Yet its occa- al operation is certain.

Mr. Barlow,"[1] observes Dr. Ramsbotham,[2] "states that he is in- ɜd to believe preternatural presentations are far more frequently with under distortion of the pelvis, than when that organ is ·formed. This remark," Dr. Ramsbotham continues, "coin- s with my own observation;" and in another part of his work,[3] 'urther remarks, "Transverse presentations are by no means paratively more frequent among the poor than those in affluent ɪmstances; but some women seem to be naturally predisposed his irregularity. Thus, a patient whom I attended in all her labors, out of five children which she has borne, has been the subject of four transverse presentations; her pelvis is slightly distorted at the brim. And another woman, now dead, who always, under pregnancy, became a patient of the Royal Maternity Charity, in twelve labors suffered seven shoulder presentations. I delivered her myself five times under these difficulties, and my father twice. This person also possessed a contracted pelvis."

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[1] Essays on Surgery and Midwifery, p. 348.

[2] Obstetric Medicine and Surgery, p. 299.

[3] Ibid. p. 334.

When the pelvic contraction is great, and involving the brim, and hence interferes much with the development and shape of the lower end of the uterus, its influence in leading to preternatural presentations of the fœtus is well marked, and capable of statistical proof. The operation which requires to be had recourse to when the pelvis is thus very highly deformed, is the Cæsarean section. In most of the cases in which this operation is recorded to have been practised, the presentation of the fœtus is not mentioned, and in 44 instances only have I been able to find, on a casual examination, any adequate data relative to the presentation of the child. References to these 44 cases are given in the accompanying footnote.[1] Among the 44 infants, 30 presented by the head, or about 68 per cent.; 8 of the children, or nearly 1 in every 5, were pelvic presentations; and 6, or one in about every 7, presented transversely. Or we may state these results as contrasted with the presentations in cases of common labor in a tabular form, as follows:

Table of Proportions of Different Presentations of the Fœtus in 44 *Cases of Cæsarean Section, and in* 84,000 *Cases of Common Labor.*

Conditions.	Proportion of Head Presentations.	Proportion of Pelvic Presentations.	Proportion of Transverse Presentations.
In Cæsarean Sections,	68 in 100	1 in 5	1 in 7
In Common Labors,	96 in 100	1 in 31	1 in 224

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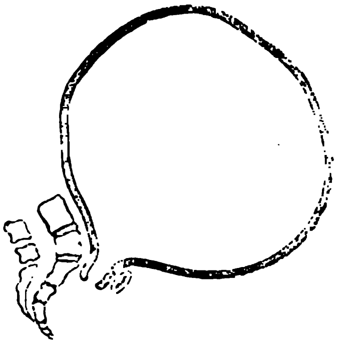
[1] The following are references to the cases in which the head is mentioned as presenting:—Smellie's Midwifery, vol. iii. p. 423; MS. Lectures of Professor Young; First Letter of Dr. Hull, of Manchester, three cases; Hull's Translation of Baudelocque's Essay on Cæsarean Section, three cases; Edinburgh Medical and Surgical Journal, vol. xxiv. p. 296, also p. 304; vol. xxx. p. 53; vol. xxxi. p. 443; Medico-Chirurgical Transactions, vol. ix. p. 13; British and Foreign Medical Review, vol. x. p. 572; vol. xi. p. 531; vol. xiii. p. 545; Lancet, June, 1840; Northern Journal of Medicine, vol. iii. p. 247; Meigs' Obstetrics, p. 531; American Journal of Medical Science, vol. i. p. 269, two cases; vol. xii. p. 386; Monthly Journal of Medical Science, 1841, p. 218; 1842, p. 425; 1843, p. 160; 1844, p. 358; 1845, pp. 323, 887; 1846, p. 309; 1847, p. 130. In the succeeding cases, some part of the pelvic extremities is mentioned as presenting:—Hull's Translation of Baudelocque's Essay, two cases; Medico-Chirurgical Transactions, vol. vii. p. 264; Edinburgh Medical and Surgical Journal, vol. viii. p. 11; vol. xvii. p. 106; British and Foreign Medical Review, vol. ii. p. 271, and a second time in the same case; American Journal of the Medical Sciences, No. xxxii. 1835, p. 546; Monthly Journal of Medical Science, 1843, p. 835. The following are references to cases in which some part of the superior extremities of the infant is stated to have presented:—Hull's Translation of Baudelocque's Essay, four different cases; and Edinburgh Medical and Surgical Journal, vol. xxii. p. 248.

In speaking of pelvic contraction as a cause of malposition of the infant, Dr. Ramsbotham seems to doubt whether it be possible to explain in what manner such a cause could lead to the effect which he attributes to it. "*How,*" he observes, "a contraction of the pelvic brim can influence the position of the fœtus in utero, it is difficult to explain or even to imagine."[1] But the solution of this apparent difficulty is sufficiently easy upon the principles which I have endeavored to state in the present essay. A contracted brim, more especially when that contraction is great, directly interferes with the expansion of the lower part of the uterus, forces the uterus, in this way, into a preternatural form, and forces the fœtus to place itself by its reflex actions in preternatural positions, in order to lie adapted to that form. When the pelvis is much deformed by mollities ossium, or rickets, as in cases requiring the Cæsarean section, other additional circumstances aid in leading to still greater alterations in the form of the uterus. The trunk is generally also deformed, the ribs approached towards the pelvis, and the abdominal cavity diminished in depth. During its development, the expanding uterus is not only prevented from developing itself downwards, but it is also prevented from developing itself to its usual extent upwards. It grows and expands forwards and laterally, or, in other words, in the directions in which there is least resistance to its increase in volume. Under these restraints and conditions it expands more, laterally and antero-posteriorly, and less in the longitudinal direction than is natural. It tends to assume such a form as is represented in Figure 10. This globular form, simulating the globular form of the uterus at the fifth month, is perhaps the cause why transverse presentations are so peculiarly common in instances in which the brim of the pelvis is so much deformed as to require the Cæsarean section.

Fig. 10.



11.—*Irregularity in the Configuration of the Uterus.*

Like every other organ of the body, the uterus occasionally shows, in particular individuals, deviations to a greater or less extent from its normal standard form. Some of its more marked deviations in shape and development interfere so much with its functional activity as to be a cause of sterility. Other changes found in its configuration do not necessarily prevent conception, but when pregnancy occurs along with them, the misshaped uterus, during its growth, continues sometimes to remain so far altered in form and shape from the natural ovoid figure of the expanded organ, that the adaptive reflex movements of the fœtus necessarily place it, in such a cavity, under a preternatural position. Jahn,[1] Meissner,[2] Meckel,[3] and others,[4] have described and collected many cases in which the uterus showed various anomalies in form and configuration under different original deviations of form or development. But without entering into an account of these individual anomalies and their effects, I will content myself with illustrating, by a single case, the mode of action of the cause in question, in the production of malpresentations:

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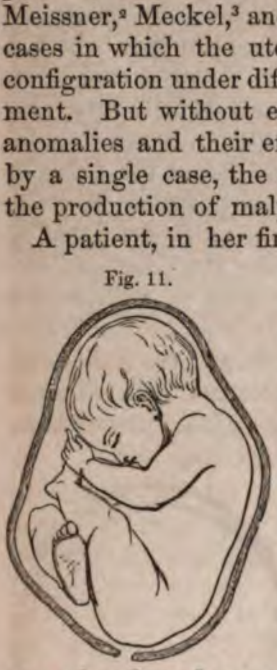
[1] Obstetric Medicine and Surgery, p. 229.

Fig. 11.



A patient, in her first two labors, had the arm of the child presenting. In her third labor, the same presentation recurred. On examining the uterus before delivery, Dr. Lecluyse found it of an anormal configuration. It was short and depressed in its vertical direction, but large and expanded towards either side. It was ellipsoid instead of pyriform in shape; and had thus gained in lateral width what it had lost in perpendicular height. Its long axis was from side to side; and in placing itself under its reflex movements, so as to adapt the long axis of its ovoid mass to the long axis of the uterus, the fœtus, of necessity, was placed and presented transversely. After the uterus was emptied in this case, the organ still presented the same deformity of shape.[5]

The preceding case refers to an original deviation in shape in a single uterus. In cases of duplicity of the uterus, impregnation sometimes takes place. The pregnant side or horn occasionally presents, when distended, such a degree of irregularity of shape, as to force the fœtus into some corresponding irregularity in position. Geiss reports a case of duplicity of the uterus, where the presentation of the fœtus was transverse.[1] In describing such cases, most authors have omitted to state the nature of the presentation.

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[1] Schlegel's Syllog. operum ad Artem Obst., vol. i. p. 257.

[2] Frauenzimmerkrankheiten, Bd. i. p. 535, *et seq.*

[3] Pathologischen Anatomie, Bd. i. p. 673.

[4] Voigtel's Handbuch der Pathol. Anatomie, Bd. iii. pp. 1–455, &c. Colombat's Traité des Maladies des Femmes, tom. i. p. 200. See also the writings of Hunter, Rokitansky, Tiedemann, Cassan, Moreau, &c.

[5] Annales de la Société d'Anvers, 1845, p. 89. London Medical Gazette, vol. xxxv. p. 847. In Figure 11, I have copied from Chailly the figure which he gives of the shape of the uterus under a pelvic presentation. A uterus of such a configuration (with the ovoid reversed—the largest extremity lowest), would, according to the principles laid down in the text, lead to the pelvic instead of the cephalic position of the fœtus.

Perhaps under the present head of causes I should notice the supposed effects of obliquities and flexions of the uterus in the production of malpresentations. These obliquities and flexions, however, when they exist before pregnancy, generally become spontaneously rectified as utero-gestation advances; and thus do not ultimately interfere with the shape of the uterine cavity, or, through its shape, with the position of the child. But in those rare cases where the configuration of the uterine cavity remains altered by their influence, particularly in its lower or cervical part, this altered configuration of the containing organ may occasionally affect the corresponding position and mode of presentation of the contained fœtus.

12.—*Mechanical and Casual Displacements of the Fœtus.*

Some authors have considered accidental displacements of the fœtus from falls, carriage exercise, strong emotions, &c., as one of the principal causes of its malpresentations, whilst others have doubted or denied the possible influence of their operation.

Up to the sixth or seventh month of pregnancy, the fœtus is in general relatively so small, and the surrounding quantity of liquor amnii so abundant, that the child may be readily displaced from the position which it happens at the time to occupy, by any violent movements or succussions on the part of the mother. When the fœtus enlarges more, and ultimately requires, in consequence of its increased length, to place its long axis parallel with the long axis of the uterus—the possibility of its being accidentally turned completely round in the uterine cavity is prevented by the physical fact of the long axis of its head and body being longer than the width or short axis of the cavity, in which it would make this supposed evolution. The usual length of the fœtal ovoid at the full time is about twelves inches, and the usual lateral width of the uterus about eight or nine inches, so that the former could not readily turn within the latter. But when the fœtus is smaller, the quantity of liquor amnii considerable, and the shock given to the maternal trunk in any degree great, a complete displacement of the fœtus may no doubt occur, so as to change, for example, a cephalic into a pelvic presentation; and the return from the pelvic to the primitive cephalic position is subsequently prevented by the same physical relations as guarded against its original occurrence. My friend Mr. Cockburn informs me that a patient of his, the mother of eight children, had seven of the children presenting naturally, and one by the pelvis or feet. When well advanced in pregnancy with the child that ultimately presented preternaturally, she happened to be thrown down with a severe fall upon her back. To Mr. Cockburn she declared immediately afterwards her own conviction that she felt the child turn round at the time of the accident, and that it would be born in a wrong position; as ultimately proved to be the case.

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[1] Rust's Magazin für die gesammte Heilkunde, Bd. xx. 1825, p. 8.

The presenting head of the fœtus sometimes happens to become mechanically displaced at the commencement of labor, and a primary normal presentation becomes thus changed into a secondary preternatural presentation. This has been principally observed to happen when there was some obstruction at the brim of the pelvis, either in the bones or soft parts; the head when driven obliquely against this obstruction by the first uterine contractions, has slipped aside, and the neck, or rather shoulder, come to present over the os uteri. Denman,[1] Merriman,[2] Burns,[3] &c., have described and directed attention to cases of secondary malpresentation of this kind. Where the quantity of liquor amnii at the time of labor continues great, and the child is small and mobile, it may occasionally, though rarely, at the full time, become repeatedly displaced in different ways under the pressure of the contracting uterus, and the reaction of the opposing maternal structures. Baudelocque relates a case in which "during a labor of thirty-six hours, the child presented successively, and several times over, the head, the feet, the back, the shoulder, or one of its sides; and, at the instant the membranes burst, the belly, the knees, and a loop of the cord."[4]

The various minor modifications of the position of the head and pelvis met with at the commencement of labor, are, for the most part, owing to slight mechanical changes produced in the commencement of labor by the compressions of the uterus upon the presenting parts, during the first pains, and the reaction of the maternal parts upon those parts of the child.

[1] "Having been called to women in the beginning of labor, and finding, by an examination, that the head of the child presented, I have left them for several hours till the first changes were naturally made. When I have examined them on my return, I have found the arm of the child presenting, the head being departed out of my reach."—Denman's Introduction, p. 495.

[2] Medico-Chirurgical Transactions, vol. x. p. 62.—In a case of tumor of the soft parts obstructing delivery, the head, after presenting during a labor which was allowed to last for three days, was removed under the action of the pains, and the right shoulder came to present.

[3] "It is a fact well ascertained, that although the head may be felt distinctly in the commencement of labor, yet when the membranes break it may be exchanged for the shoulder or some other part. I have been informed of a case where the shoulder was exchanged for the head, and Joerg seems to have met with the same circumstance."—Burns' Principles of Midwifery, 9th edition, p. 411.

[4] System of Midwifery, Heath's Translation, vol. i. p. 264.

V. RELATIVE INFLUENCE OF THE SPECIFIED CAUSES IN THE CTION OF THE DIFFERENT GENERA OF PRETERNATURAL PRE- TIONS.

is considerable variety traceable in the power and frequency ich the different causes of preternatural presentations that described, act in producing these different presentations. t only produce malpresentations, with different degrees of , but some produce one form of malpresentation more than another. The comparative effects in this respect of f the causes which I have enumerated cannot be ascer- ecause we have no statistics of the results to guide us. In e to others, I have adduced statistical information sufficient e us to calculate imperfectly, and in a general way, their effects. The following table shows the comparative fre- with which six of the causes of preternatural presentation, have above considered, respectively led to the occurrence rnatural presentations of the fœtus:

able of Proportion of Preternatural Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Malpresentations.	Proportion of Malpresentations.
ture labor, . . .	1087	325	1 in 3
ean section, . . .	44	14	1 in 3
pregnancy, . . .	1615	531	1 in 5
of the child, . .	669	116	1 in 6
cephalus, ...	69	11	1 in 6
ta prævia, . . .	366	56	1 in 7
on practice, . . .	84,684	3128	1 in 27

the causes producing preternatural presentations do not produce the different forms of these malpresentations in the lative proportion. Some causes predispose more to pelvic transverse presentations, and others the reverse. The fol- table gives the relative frequency of pelvic presentations, he influence of the six causes enumerated:

Table of Proportion of Pelvic Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Pelvic Presentations.	Proportion of Pelvic Presentations.
ture labor, . . .	1087	279	1 in 4
ean section, . . .	44	9	1 in 5
pregnancy, . . .	1615	498	1 in 3
of the child, . . .	669	104	1 in 6
cephalus, ...	69	10	1 in 7
nta prævia, . . .	366	39	1 in 9
non practice, . . .	84,684	2750	1 in 31

It will be observed, that the order of relative frequency of pelvic presentations under these six specified causes, differs from the order of relative frequency of preternatural presentations in general under the same causes, and as given in the table immediately preceding. The order of relative frequency of transverse presentations from the same six causes, is very different from either of the above, as the facts in the following table will prove:

Table of Proportion of Transverse Presentations under Six specified Causes.

Conditions.	No. of Cases.	No. of Transverse Presentations.	Proportion of Transverse Presentations.
Premature labor, . . .	1087	46	1 in 23
Cæsarean section, . . .	44	5	1 in 9
Twin pregnancy, . . .	1615	33	1 in 49
Death of the child, . . .	669	12	1 in 55
Hydrocephalus, ...	69	1	1 in 69
Placenta prævia, . . .	366	17	1 in 21
Common practice, . . .	84,684	378	1 in 224

The preceding tables demonstrate, that some of the causes which are liable to lead to preternatural presentations are more liable to produce pelvic than transverse presentations; or the contrary. And if we venture to inquire into the grounds of this difference of of effect, we shall find it existing, I believe, in the peculiarities of form, and, consequently, of adaptation, which the causes in question produce, either upon the containing uterus or the contained fœtus. For example, let the cause producing the malpresentation be liable to change the form of the uterus, so that this organ be anormally increased in width, or in lateral or anterior development, and the result will be a tendency to the frequent occurrence of transverse presentations of the fœtus; for the long axis of the ovoid mass of the fœtus becomes placed transversely, in order to be adapted to the misshapen uterus, the long axis of which, in these supposed conditions, is more lateral or transverse than vertical. We have often this form of the uterus in Cæsarean section (Fig. 10), for reasons that I have already mentioned under that head; and in placenta prævia, where the placental mass fills up more or less the cavity of the cervix, and prevents its full development. Under the action of both these causes, the preceding table shows that transverse presentations are especially common. Again, in hydrocephalus of the fœtus, transverse presentations are rare, but pelvic presentations common; because the irregularity of form in the contained body is such, that the child may be adapted to the containing uterus, far more readily in a pelvic than in a transverse position.

SECTION VI. ON THE RECURRENCE OF MALPRESENTATIONS IN SUCCESSIVE PREGNANCIES IN THE SAME MOTHER; AND ITS CAUSATION.

Various authors mention the aptitude to the recurrence of preternatural presentation in different pregnancies in the same mother; and the fact is well known and established in obstetric pathology.[1]

Some time ago I delivered by turning, for presentation of the arm and cord, a patient of Mr. Cunningham. It was the sixth child, and the only one born alive. With one exception, the presentations in her five previous labors had been all preternatural, and most of them accompanied with prolapsus of the cord. Dr. Collins[2] mentions an instance in which as many as nine preternatural presentations occurred in the same mother.

Occasionally the presentation in the same mother, though always preternatural, is not always the same presentation. For example, Dr. Lee[3] mentions a woman with distorted pelvis, in whom the inferior extremities presented in the first and second labors, and an arm in the fourth; and another who had eight preternatural labors in succession, the arm presenting in the first four or five, and the nates or inferior extremities in all the others. In other instances there is a regular recurrence of exactly the same type of preternatural presentation in several successive labors. A case is recorded by Walther,[4] in which the shoulder presented in six successive pregnancies, in consequence of a malformation of the uterus. Madame Renard[5] has recently published an instance, in which a patient had the right shoulder presenting in five successive labors; the pelvis was deformed. Even more rare forms of complex preternatural presentation sometimes recur regularly in different labors in the same patient. Dr. Hamilton was in the habit of mentioning, in his lectures, a case in which presentation of the breech and one foot occurred in six or seven successive labors in a patient of Mr. Moir.

The recurrence of the same, or different malpresentations in successive pregnancies in the same mother, has generally been looked upon as a problem more difficult to explain and solve, than the more simple fact of the occasional occurrence of a preternatural presentation of the fœtus. And some authors seem to consider the phenomena as almost beyond the reach of explanation. "I feel wholly unable," observes Dr. Lee,[1] "to explain the cause of preternatural presentation occurring repeatedly in the same individuals, as described by Dr. Denman and other systematic authors." "It is," he elsewhere observes,[2] "very difficult, or impossible, to explain why the head of the child does not invariably present in labor, or assign a cause for the same woman having preternatural presentations in several successive labors, the head of the child being rarely, if ever, in them the presenting part."

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[1] "Though preternatural presentations," says Denman, "seldom occur when they are dreaded and expected, it is remarkable that some women are peculiarly subject to them; not once only, which might be considered the effect of some accident, but exactly to the same presentation, whether of the superior or inferior extremities, in several successive or alternate labors."—Introduction to the Practice of Midwifery, p. 467. See also the works of Ramsbotham, Burns, Meigs, &c.

[2] Practical Treatise, p. 40.

[3] Lectures on Midwifery, p. 327.

[4] Journal de Chirurgie, tom. iii. p. 59.

[5] Gazette des Hôpitaux, April, 1849.

The doctrine of the causation of the positions and presentations of the fœtus, which I have attempted to develope in the present memoir, affords a simple key to this obstetric enigma.

Some of the causes producing malpresentation, in the way described, are occasional or accidental only; and are not liable to recur in other pregnancies in the same mother. Hence, under the action of these causes, a malpresentation of the fœtus will occur in one or two labors only in the same patient, and not in all. To this set of instances belong the presence of twins; death or disease, and malformation of the fœtus; prematurity in the labor; mechanical, accidental displacement of the fœtus; and changes in the configuration and form of the uterus from spasms in its parietes, from the cervical or other anormal implantation of the placenta, and perhaps from other conditions which I have not taken time to trace out in the preceding observations, as fœcal accumulation in the colon, compression of the abdomen, and other such causes as may produce unequal pressure on the external surface of the uterus, and thus change the shape of its internal cavity—causes, some of which are capable of being prevented and treated, and their effects on the presentation of the child thus averted. But other causes of malpresentation of the fœtus are not occasional and accidental, but permanent and constant. They are not limited to one pregnancy, but present in all. They are anatomical conditions, either existing throughout the whole life of the mother, or recurring with each successive labor. For example, the lower part of the ovoid cavity of the uterus may be altered from the normal configuration and shape in each successive pregnancy, by deformity of the brim of the maternal pelvis—the deformed brim preventing its full development, forcing it always to assume a peculiar type and form, and this type being one which obliges the fœtus to assume some special or preternatural position, in order to adapt and accommodate itself to the existing special and preternatural form of the uterine cavity. I have already cited from Dr. Ramsbotham, Renard, &c., cases of deformity of the brim of the pelvis, leading in this way to a succession of preternatural presentations of the child in the same mother. Again, if the uterus itself is misshapen and malformed, a similar result follows. In each successive pregnancy there is the same constant deviation from the normal ovoid form of the uterine cavity, and as a consequence, the same deviation from the normal adaptive position of the fœtus. In the instances which I have quoted above from Walther and Lecluyse, of the recurrence of the same malpresentation in several successive pregnancies in the same mother, the cause of these malpresentations was an irregularity in the configuration of the uterus. The same organ—as the mouth, nose, hand, &c. &c.—is liable, when irregular in form, to present the same irregularity in several members of the same family. The same no doubt holds good in regard to the uterus also. Dr. Keiller has informed me of an instance of preternatural presentation recurring in different pregnancies in twin sisters. It appears to me that this curious fact will find its solution in some peculiarity in the form of the uterus repeated in the two sisters.

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[1] Clinical Midwifery, p. 117.

[2] Lectures on Midwifery, p. 327.

If the preceding remarks had not already extended far beyond the limits intended, I would have proceeded to show that the *positions* of the head, &c., of the child, in relation to the circumference and different diameters of the maternal pelvis, are, like the presentations, regulated in their ultimate analyses by the reflex or excito-motory actions of the fœtus. It is only by reference to the reflex excitations and motions of the fœtus, that we can explain, for example, the rarity of the positions of the head, &c., in the direct diameter of the brim, their frequency in the oblique, and especially in the right oblique diameter—and the greater relative proportion of occipito-anterior than of occipito-posterior positions of the cranium.

ON THE EXCITATION OF FŒTAL MOVEMENTS BY COLD.[1]

It is very generally believed by accoucheurs that the sudden application of a cold hand, or other similar body, to the cutaneous surface of the abdomen of a woman advanced in utero-gestation, is capable of exciting movements on the part of the fœtus; and such application has been often recommended as one of the simplest and best means of ascertaining, in doubtful cases of pregnancy, both the existence and the vitality of the child.[2]

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[1] See Edinburgh Monthly Journal of Medical Science, July, 1850, p. 90.

[2] See, for example, Montgomery's Exposition of the Signs of Pregnancy, p. 89; Dr. Gooch on Diseases of Women, p. 203; Dr. A. Hamilton's Letters to Dr. Osborne, p. 147; Dr. J. Hamilton's Practical Observations, p. 151, &c. &c.

But as there is no direct organic communication between the abdominal walls of the mother and the body of the fœtus, or between the nervous system of the mother and that of the child in utero, the power of exciting muscular movements in the latter by a sudden impression of cold upon any part of the abdominal maternal skin seems, physiologically considered, an impossibility. And clinical observation and direct experiment seem to me to prove the whole idea to be fallacious.

If the hand, whether cold or hot, is so much pressed inwards, as it inadvertently often is, so as either to indent, or alter the shape of the uterine walls, or to push and irritate the body of the fœtus, then fœtal movements will be liable to follow.

If, on the contrary, the hand, however cold, be applied so as merely to touch the skin, and not make pressure upon it and the uterus beneath, the impression of cold thus produced will never, I believe, be found to excite fœtal movements.

Such at least is the result of numerous clinical observations which I have tried to make upon the subject. In some direct experiments in regard to it, I touched the surface of the abdomen with metallic bodies, both larger and colder than the human hand, without exciting *any* movements in the fœtus. In a number of instances, with Dr. Weir, I applied large pieces of smooth ice to the surface of the abdomen, without in any one instance finding fœtal movements to follow. And yet, in these same cases, comparatively slight pressure with the hand upon the abdominal parietes and uterus, excited fœtal movements, whenever in any way the fœtus was irritated or displaced by that pressure.

VITAL CONTRACTIONS IN THE UMBILICAL ARTERIES AND VEINS.[1]

(From Edinburgh Monthly Journal of Medical Science, May, 1851, p. 494.)

It is well known that, up to a late period, some physiologists doubted entirely whether the larger arteries of the human body could be made to contract at individual points by stimulants applied to those points. Few or no satisfactory experiments have yet been published, showing that the veins possess the property of contracting locally under local stimuli. But in the umbilical cord, the medical practitioner may have daily an opportunity of repeating the following observations and experiments, which prove both of these important physiological facts, viz., the local contraction both of the tubes of arteries, and of the tubes of veins, under local stimuli applied to them.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, March 26, 1851.

First. After the child is born, the cord, whether tied or not, is generally seen to become contracted at different points, in consequence of the local contraction at those points of the vessels included within it. In fact, its vessels are thrown, by the irritation of the external atmosphere, and in consequence of their partially empty condition, into tubes consisting of a series of dilatations and contractions, like the peristaltic contractions of the intestine; but with this difference, that the contractions of the vessels are permanent.

Secondly. By pinching a portion of the cord, immediately after the birth of the child, between the nails of the thumb and finger, contractions of the vessels of the cord will be seen to occur at the point irritated. And if this mechanical irritation is confined to individual vessels, as to one of the arteries or to the vein, the effect is equally marked.

Thirdly. If the sheath of the cord be slit up by a pair of scissors or knife, with any of its vessels or arteries exposed, the same experiment may be repeated upon the exposed individual vessels with similar effect, viz., that the mechanical pinching of them will be followed in the course of a short time, by gradual but very marked contraction in the irritated part.

Fourthly. Similar local contractions of these vessels occur, under the local application to them of irritating chemical substances, or of electricity.

Fifthly. These contractions do not instantly follow the application of the stimulant, but a short time intervenes before the effect is seen, and the contraction is permanent.

Sixthly. When the experiment is made upon the human umbilical cord, or upon that of the lower animals, when the circulation is still going on in the vessels of the cord, the irritated vessel will sometimes almost entirely close its tube under such local irritation.

Seventhly. These simple experiments, illustrative of the contractility and irritability of the coats of the veins and arteries, are the more remarkable as seen in the umbilical veins and arteries, in consequence of anatomists not being able to detect any nerves in the umbilical cord; although probably elementary nervous tissue may exist in some form in it. For if it did not exist, then we should have irritability in these vessels existing without nervous influence. The investigation of the existence of nerve in any form in the umbilical cord and its vessels, thus becomes an interesting microscopical study, in reference to the physiological question of the dependence or independence of the contractility of vessels on the presence of nerves.

PERITONITIS IN THE FŒTUS IN UTERO.

(From Edinburgh Medical and Surgical Journal, October, 1838, p. 390.)

In the recent progress of pathology it has been amply proved that the fœtus in utero is liable to a considerable variety of morbid states. Of its diseases, some, we have reason to believe, are altogether of a functional nature; but in regard to this class of fœtal affections we as yet possess comparatively little information, because, excluded as the fœtus is, during its abode in utero, from any of our present means of observation, it is only when its morbid derangements produce symptoms of a very aggravated character, that we are enabled to recognize their existence during the continuance of intra-uterine life. Indeed for nearly all the limited knowledge which we as yet possess of the diseased conditions of the fœtal system, we are indebted principally if not entirely to pathological anatomy; and consequently the diseases of that system with which we are chiefly acquainted, are either such as are organic in their nature or that lead to an organic result.

Of all the various morbid actions which are liable to occur in the fœtus, *inflammation*, with the different pathological changes which it produces, seems to be one of the most important, both as regards the frequency of its occurrence, and the nature of the effects to which it gives rise. In the present communication, it is my intention to bring forward a series of cases to prove, that one species of inflammatory action, namely, *peritonitis*, forms a common variety of fœtal disease, and probably constitutes one of the more frequent causes of death of the fœtus during the latter months of pregnancy.

It may be necessary, however, to premise, that the investigation by pathological anatomy, of the presence and effects of inflammatory action, and indeed of all other morbid changes in the fœtus, is beset with unusual difficulties. When the fœtus labors under any morbid state which happens to prove fatal during its abode in utero, there generally elapses an interval of from five to twenty days before the uterine contractions, necessary for its expulsion, supervene. During this period a number of changes are liable to occur, which are calculated to mask or destroy the usual morbid appearances left by inflammation. Pathologists are now becoming fully aware that various injections, colorations, serous and sero-sanguinolent effusions and softenings of different tissues and organs, more or less perfectly resembling the corresponding alterations produced in the same parts by inflammation, are liable to be met with in the dead body of the adult, as the effects of merely chemical and physical causes acting during the latter hours of life, or after death. In the case of the dead fœtus retained in utero, we have not only the same causes producing the same results, but these results increased considerably in their degree and intensity by the longer period during which their causes are generally allowed to operate; and, besides, we have further to take into account the additional effects of the endosmosis of the liquor amnii, and of the blood and other fluids of the fœtus through the dead tissues of the body, and the continued maceration of these tissues in the effused and transuded fluids in which they are placed. In consequence of the operation of these and other causes, we have constantly found in our examination of fœtuses that had died several days before birth, the heart and large bloodvessels almost entirely emptied of blood, the different serous cavities of the body filled by an abundant sero-sanguinolent fluid, and the same fluid often collected in the general course of the cellular tissue, but more particularly in that of the scalp, while at the same time the different solid tissues are reduced more or less in consistence, and altered in color and physical appearance. We have seen tissues and membranes which from the morbid secretions existing upon them, we knew to have been the seats of acute morbid action immediately previous to death, macerated and blanched, and sometimes variously discolored from the imbibed bilious, intestinal, and other secretions; whilst, on the other hand, we have found other membranes and tissues of the body that had in all probability not been the seat of any morbid state during life, more or less deeply reddened, injected, tumefied, and softened. Indeed, we had not been long engaged in this field of pathological inquiry, before we became fully convinced that we were not entitled to consider any tissue or organ in the dead fœtus as having been the seat of inflammation during ife, unless we could detect in that tissue or organ one or other of hose characteristic morbid secretions, or more distinct permanent changes of structure, which are recognized as the distinctive organic results of inflammatory action, such as the deposition of coagulable ymph, and the indurations, thickenings, &c., to which this deposit gives rise, the effusion of more or less serous or sero-albuminous luids, or of true purulent matter, ulceration, &c. Accordingly, in he details of the following cases, it will be observed that we have only entered as genuine examples of inflammation of the peritoneum those instances in which there existed upon that membrane, or in its cavity, one or other of the organic products of inflammation to which we have just alluded; but more particularly, effused coagulable lymph, and the adhesions and pseudo-membranes which such lymph so readily produces, when thrown out upon serous surfaces.

CASES OF ACUTE PERITONITIS.

Case I.—On the morning of the 15th October, 1836, my attendance was requested at the Lying-in Hospital in a case of twins. The first child had been born after a natural and easy labor. On examination, I found the second child presenting by the head, and labor pains having spontaneously recurred, it was expelled in about forty minutes after the birth of the first. The first-born child was living, healthy, and well formed. The second had apparently been dead for some days: its cuticle could be easily peeled off, and was raised into bullæ at various parts by a sero-sanguinolent effusion beneath it. Its body, however, was by no means emaciated, but as plump and fat as that of the first child.

Being unable to discover in the portion of the double placenta belonging to the second child any disease that could account for its death, I opened its body, twelve hours after birth, in presence of Mr. Scott, house-surgeon to the hospital, and Dr. Pollexfen. Besides the subcuticular effusions already alluded to, there was a considerable accumulation of serous and sero-sanguinolent effusion in the cellular tissue in different parts of the body, and in the cavities of the pleuræ, pericardium, and peritoneum. Over the surface of the last-mentioned membrane (the peritoneum) there were also deposited several isolated patches of soft coagulable lymph, which had produced at various points adhesion of the folds of the intestine to one another, and to the internal serous surface of the abdominal parietes.

In this case, the consistence and other characters of the effused coagulable lymph were such as sufficiently indicated that it was the result of recent and acute peritoneal inflammation. That the child had perished of an acute disease was still further attested by the general plump condition of its body, and by the large deposit of fat in the subcutaneous tissue and other parts, which was observed on dissection.

The mother, Ellen C———, was a healthy young woman, of twenty-two years of age. It was her first pregnancy. She was not aware of having received any physical injury, or of having experienced any particular mental emotion, that could enable her in the least degree to account for the death of the second child; and her feelings had never led her to suspect that any change had occurred in the contents of the uterus in the last periods of pregnancy.

CASE II.—I was sent for, April 3, 1837, by Dr. Allen, house-surgeon to the Lying-in Hospital, to a case of difficult labor which he was attending in Blackfriars Wynd. The left arm was presenting, and the membranes being entire and the passages well dilated, I at once passed up my hand into the uterus, turned, and delivered the child by the feet. It had been evidently dead for some time, as shown by the detachment of the cuticle from the abdomen and other parts. From the appearance of the child and the calculations of the mother, it seemed to have been born a few days before the seventh month.

On opening the body next day, Tuesday the 4th, along with Dr. Allan, Dr. Charles Bell, and Dr. R. Paterson, we found an effusion of reddish serum within the sacs of the pleuræ and pericardium; but the lungs and heart were healthy. There was a similar effusion within the abdominal cavity; and on the peritoneum covering the convex surface of the liver, we observed various distinct patches of coagulable lymph, with corresponding points of a similar effusion upon the serous membrane lining the abdominal parietes and diaphragm. The adhesions formed between the opposed surfaces of peritoneum at the points of the deposit were so slight, and the coagulable lymph forming them so soft in consistence, that they readily gave way under the manipulations required for exposing the contents of the abdomen. The mesenteric glands were large, some of them equalling in size the half of a small split pea.

The mother of the child was 36 years of age, and had borne six living children, besides having had a miscarriage several years previously, and another on the 13th of June last (1836). In this last abortion the child was expelled about the sixth or seventh month, and she herself attributes its death, and that of the fœtus whose history I have given, to an excess of hard labor, and more particularly to the exertion required in carrying loads of water up a long stair of three stories. In her first pregnancies she had not been exposed to such toil. In her last pregnancy, the motions of the fœtus had continued from the first of January, when they were first felt, till eleven days before she was delivered. For two or three days previous to this last date, it moved "a great deal more," to use her own expression, "than usual."

CASE III.—December 23, 1837. I examined, with Dr. Banks and Mr. Stewart, the body of a dead-born child, which had been sent to me for dissection on the previous day by Mr. Brown. It was a fœtus of about the seventh month; and the state of the cuticle and tissues in general showed that it had been dead for some time before birth.

The thoracic organs were healthy, though there was the usual quantity of pseudo-morbid reddish serous fluid in its serous cavities. On laying back the abdominal parietes, a patch of soft coagulable lymph was seen on the abdominal peritoneum near the right iliac region, and on further examination this was found to have formed a portion of a quantity of the same deposit, effused around the caput cœcum and its vermiform appendage. The peritoneum covering the liver and other remaining parts of the intestinal tube seemed healthy; but the peritoneal coat of the spleen was covered, more particularly on its outer or convex surface, with a thickish layer of coagulable lymph, which united it to the corresponding portion of the abdominal parietes by a large web of false membrane.

Mary C———, the mother of this child, is a strong and healthy young woman of 22 years of age. She had previously borne one living child. About a fortnight before the birth of the dead-born fœtus above described, she had a fall down stairs; and to this accident she at the time ascribed the death of the infant, as it ceased to move in a day or two afterwards. At the present date, July 15th, she cannot recollect if the motions of the fœtus were greater than natural after the fall, previously to their final cessation.

Case IV.—On dissecting, January 8, 1838, the body of a dead-born male fœtus which had been for two or three months in my possession, I found the most marked effects of inflammation in almost all parts of the peritoneal cavity.[1]

The upper or convex surface of the liver, but more particularly of its right lobe, adhered to the corresponding surface of the diaphragm. The left edge of its left lobe was united by effused lymph to the spleen, and this latter organ was further morbidly adherent along its external surface, partly to the large intestine, and partly to the abdominal peritoneum. The omentum was connected at one or two points by coagulable lymph, to the concave surface of the liver, and to the inferior part of the spleen. The small and large intestines were agglomerated together into a mass, and their corresponding surfaces intimately united at numerous points by effused lymph. In consequence of these adhesions the jejunum was intimately united to the sigmoid flexure of the colon. Both the tunicæ vaginales of the testes still communicated with the cavity of the peritoneum, and the serous surface of the left one was partially coated by a layer of coagulable lymph, or thin false membrane. The other cavities of the body were healthy. The child was not in an emaciated state.

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[1] Recent preparations of the abdominal viscera, showing the particular morbid appearances described in Cases III. and IV., were shown at a meeting of the Medico-Chirurgical Society, and are still preserved.

I regret that I have not any notes of the history of the mother of this child.

CASE V.—July 28, 1838. I examined in the Lock Hospital, with Drs. G. Weir and Allan, and Messrs. Scott and Bannatine, the body of a child of which one of the patients in the house had been delivered on the preceding evening.

The cuticle was loose and easily separated. The cavities of the pleuræ and pericardium were filled with a reddish serous effusion; but these membranes, with the exception of numerous points of purpurous effusion beneath them, were otherwise quite healthy. The purpurous spots were seen under both the pleura pulmonalis and costalis. The cavity of the peritoneum contained upwards of an ounce of a still deeper-colored reddish serous effusion, along with one or two clots of blood, which appeared to have come from a ruptured point in the lower surface of the right lobe of the liver. The edges of the laceration were partly reunited by coagulable lymph. A considerable portion of the liver in the neighborhood of this part was much congested, more deeply colored, and softer than the remainder of the viscus. The gall-bladder was filled with a quantity of viscid bile, and its coats were thickened to about a line and a half or two lines, by a serous effusion into its cellular tissue. The surface of the abdominal peritoneum was coated by a beautiful lacelike and adherent layer of tough coagulable lymph, which was of considerable thickness at some points, and threw out long lines or films that were in contact with the surface of several of the abdominal viscera, but not apparently in any place adherent to them. This layer of lymph was particularly abundant in the left hypochondriac and in the iliac regions. Several loose long films and masses of it were seen also among the intestines and upon the mesentery. The mesenteric glands were large. All the other abdominal and pelvic organs were healthy, with the exception of one of those small pediculated serous cysts adhering to the right broad ligament of the uterus, which are so common in this part of the body of the adult female.

Helen G——, the mother of the child, is 19 years of age, and naturally of a healthy constitution, but she has now suffered under four different attacks of venereal disease. About fifteen months ago she had severe ulcerated sore throat and other secondary symptoms. During the course of the present pregnancy she entered, April 5th, the Lock Hospital under my care, affected with an ulcer and gonorrhœa of five days standing. She was dismissed, cured, on the 20th of the same month. She re-entered the hospital 12th July, suffering under another recent attack of gonorrhœa, and with two slight ulcers; but was nearly well again when labor pains supervened on the 27th. The liquor amnii was in great quantity. The placenta was pretty firm and healthy, and, had not the bleached anæmic appearance which it usually presents in cases in which the child has been dead for a week or two. The motions of the fœtus were not very sensibly felt by the mother after the 13th of July, but subsequently to that period they were repeatedly discovered by applying the hand to the abdomen, and the fœtal heart was most distinctly heard by myself and several of the pupils of the hospital, only five or six days before delivery. I counted its beats at that time at 25 in the 10 seconds. The mother was not aware of having been exposed, during the later periods of pregnancy, in any such way as could account for the death of the fœtus. She had an abortion about eighteen months ago, when passing through an attack of typhus fever.

Case VI.—On opening a dead-born male fœtus of the seventh month, which had presented by the breech, and been delivered by Dr. Allan, I found, with that gentleman and Messrs. Scott and Bannatine, the following morbid appearances, on inspecting the body the day after birth, July 29th, 1838.

The cuticle was separated in many parts, and could be easily removed. The face of the fœtus was swollen and deformed with œdema; and the upper extremities, but particularly the hands, were also anasarcous. The feet and legs were likewise dropsical, though in a less marked degree, and there was a considerable amount of hydrocele. The cellular tissue of the scalp and loins was infiltrated with the usual reddish serous effusion. The cavities of the pleuræ and pericardium contained a similar fluid, but these serous membranes themselves, as well as the lungs and heart, were perfectly healthy. The cavity of the peritoneum was filled with a considerable quantity of the same effusion, and the surface of the abdominal peritoneum, more particularly on the right side, was covered with a lace-like layer of adherent coagulable lymph, which presented appearances and characters very exactly resembling those seen in the preceding case. Several masses and filaments of lymph were also seen among the convolutions of the intestine, and produced a pretty strong adhesion between them at one or two points. The spleen was large, and four drachms, thirty-five grains in weight; its surface had several patches of lymph upon it, and the inferior portion of the organ was intimately and extensively united by morbid adhesions to the larger omentum. The mesenteric glands were large and well developed. The other abdominal organs were healthy.

The mother, Elizabeth H——, is a robust woman of 42 years of age. The present is her fourth child. The first was alive and at the full time. The second was believed to be at the full time, but was born dead. The third, like the present, was at the seventh month, and also dead at birth. She confesses to having been affected with venereal disease, but would not afford such information as to enable us to judge of the form of the affection, or the particular period at which she suffered from it. She attributes the death of the present child, to her having fallen down a flight of stairs about a fortnight before delivery. The placenta was adherent.

Case VII.—I had an opportunity of examining, February 21st, 1838, the following case of fœtal peritonitis with Dr. Fisher, under whose care the mother was delivered.

The appearance of the body of the infant was such as indicated that it had been dead for some time in utero.

The epidermis was peeling off in various parts. The muscles of the limbs were soft and flabby, and the periosteum was separated from the bones at various points. There was no serous effusion into the subcutaneous cellular tissue of the trunk and extremities, but the sero-sanguineous extravasation beneath the scalp, so frequent in dead and retained fœtuses, was strongly marked. Some effusion existed in the thoracic serous cavities, but the lungs and heart were healthy in structure. In the cavity of the peritoneum there existed a turbid effusion, with large flakes of coagulable lymph in it. On the upper or convex surface of the liver, a few non-adherent patches of lymph were observable; and there was the same morbid effusion on the lower surface, particularly along the edge of the left lobe, where the lymph was firmer and adherent. The spleen had numerous patches of slightly adherent lymph scattered over its peritoneal surface. Similar patches were seen on the peritoneum of the colon, and produced at one point, near the sigmoid flexure, adhesion of two of the contiguous folds of that bowel. The peritoneal surfaces of the small intestines, mesentery, and abdominal parietes, had numerous flakes of coagulable lymph deposited upon them. Patches of lymph were adhering to the right Fallopian tube. The mesenteric glands were large. The mucous membrane of the stomach and intestine appeared healthy.

Ellen S——, the mother of this infant, is a stout young woman of 20 years of age; and had three years before a dead-born child at about the seventh month. It was believed to have been dead for some time before its expulsion from the uterus. In the present pregnancy, the mother calculated that she was between the sixth and seventh month of utero-gestation when labor came on. She had not felt the child move for about twenty-three or twenty-four days before delivery; but during the two days preceding the cessation of its motions, these motions, she alleges, were distinctly much greater than usual. She cannot account in any way for the death of the child, except it be that she had a fall upon her right side the day on which its motions ceased; but the increased and morbid movements of the fœtus had been sensibly felt during the whole day preceding this accident.

Her general health was good during pregnancy, with the exception of occasional nausea, vomiting, and pain in the back. She denies that she ever suffered under any form of venereal complaint.

CASE VIII.—I inspected, January 10, 1838, with Dr. Fairbairn and Dr. John Reid, the body of a fœtus that had died some time before birth. Its cuticle was separating in different parts, but its cellular texture was not much infiltrated.

The pleuræ and pericardium contained a reddish serous fluid, but the thoracic organs themselves were healthy. On opening the abdomen a considerable quantity of sero-sanguinolent liquid escaped, having numerous flocculi and large shreds of coagulable lymph floating in it. Similar shreds of loose coagulable lymph were found lying upon the peritoneal surface of the intestines at various points, but nowhere did this lymph seem to be adherent to that surface. On passing the handle of the scalpel among the loose intestines, it came out covered with patches and shreds of the substance in question.

The mother of the child, Mrs. M——, aged 36, had been exposed to much fatigue and hardship during pregnancy, and her health had been very infirm during the whole period. The movements of the child ceased altogether, fifteen days before its birth. On the day of their cessation it had moved with great and unusual violence, as if, to use the patient's own expression, "the infant were coming out at her side." Two days previously to this occurrence, the mother had been obliged to sleep in wet clothes after a long day's travelling on foot. She had borne two living children previously.

CASE IX.—In a male fœtus between the fourth and fifth month, which I examined, July 7, 1838, with Dr. Allan and Mr. Bannatine, a few hours after its expulsion from the uterus, the peritoneal cavity contained a quantity of serous effusion, having numerous flocculi and shreds of coagulable lymph floating in it. Patches and small masses of this lymph were deposited in considerable abundance upon the peritoneum, covering the abdominal parietes and different abdominal viscera, but nowhere did we find any of this exudation adherent to the serous surface. The left pleura contained a quantity of clear, limpid, reddish-colored serosity, without any flocculi floating in it, or deposited upon the surface of the membrane. The fluid in the cavity of the right pleura was turbid, and contained numerous minute masses of animal matter, the source of which was readily traced to a softened and pulpy condition of the corresponding lung, and to part of the broken down substance of the organ having escaped through a rupture in the pleura. Whether this state was the effect of intra-uterine disease, or of physical injury during or after birth, seemed to us impossible to be determined.

The mother, Frances G——, a healthy young woman of 18 years of age, had been under my care in the Lock Hospital from the 12th of April, 1838, to the 26th of May. She was then suffering under a recent and severe attack of gonorrhœa, and had the remains of a chronic syphilitic eruption upon the skin. During her pregnancy she had been comparatively well in her general health till about a fortnight ago, when she had several attacks of chilliness and slight fever, with pain in the uterine tumor, increased by stooping. These symptoms lasted for three or four days, but were not so severe as to confine her to bed. Her last menstruation occurred five months and a few days ago. The fœtus was six ounces and two drachms in weight, and exactly six inches and a half in length. It had seemingly been dead for some days. The placenta was healthy, but in that white anæmic state which is generally seen in those cases of abortion and premature labor in which the infant has been for some time dead in utero.

Case X.—A case of peritonitis similar in its anatomical character to the two last, is mentioned by Cruveilhier as having been met with by him in the instance of a child that was born with the abdomen large, and evidently containing a quantity of liquid. Death occurred about three hours after birth. On opening the abdomen there was found a great quantity of yellowish serosity, with some pseudo-membranous flocculi in the cavity of the peritoneum. The intestines and stomach were extremely contracted. The large and small intestines were filled with meconium. The stomach contained thick white mucus, like coagulated milk (but the child had not swallowed a drop of that fluid), and this appearance of the gastric mucus may, perhaps, M. Cruveilhier suggests as a query, be the effect of inflammatory action in the organ. The internal surface of the stomach presented a very marked punctuated redness, in some parts of an irregular form, in others disposed in lines. The liver was large, and the spleen was also increased in size, and dark-colored. The gall-bladder contained only some colorless mucus. Indurated and enlarged lymphatic glands surrounded the hepatic duct.

The lungs contained numerous small, but unequally sized red, spheroidal masses of inflammatory induration, like crude tubercle, both on their surface and in their substance.[1]

Case XI.—For the details of the following interesting case of congenital peritonitis, I am indebted to my friend Mr. Scott, who had an opportunity of seeing the child during life, and of examining its body after death, with Mr. Logan.

During the act of parturition, after the birth of the head, the expulsion of the body of the infant was prevented for some time by the distension of the abdomen. On examining the child shortly after birth, Mr. Scott found the belly marked with spots of purpurous or hemorrhagic effusion, especially at the sides; it was very tense, and fluctuation was indistinctly felt. Both hands were œdematous, the left more so than the right. There was no œdema of the lower extremities. The child was plump and fat, but cried only in the feeblest manner, and kept its legs firmly drawn up. It survived for twenty-eight hours only after birth; and, before death, œdema of the scrotum and penis took place, with an erysipelatous blush extending to the lower part of the abdomen. The bowels of the child had never been opened, though castor oil had been twice administered to it; and little, if any urine whatever had been evacuated. The infant was large, and had evidently reached the full term of utero-gestation.

On opening the body after death, some air and a quantity of fluid escaped, when an incision was made into the cavity of the abdomen. "Marks of inflammation," as Mr. Scott observes in the notes with which he has favored me, "were obvious on the surfaces both of the abdominal and intestinal peritoneum, and from these surfaces I collected about a teaspoonful of flaky puriform matter, exactly resembling that which I have found in my dissections of adult females who have died of puerperal peritonitis." The intestinal canal was pervious throughout, but the stomach, upper intestines, and cœcum were much distended with air.

The left lung was not at all inflated; the right seemed to have been fully used. Spots of purpura were seen on the pleura of both sides. The internal surface of the contracted urinary bladder was covered with similar spots.

Mrs. P——, the mother of this infant, had previously borne three living children. A fortnight before her confinement with the dead-born child above described, she was exposed to cold and wet in walking from Portobello at night. On reaching home, she was seized with shiverings, which recurred frequently during the two following weeks. She did not feel the motions of the child after the night on which she first had rigors, and she herself believed that it had been killed by the exposure to which she had been that day subjected.

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[1] Anatomie Pathologique, livrais. xv. p. 2.

CASE XII.—In a memoir read to the Royal Academy of Medicine in 1825, M. Veron, among other cases proving the existence of inflammatory and other diseases in intra-uterine life, adduced an instance of peritonitis, analogous in several respects to the case which we have last detailed. The case was observed in 1822, with M. Baron, on the body of an infant who had been brought dead to the Foundling Hospital at Paris. From the state of the umbilical cord and the appearance of the child, it seemed scarcely to have survived a day after birth. On opening the peritoneal cavity, it was found to contain a quantity of purulent serosity, but not in any great abundance. There was an albuminous layer or deposit of coagulable lymph of about a line in thickness on the surface of the cavity, and so adherent to the membrane, that all the abdominal organs and intestines were glued together, and formed only one mass. The serous membrane itself was intensely red. The intestinal mucous membrane presented no such color.[1]

CASE XIII.—In the Journal Général de Médecine, M. Brachet has detailed the following well-marked case of acute inflammation of the peritoneum and substance of the liver, in a fœtus which was born dead at the full term of pregnancy. The body of the child was large and well developed, but its flesh was soft, its abdomen large and distended, and the skin had a yellow tint.

On inspection, the contents of the cranium and thorax were found healthy. The cavity of the peritoneum contained some reddish serosity, and its right superior part was occupied by some filamentous shreds of coagulable lymph, forming the rudiments of false membranes. The superior or convex surface of the liver was rugous, and adhered over almost all its right lobe to the corresponding portions of the abdominal parietes, by means of patches of pseudomembrane that were not yet organized. The liver itself was very red and larger than natural; its tissue was friable and softened in its enlarged right lobe; on dividing it, an inodorous gray reddish fluid, resembling purulent sanies, flowed from the cut surface. The other abdominal viscera were healthy, with the exception of a reddish state of the omentum.

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[1] Observations sur les Maladies des Enfans, Paris, 1825, p. 17.

The mother of this infant had, during the nine months of pregnancy, experienced only the usual indisposition attendant upon that state. Lively fœtal motions had been felt up to the middle of the eighth month. From that period till the supervention of the pains of labor, they had become less and less sensible, and at last had ceased altogether during the act of parturition, which was extended to eighteen hours. It was her first pregnancy. The placenta was very large, and slightly gorged with blood, but in other respects healthy.[1]

Case XIV.—In 1821, a well-marked case of fœtal peritonitis and enteritis was observed by Chaussier in the Hospice de la Maternité, and reported by him to the Société de Médecine.[2]

The subject of the case, a male child, born about the seventh month, was well formed, and presented even a degree of plumpness. The abdomen was more distended and elastic than natural; and a glyster was given to it and returned with little effect. In the subcutaneous cellular tissue there was a slight serous infiltration, more particularly in the lower extremities. Its respiration was labored, and it died in an hour and a half after birth.

On opening the abdomen, about ten drachms of a yellowish viscid serosity, containing some small flocculi, flowed out. The omentum appeared somewhat thickened. The convolutions of the small intestines were so united and adherent to one another by a tenacious layer of concrete lymph, that they were formed into a single roundish mass, encircled by the course of the colon. On examining more minutely the small intestines, the cellular coat was found pale, thickened, and friable, and penetrated by a semifluid whitish matter or lymph, which separated it from the peritoneal coat. The cavity of the intestine was filled with grayish mucus; and the mucous membrane appeared thickened, and traversed here and there by small patches or circles of vascular injection. The other viscera of the abdomen, and those of the head and thorax, seemed healthy.

The mother of this child was a woman of 22 years of age, and pregnant for the first time. She had always enjoyed the best health, and had not met with any accident, or experienced any disagreeable symptom during the whole course of pregnancy. The labor was natural, speedy, and easy.

[1] Journal Général de Médecine, tom. cii. 1828, p. 43.

[2] Bulletin de la Faculté et de la Societé de Médecine, tom. x. 1821, p. 242.

The same case is mentioned by Billard (p. 242), as described by Dugès in his Recherches sur les Maladies les plus importantes, &c., des Enfans nouveau-nés, Paris, 1821.

CASE XV.-XVII.—After quoting the foregoing case (XIV.) as given by Dugès, M. Billard, in his excellent treatise on the Diseases of Children, adds, that he "had found peritonitis to the same degree in three infants who died a short time after birth, and who were all fresh and vigorous. In none of these three cases had any symptoms of the peritonitis been observed during life, and it was only by post-mortem inspection that the cause of death was discovered. In one of them there was an abundant sero-purulent effusion, and the intestinal convolutions, which were very red exteriorly, were beginning to contract adhesions to one another." M. Billard does not state what particular morbid appearances were presented by the other two cases.[1]

CASE XVIII.—In the slight summary which Professor Carus, of Dresden, has given, in his well-known work on Midwifery,[2] of the diseases of the fœtus, he states, "I have observed on the peritoneum of several children born dropsical, perceptible inflammation in several places, and once even the effusion of plastic lymph and adhesion."

CASES OF CHRONIC PERITONITIS.

CASES XIX., XX.—M. Billard, in the work already referred to, has alluded to two cases that had fallen under his own observation, of infants who died shortly after birth, and in whose bodies he found coagulable lymph effused upon the peritoneum in such a solid form as to indicate the existence of an inflammatory action, which had run through its different stages during intra-uterine life. The first of these infants died in eighteen, and the other in twenty-four hours after birth; and in both, solid, and apparently old, adhesions existed among the different intestinal convolutions. In one of them, the anterior or convex surface of the liver adhered by four very tough, although very slender, filaments to the anterior wall of the abdomen. One of the infants was lean, small, and very pallid; but the other had the usual plumpness of the new-born child.[3]

CASE XXI.—In the second volume of his Pathological Anatomy, M. Andral mentions an instance in which he found all the intestines agglutinated together by intimate and very firm cellular adhesions, the result, as we presume, of old peritonitis, in an infant only two days old.[4]

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[1] Traité des Maladies des Enfans, Paris, 1837, p. 479.

[2] Lehrbuch de Gynækologie, Bd. ii. S. 251.

[3] Traité, &c., loc. cit.

[4] Anatomie Pathologique, tom. ii. p. 737.

CASE XXII.—The best-marked case, however, of chronic peritonitis in the fœtus which we have been able to meet with, is one casually described by Morgagni. The subject of the case was an infant, who was brought to him with the umbilical cord not tied, and consequently that had probably not lived for any length of time after birth. With respect to size, it seemed to be of less than the full time. The body was not in any way decayed or putrid.

The lungs were of a red color, degenerating into a dark brown, and parts of them when laid upon water immediately sunk to the bottom.

The abdominal cavity was filled with a large quantity of black blood, which was subsequently traced to have escaped from an erosion or laceration of considerable extent upon the concave surface of the liver. The whole of the upper or convex surface of the liver adhered to the diaphragm and corresponding parts of the abdominal parietes. At first sight, the mesentery and the intestinal tube seemed, with the exception of the rectum and lower part of the colon alone, to be entirely wanting; but, on further examination, these apparently deficient parts were found agglomerated into a small mass, under the lower surface of the liver, and covered over by a false membrane. This pseudo-membrane was of considerable thickness, of a tenacious consistence, and rendered rough by a kind of granular deposit. There was meconium in some of the upper intestines, but none in the rectum.[1]

GENERAL SUMMARY OF RESULTS.

The various cases which I have cited in the preceding pages will, I believe, be found to afford sufficient evidence for establishing the pathological fact, that the fœtus in utero is occasionally the subject of peritoneal inflammation; and by an analysis of the same and of other additional data, I shall now endeavor to trace out some of the leading and general circumstances regarding the morbid appearances left by the disease—the causes which are liable to produce it —the symptoms which most frequently indicate its presence—its most common terms of duration—the periods of gestation at which it most commonly occurs—and its effects upon the life of the fœtus. We reserve the discussion of its more indirect effects upon other morbid states of the abdominal organs for a future occasion.

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[1] De Causis et Sedibus Morborum, Ep. lxvii. § 17.

MORBID APPEARANCES OBSERVED ON DISSECTION.

The nature of the morbid inflammatory effusions or products observed in the cases of fœtal peritonitis which we have related, has varied considerably.

In two cases (XVI., XVII.) the particular morbid appearances which were met with are not specified. In three (XI., XII., XV.) the morbid effusions into the peritoneal cavity presented more or less of a puriform character, combined with the presence of coagulable lymph; and in all the remaining cases, this latter morbid product (coagulable lymph or fibrin) was found either alone, or accompanied (as in Cases I., II., V., VI., VII., VIII., &c.) with a larger or smaller quantity of serous effusion. The coagulable lymph again has been seen in different cases under different forms. In three instances (VIII., IX., X.) it consisted of unadherent flocculi and membranous shreds of various sizes, floating in the serous effusion, or lying on the surface of the peritoneal membrane; in others (I., II., XIII., XVI.) it was still soft and pulpy, but was attached to the serous surfaces on which it was deposited, and produced even slight and lacerable morbid adhesions between some of the opposed points of these surfaces; and in four cases (V., VI., VII., XXIII.) it was found to present both of these characters, being in part adherent and in part still loose and unattached to the serous membrane.

In another set of cases again (III., V., VI., XIV.) the adherent coagulable lymph was still more advanced towards the process of organization, and had assumed a somewhat firmer and more membranous character; in some instances being effused in such abundance as almost to agglutinate together into one mass (IV., XII., XIV., XXI.), a greater or less number of the abdominal organs covered by the peritoneum, or, where the effusion was more partial and limited, appearing in the form of a membranous (III.) or lace-like (VI.) web, or of threads or filaments (XIX.) passing between some of the morbidly attached surfaces and organs.

Lastly, in the more chronic cases, the effused coagulable lymph may, as we have seen, pass into a still more solid and pseudo-membranous form (XIX., XX., XXI.); or appear, as in the remarkable case quoted from Morgagni, under the character of a false membrane investing almost all the abdominal viscera, and containing in its substance some morbid bodies, not improbably analogous to those tubercular deposits which are so often observed in cases of chronic peritonitis in the adult, whilst, at the same time, this morbid membrane had exercised upon the included viscera, that contractile power which is possessed in a greater or less degree by all organized lymph, whether it exist in the form of granulated cicatrices upon the external or internal surfaces of the body, or as organized false membranes, or morbidly developed fibrous tissue.

Complications with coexistent inflammatory disease in other abdominal organs and tissues, have been observed in only a very few of the cases. In one instance (XIV.), besides the layer of coagulable lymph upon the free surface of the peritoneum, there was a considerable effusion of semifluid whitish matter (concrete albumen, Dugès), into the cellular tissue of the smaller intestines, producing a degree of thickening and friability in the coats of the bowel. In a second case (XIII.), the peritoneal inflammation was accompanied by hepatitis in the stage of softening and incipient purulent infiltration; in another to be presently quoted (XXIII.) the morbid changes in the same organ were of a more chronic character, the coats of the liver being opaque and somewhat thickened, and the organ itself reduced in size. In one instance (X.), there were found some of those small masses of inflammatory induration in the lungs which form the most common type of pneumonia in the fœtus and infant.

In two cases (V., XXII.), there were in the peritoneal cavity coagula of blood, which had apparently proceeded in both instances from a rupture or laceration upon the concave surface of the liver. In the first of these cases (V.), the edges of the laceration had again become agglutinated together, a fact showing that the lesion must have occurred several days before death; and the congestion observed in the surrounding hepatic tissue was so great, that it might probably have acted as the predisponent, if not as the exciting, cause of the laceration.

In two instances (VI., X.), the spleen was unusually large, and in a case mentioned by Petit-Mengin,[1] which we shall have occasion to quote in our next communication, as an instance of the combination of ascites and peritonitis in the fœtus, the spleen was enormously hypertrophied, and had its peritoneal surface morbidly adherent.

In four of the cases (II., V., VI., VIII.), I have mentioned the very large size of the mesenteric glands. This is an appearance which I have now met with so often in my dissections of dead-born children, that I should feel inclined to regard it rather as a state natural to the fœtus than otherwise. Certainly, according to my own experience in such cases, the glands, if morbid in any way, are simply hypertrophied, and do not, as Oehler[2] would seem to imply, show some of the characters of scrofulous degeneration.

In two of the cases which I have related (V., XI.), minute hemorrhagic effusions, similar to those seen in Purpura Hemorrhagica, were observed in some of the internal organs of the body. This particular morbid appearance would, according to the experience of Cruveilhier,[1] seem to be not unfrequent in the body of the fœtuses who have died in utero.

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[1] Gazette Médicale de Paris, June, 1833.

[2] Prolegomena in Embryonis Humani Pathologiam, pp. 34 and 44.

The *extent* of the inflammatory action in fœtal peritonitis, as shown by the post-mortem appearances, appears in the majority of cases to have been pretty general over the peritoneal surface; but occasionally (as in Cases II. and III.) we find it more or less limited to particular localities and portions of the membrane, and we shall afterwards have occasion to point out the pathological importance of this fact, in reference to the production of congenital hernia by local inflammatory adhesions formed between the peritoneal surface of the descending testicle, and some of the contiguous abdominal viscera.

EXCITING CAUSES OF FŒTAL PERITONITIS.

On the nature of the causes of peritonitis and other inflammations in the fœtus, we as yet possess little accurate information. Internal inflammatory diseases in the adult are comparatively seldom the result of noxious agencies applied directly to the organ or tissue which is the seat of the inflammatory action, but are generally the result, as we think might be shown if this were a fit opportunity for discussing such a subject, of a variety of intervening morbid alterations, which originate in the first instance in derangements of the secretions and other functions of the part to which the external exciting power is applied, and that subsequently react through it upon the economy in general, or upon that particular part of it in which the inflammation ultimately becomes located. These intermediate morbid states seem further capable, in different instances, of being produced by very different morbific agencies; and may probably in the fœtus, as in the adult, occasionally consist in, or be excited by, derangements in some of the natural secretory and excretory actions of the fœtal economy, as in the non-elimination of different matters from the fœtal circulation in the placenta of the mother, or in the introduction through the same channel of morbific substances previously existing in her system. We can have little doubt but that in the latter mode, the particular poisons exciting the specific inflammations characteristic of plague, small-pox, syphilis, &c., are conveyed from the mother to the fœtus in those cases in which the fœtus is attacked in utero by these diseases.

Causes more particularly referable to the conditions of the Mother.—In some of the cases of fœtal peritonitis which I have detailed, the mother had been exposed to severe labor (II.), or fatigue and exposure to cold and moisture (VII., XI.), or bodily injury (III., VI.? VII. ?) during her gestation; in two cases (VIII., XXIII.) there existed general ill health during the whole of that period; and in one of these (XXIII.) the mother herself was twice attacked with peritonitis during the course of pregnancy. In two of the cases (V., IX.), the mothers had an attack of gonorrhœa during the period of utero-gestation, along with a syphilitic eruption in the one instance (IX.), and ulcers in the other (V.) A third (VI.), confessed that she had suffered from venereal disease; and the line pursued by others of the number (III., VII., and I believe also IV.), was such as certainly freely exposed them to syphilitic infection. Indeed it appears to me highly probable from the investigations which I have already made upon this point, that a great proportion of those children of syphilitic mothers that die in the latter months of pregnancy, may yet be shown to have perished under attacks of peritoneal inflammation.

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[1] Anatomie Pathologique du Corps Humain, livr. xv.

But before attributing to this, or to any of the above causes on the part of the mother, too great and exclusive an influence in the production of peritoneal inflammation in the fœtus, it must be recollected, that in other instances which we have brought forwards, as for example, in Cases I., XIII., XIV., XXIV., the mother was not aware of being in any way exposed to any known morbific influence, and had not been the subject of any particular indisposition, either during pregnancy, or antecedently to it. Besides, that the disease in the fœtus may occur altogether independently of any morbid state of the maternal system, and from causes strictly originating in, and confined to, the fœtal economy itself, would seem to be shown by the first instance which we have related, where, in a case of twins, *one child only* was affected, whilst the other was healthy and lively, although they were connected to the mother by one common placenta, and consequently were both exposed equally to any morbific influence, which the state of her economy might have been capable of exerting upon them. In some of the instances we have cited, the children born with peritonitis were, as in the case (I.) just now referred to, the product of a first pregnancy, and the offspring of a healthy mother (XIII., XIV.) In three instances the mothers had previously borne one or more living children (III., VII., XI.) But others of them (II., III., V., VIII.) had already previously suffered from the abortion or premature delivery of a dead fœtus or fœtuses. In none of these latter instances have I as yet had an opportunity of examining two of the dead-born children of the same parents, to ascertain whether there may have been an identity of intra-uterine disease in them, but it seems not improbable, from other ascertained facts relative to intra-uterine pathology, that in certain cases such an identity of morbid action might be traced.

Causes Referable to the Conditions of the Fœtus.—In some instances, peritonitis in the fœtus would appear to be directly induced by morbid physical conditions of the abdominal viscera, and by irritant fluids accidentally applied to the peritoneal surface itself. Legouais and Dugès[1] are said to have met with cases in which peritonitis in the fœtus had been apparently produced by an internal strangulation of the intestines. When the urethral canal of the fœtus has been impervious, the urinary bladder has often been found greatly distended with an accumulation of urine, as seen in numerous cases recorded by Ruysch, Portal, Sandifort, Meckel, Vrolik, Steghlehner, Chaussier, Billard, Howship, Wilson, Lee, Montgomery, and others. In instances of this kind, the distended organ seems liable to give way under the great morbid dilatation of its cavity,[2] and the effusion of urine into the cavity of the abdomen consequent upon the perforation of the viscus, would appear, as in the adult, to be followed by severe and fatal peritoneal inflammation, as exemplified in the following case, recently detailed by Mr. King.[3]

CASE XXIII.—On opening the hydropic abdominal cavity of a fœtus of the fourth month, it was found to contain a considerable quantity of opaque viscid fluid, having numerous soft flakes of fibrinous matter floating in it. The natural gloss of the peritoneum was a good deal destroyed, and the surface was in parts slightly coated, as with fibrin. The liver was reduced in size, and had become much rounded in figure; its tunic was opaque and somewhat thickened. With this organ the bowels were collected into a bunch, in the middle of the superior part of the abdomen. The urinary bladder extended to the umbilicus, had a globular form, and was so distended as to be capable of containing above half a pint of water; its coats were decidedly thickened. A little behind its summit a perforation was found, around which the vesical tunics were very much reduced, as if by absorption from the pressure of distension. This ruptured opening of communication from the bladder into the peritoneum was a simple fissure, rather less than half an inch in length, and its margins were extremely thin. The ureters were enlarged, tortuous, and somewhat thickened. The kidneys were small, and not materially affected by the internal pressure. The urethra was imperforate from the prostate gland forwards. No very decided alteration was seen in any of the other organs.

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[1] Cyclopædia of Practical Medicine, vol. iii. p. 291, and Billard, p. 483.

[2] In one recent case which I had an opportunity of examining with Dr. John Moir, the muscular and mucous coats of the dilated bladder seemed entirely removed at one circumscribed point, and the peritoneal tunic alone remained to prevent the effusion of urine into the abdominal cavity.

[3] Guy's Hospital Reports, No. V. p. 508.

The mother of this child, aged 27, was of a strumous appearance, and menstruated irregularly. After having been married eighteen months, she became pregnant with the above infant. She appeared to suffer from peritonitis at an early period of her pregnancy, and subsequently continued more or less ailing and delicate. The premature parturition was preceded by an attack closely resembling peritonitis. She did not suppose herself with child until the time of her delivery. The fœtus, a male, was born dead, with a full ascitic abdomen, and the abdominal parietes very considerably distended and attenuated.

In two cases that we have detailed (V., XXII.) in which there were coagula of blood in the abdominal cavity, from a partial laceration of the substance of the liver, could the peritoneal inflammation have been excited in consequence of the effused blood acting as an irrritant upon the serous membrane?

In connection with these instances of inflammation of the peritoneum, originating in direct physical and chemical injury of the membrane itself, I may here mention, as cases in all probability referable to a somewhat similar principle, that I have repeatedly observed an effusion of patches of coagulable lymph upon the peritoneal surface of the intestines and other abdominal viscera, in instances of monstrosity consisting in the extroversion of these viscera from a partial deficiency of the abdominal parietes; and I have seen this both in the human fœtus, and in that of the lower animals. Thus, for example, I find that among the short notes which I made some years ago, of various cases of fœtal monstrosity contained in the museum of Guy's Hospital, London, I have incidentally marked the existence of an effusion of coagulable lymph upon some part of the peritoneum, as visible in three of the cases in which there was general extroversion or hernia of the contents of the abdomen, from the deficiency alluded to. One case (2542 A) is described as having "some fibres of coagulable lymph upon the peritoneal surface of the liver and protruded intestines; the head of the fœtus is much malformed, and probably it was one of those instances in which this part adhered by inflammatory false membranes to the inner surface of the placenta or amnion. There is also a strong thread or band, probably composed of organized lymph, attached to the skin over the external side of the left elbow." The second case is marked as a "fœtus with harelip and extroverted heart and abdominal viscera, and with apparently a few patches of coagulable lymph upon the peritoneum and pericardium:" and the third case

(2250 A), a malformed fœtal pig, is mentioned as having, among many other anomalies of structure, "the extroverted abdominal viscera partially surrounded by a transparent serous-like membrane, which has strings of coagulable lymph attached to it." I have now seen in other pathological collections, several additional instances of similar partial effusions of coagulable lymph upon the peritoneum, and even intimate morbid adhesions between the contiguous serous surfaces of such abdominal viscera, as happened to be protruded in cases of fœtuses with malformations resembling the above. We may here observe also, that Scarpa, in his Treatise on Hernia, mentions and represents[1] a case of umbilical hernia in the human fœtus in which a considerable portion of the jejunum adhered, no doubt in consequence of previous peritonitis, to the entrance of the hernial sac; and the same author, in another passage of his work,[2] points out the "firm adhesion" contracted by the protruded abdominal viscera to the hernial sac, in instances of congenital umbilical hernia, as one of the causes opposing reduction and leading to the early death of almost all those infants that are born affected with this disease.

SYMPTOMS OF FŒTAL PERITONITIS.

In the prefatory observations made to the present communication, I have already taken occasion to allude to the almost insuperable difficulties which are opposed to our arriving at any accurate knowledge of the symptomatology of intra-uterine diseases; and the present affection only affords too apposite an illustration of the remark in question.

In eleven only of the preceding cases have we any account whatever of the condition and feelings of the mother during the period of pregnancy. In four (I., VII., XII., XXI.), out of these eleven cases, nothing seems to have occurred that was calculated to direct the particular attention of the mother to anything peculiar in the condition of the fœtus; in three (III., V., X.), the cessation, about a fortnight before delivery, of the motions of the fœtus as felt by the mother, was the only circumstance remembered, and in one of these cases (V.), the fœtus certainly continued to live for some time after this occurrence; in another case (XI.), the motions of the infant became less and less sensible during the last two weeks of gestation; and in the three remaining instances (II., VII., VIII.), these motions, after being much and morbidly increased for two or three days, ceased entirely and rather suddenly, at a period varying from eleven (II.) and fifteen (V.) days, to upwards of three weeks (VI.), before delivery. This last combination of symptoms, namely, a great, but temporary, increased degree of the fœtal motions, attended occasionally with spurious pains, and followed by the sudden and final cessation of all perceptible movements on the part of the infant, may, we believe, be not unfrequently noticed in cases of acute and fatal peritonitis of the fœtus; but, at the same time, it must be held in recollection, that this same sequence of morbid phenomena is common to peritonitis, with all those diseases of the fœtus in utero which are similarly acute and fatal in their character, and consequently they cannot by any means be held as diagnostic marks of peritoneal inflammation alone. We omit here, as we have done in the detail of individual cases, the recapitulation of various well-known but equivocal symptoms in the maternal system, of the death of the child, such as rigors, a sense of weight in the tumor of the uterus, flattening of the abdomen, &c.; because these signs, when they do occur, can only be regarded at best as probable indications of the death of the fœtus, without leading in any way to a knowledge of the cause of that event.

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[1] Treatise on Hernia, Wishart's translation, p. 377, and plate xiv. fig. 2. [2] Ibid. p. 378, § 6.

When the child has been born alive, but affected with congenital peritonitis, it has sometimes, in the more chronic forms of the disease, been emaciated (XIX.) but not always so (XX.); and in the more acute cases, when any great degree of change is observed in the condition of the child in regard to its natural condition of fatness and plumpness, we shall in general be justified in ascribing it to other causes besides the peritoneal inflammation, as we know that this disease may even prove fatal without bringing down the state of the little patient in this respect (XIII., XIV.) In several cases the abdomen was swelled and fluctuating at birth (V., X., XI., XIII., XIV., XVIII.); sometimes even tense and tender to the touch (XXIV.) With the abdominal effusion a certain degree of hydrocele generally exists in the tunicæ vaginales of male infants (as in Case VI.); and in some there has also been observed a coexistent degree of dropsical swelling in other parts of the body, as in the hands (XI.), in the upper extremities and face (VI.) in particular, or in the lower extremities, and beneath the skin of the whole body (XIV.)

In two of the cases, the children's skin presented at birth the yellow discoloration of jaundice. In one of them, that was dead-born (XIII.), the liver on inspection was found to be the seat of acute inflammation, and commencing purulent infiltration.[1] In the second case (XXIV.), which will be subsequently more particularly described, the child was born alive and survived.

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[1] Baumes, in his Traité de l'Ictere des Enfans de Naissance (Paris, 1806), mentions an interesting case (p. 45) of congenital jaundice complicated with hepatic inflammatory disease. A mother, who was herself much affected with jaundice during pregnancy, produced a child with both skin and conjunctiva sensibly discolored. The child, whose right hypo-

Again, in other instances of congenital peritonitis, none of the equivocal symptoms here alluded to, have been remarked, and the cause of death has only been discovered by the post-mortem dissection (XV., XVI., XVII.)

DURATION OF FŒTAL PERITONITIS.

We have as yet but few data on which we can rely with any great degree of certainty for fixing the general duration of attacks of peritonitis in the fœtus. We have enough, however, I believe, to show that, contrary to the surmises of some pathologists, inflammatory action may occasionally proceed with nearly as great a degree of acuteness and severity in intra-uterine life as after birth. In some of the cases of peritonitis that have been related (I., II., XX.), the plump and unemaciated condition of the fœtus after death affords very strong evidence that the fatal morbid state under which it had suffered had not been long in its duration. In others, those symptoms of increased movement and restlessness in the fœtus which indicated the occurrence of acute disease in some part of its system, were only remarked for one (VII.), two (VIII.), or three (II.) days before its death; and in one of these cases (VII.), we have further corroborative evidence of the occasionally very acute character of the disease in this circumstance, that the apparent exciting cause of the peritonitis was applied only two days previous to the death of the fœtus, as indicated by a sudden and total cessation in its motions subsequently to a greatly increased degree of them. In two other cases (III., XI.) also, the fœtal movements ceased in the course of a day or two after the supposed exciting cause of the fœtal disease had operated upon the maternal system. Besides, the inspection of the dead body in this and in other instances (as in I., II., VIII., IX., X., XII.), presented such morbid appearances as corresponded only with those left by the more acute and rapidly fatal forms of peritonitis in the adult. Again, in other cases, the state of emaciation, and hence, probably, of long-continued disease (XIX.),

chondrium was very prominent and hard at birth, died under increased symptoms of icterus in four or five weeks afterwards. On opening its body, the tissues of the abdominal parietes and of all the abdominal viscera were seen to be stained yellow. The liver was very large, particularly the left lobe, which was morbidly adherent on its surface to some neighboring parts, and softened in its substance. The right lobe felt indurated, and its lower or concave surface contained a small abscess. The gall-bladder was half filled with a greenish, limpid, slightly bitter fluid. The ductus choledochus seemed obstructed by a viscid yellowish matter. The stomach was much contracted.

combined with the particular appearances found on dissection (XIX. to XXII.), show in as unequivocal a manner, that in these instances the inflammatory action must have been of a decidedly chronic character.

PERIODS OF FŒTAL LIFE AT WHICH PERITONITIS OCCURS.

Velpeau, in his elaborate treatise on Midwifery,[1] without specifying any particular diseased appearances, remarks in general terms, that he "had seen incontestable morbid alterations in the lungs, liver, *peritoneum*, and other parts of the body, at the *third* month." I have certainly, in several different specimens, observed unequivocal evidence of inflammation and morbid adhesions between different points of the cutaneous surface of the embryo, at a period as early as, or even earlier than, that mentioned by Velpeau; and although I have adduced no case of peritonitis at that age, we shall take occasion, in our next communication, to show reasons for our belief, that we may yet be able to trace many of the malformations of the abdominal and pelvic viscera, as well as those of different other parts of the body, to different diseased actions, but particularly to inflammation occurring in some of their structures during the earlier stages of their embryonic development and growth.

In some of the instances of fœtal peritonitis brought together in the present paper, the particular age of the fœtus is not noted. Of the remaining cases, the earliest are two (IX. and XXIII.), in which the fœtus was considered about four months old, or between that and the fifth. In two instances (II., VII.), the child was believed to be between the sixth and seventh month; in three (III., VI., XIV.), about the seventh month; and in others again, near to (V., XVII.), or apparently at, the full time (I., V., VIII., XI., XIII.) Most of the children who were affected, but still alive at the period of birth (as in Cases X., XI., XVI., XVII., XXIV.), may also be presumed to have nearly, if not fully, reached the complete term of utero-gestation, before the supervention of the disease.

EFFECTS OF PERITONITIS UPON THE LIFE OF THE FŒTUS.

If I might be allowed to draw any general conclusion from the comparatively limited opportunities which I have as yet enjoyed of investigating the diseases of the fœtus, I should certainly feel inclined to regard peritonitis as much more frequently fatal to the fœtus during the latter months of pregnancy than any other individual acute disease to which its economy is liable. I had occasion to see nine of the cases which I have related, in dispensary and hospital practice, within a period of twenty-three months. During the same period I have met with other inflammatory diseases in the fœtus, but certainly with no single one in so many individual instances. According to the observations also of Professor Chaussier and Madame Boivin, peritonitis would seem to have been not unfrequently met with by them in their dissections of the still-born children at the Hospice de la Maternité at Paris.[1]

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[1] Traité Complet de l'Art des Accouchemens, tom. i. p. 392.

In a considerable number of the cases which have been narrated in the preceding pages, the peritonitis seems to have been so acute and severe as to prove fatal to the fœtus before birth.

In eight of the cases (I., II., III., V., VI., VII., VIII., IX.), the children had evidently been dead for some considerable time before their expulsion from the uterus, as shown by the decomposing state of their bodies. In one (XIII.), the infant, though dead-born, was thought to be alive up to the commencement of parturition. In the three first cases of chronic peritonitis, the children were born alive, and survived to periods varying from eighteen (Case XIX.) and twenty-four hours (XX.) to two days (XXI.) after birth. In several of the acute cases also, the child was born affected with the disease, and did not die till three hours (X., XIV.), nearly a day (XII.), or even longer (XI.), after the time of birth. In one example, indeed, recorded by Professor Desormeaux, a child born with all the most marked symptoms of congenital peritonitis, rallied completely after birth, and survived. The following are the interesting details of the case as given by Desormeaux himself.

CASE XXIV.—"I had under my care," he remarks, "some years ago, an infant whose mother's health had been excellent during the whole period of pregnancy. The infant at birth was extremely emaciated; the surface of its body was of a yellowish-white color; and it had an expression of suffering, and, as it were, of old age, strongly imprinted on its countenance. Further, the little patient had the abdomen swelled, hard, and tender; the intestinal convolutions could be traced under the integuments; and all announced an intense and already chronic enteritis. The infant was intrusted to a good nurse; and, notwithstanding its feebleness, it was able first to receive some drops of milk, and after a time to suck. It has since become," M. Desormeaux adds, "a very fine child, and enjoys good health."

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[1] "Nous avons recontré, avec M. le Professeur Chaussier, que nous avons longtemps accompagné dans ses recherches sur les maladies du fœtus, un certain nombre de cas de péritonitis avec ou sans épanchemens, et toujours accompagnés d'adherences plus ou moins multipliées, des intestins."—Mad. Boivin, Recherches sur une des Causes de l'Avortement, p. 56. See also a note by Chaussier at pp. 34-35 of his Memoire sur la Viabilite de l'Enfant Naissant, Paris. 1826.

[2] Dictionnaire de Médecine, Art. Œuf. tom xv. p. 403.

From peritonitis forming so often a direct cause of death to the fœtus, its study, as that of other fatal forms of intra-uterine disease, becomes a matter of interesting inquiry to the practical accoucheur, as bearing strongly upon the important question of the various causes by which abortion and premature labor may be induced. And certainly the attention which has of late years been bestowed upon intra-uterine pathology has been useful in eliciting a higher degree of information on that subject; for the more our knowledge of the diseases of the fœtal economy has increased, the more have accoucheurs become convinced of the truth and practical importance of the fact, that the causes of abortion, and of the death of the fœtus during the different periods of pregnancy, are, in many instances, not to be sought for in any diseased condition of the general system of the mother, or in any morbid state of her reproductive organs, but in diseased actions originating in, and more or less strictly confined to, the fœtal appendages, or to the organs or tissues of the body of the fœtus itself. Besides, I feel much inclined to believe, that to these fœtal diseases, and more particularly to certain degrees of inflammation, and to the results or products of that morbid action in different parts of the body of the fœtus, when it happens to be of a local and limited, and consequently not of a fatal character, we may yet be able to trace the origin of various morbid states, the true nature of which is at present little suspected.

In my next communication I shall endeavor to show the truth of this last remark, in so far as it relates to the effects of that individual species of inflammation which we have been here considering, namely peritonitis.

ON THE INFLAMMATORY ORIGIN OF SOME VARIETIES OF HERNIA AND MALFORMATION IN THE FŒTUS.

(From Edinburgh Medical and Surgical Journal, July, 1838, p. 17.)

In following the line of inquiry pointed out in the concluding part of our last communication, we have now to direct our attention to the *indirect* morbid effects which occasionally result from peritonitis in the fœtus. In pursuing this subject, we shall consider in the following remarks—1st. The pathological relations of fœtal peritonitis in the production of certain congenital forms of umbilical, diaphragmatic, and inguinal hernia; and 2dly. We shall endeavor to trace the primary origin of some varieties of malformation among the abdominal and pelvic viscera, to the same proximate cause.

Lastly, in connection with this latter subject, we shall briefly advert to one or two circumstances explanatory of the dependence of other species of malformation upon the effects of inflammatory action in the early embryo.

We reserve the observations that we have collected on ascites and hydrocele—two other occasional indirect results of peritonitis in the fœtus—till we come to speak of the pathology of intra-uterine dropsy.

CONGENITAL HERNIA.

Umbilical Hernia.—During the progress of embryonic development, the abdominal viscera—like those of other parts of the body—pass through a number of changes, both in their own intrinsic forms, and in their relative position to one another, and to the adjoining organs. Thus, in the embryo of the tenth or twelfth week, the abdomen is still so far open in the course of its mesial line, that some folds of the intestines are generally found situated at that time in the fœtal extremity of the sheath of the umbilical cord, or, to speak more precisely, in the sheath of the vitelline duct, as it is enclosed within that of the umbilical cord. In this way we have in embryos of the above early age a kind of natural, but temporary, umbilical hernia. In some cases this hernial protrusion is liable to be rendered a permanent and morbid type of structure, in consequence of such diseased actions having been excited in these parts, as impede or prevent the usual changes of structure and of position which follow in the normal course of fœtal development. The particular diseased action which may lead to this deficiency of development is no doubt subject to considerable variations in its nature in different instances; but in some cases it would certainly seem to be intimately connected with, if not strictly and originally dependent upon, the effects of peritoneal inflammation. Nor is it difficult to understand the mechanism by which the pathological cause in question might lead to the pathological effect which we here attribute to it.

For, if the protruded abdominal viscera should once happen to become united by inflammatory adhesion to some part of the walls of the sheath which contains them, the effect of this adhesion would be such a continuous distension of that sheath by the retained and displaced bowels as might prevent its natural closure. We know from various analogous instances in the history of malformations, that the effect of the simple retention of a displaced organ in any of the temporary cavities or openings of the body, is often sufficient to interrupt the process of their obliteration, as, for example, in the congenital inguinal hernia which we have afterwards to describe;

and while we own that, in reference to umbilical hernia, our proof is deficient in so far that we are still unable to cite any direct observations to show the first commencement of the formation of this affection in peritonitic adhesions between the bowel and the sac in the early embryo, it is yet, however, rendered so far complete by our now knowing the facility and frequency with which such morbid adhesions occur on the peritoneal membrane of the fœtus. Besides, we have a kind of test of the truth of the whole opinion in tracing the history of cases of umbilical hernia—for if this diseased state occasionally originated in such inflammatory adhesions as I have here supposed, it is evident that the existence of these adhesions would necessarily render the protruded bowel irreducible at birth. Accordingly, as already stated in my former communication, we find Scarpa declaring it as the result of his experience, that the complete artificial reduction of the intestine in congenital umbilical hernia is chiefly opposed by morbid connections existing between the protruded viscera and the walls of the hernial sac. I have previously had occasion to refer to a case of umbilical hernia in Scarpa's work, in which these morbid adhesions have been represented by him; and the same circumstance seems to have been previously remarked in a similar example by Fried,[1] the neck of the sac being described by this latter author as intimately united to the intestines (*annuli intestinis arctè accreti*).

Jules Cloquet has more recently described and represented an instance in which similar adhesions from fœtal peritonitis existed under the same circumstances.

CASE XXV.—In a female child that died a few hours after birth, there existed at the right side of the umbilicus a round perforation with red ulcerated edges, and through it a great portion of the small intestines had escaped. "The protruded convolutions of bowel, about a foot and a half long, were united together into one mass by membranous adhesions, some of which appeared recent and were easily torn, whilst others were old and solid; but both were evidently the effect of albuminous effusion from the inflamed intestines. The portion of mesentery supporting the convolutions, strongly adhered to the circumference of the ulceration, and thus prevented the reduction of the bundle of intestine. The intestine itself had its coats firm and thicker than natural."[2]

After the above remarks were prepared for the press, my friend Dr. J. Reid directed my attention to the following illustrative in-

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[1] Sandifort's Thesaurus Dissertationum, tom. i. p. 314.

[2] Surgical Pathology, London, 1832, p. 162, pl. xi. fig. 4.

e of umbilical hernia in a body of a malformed fœtus, that he
eginning to submit to a minute anatomical dissection.

SE XXVI.—In a diminutive male fœtus, wanting the head and
, and born, as acephalous monsters always are, co-twin with a
d and perfect child, the situation of the umbilicus was occu-
by a hernial tumor of the size of a small walnut. The tumor
onsiderably contracted at its neck. On opening it, the caput
m and several folds of the small intestine were found in its
y. The caput cœcum and its appendix vermicularis were
, but one or two folds of the lower part of the ileum, for about
ch in extent, were firmly attached to, and incorporated with,
alls of the hernial sac. These walls consisted of an expansion
e sheath of the umbilical cord. The mesentery of the adhe-
and of the adjoining portions of intestines was much contracted
stretched in consequence of this morbid attachment of the
l. The union between the hernial sac and adherent piece of
tine was so complete, that it appeared to Dr. Reid and myself,
ssible to doubt that the inflammatory action which gave rise to
ust have occurred at a very early stage of fœtal life. Indeed,
ontiguous attached surfaces of the bowel and sac were so per-
r amalgamated together, that they looked as if they were
red by a continuity of the same serous membrane.

bringing forward these observations upon the probable inflam-
ry origin of some cases of congenital umbilical hernia, I would
e same time remark, that we by no means doubt that the adhe-
which are detected at birth between the intestine and the sac
ometimes merely secondary results, or the effects of accidental
onitis supervening among the abdominal viscera, which have
dy become protruded in consequence of other pathological
s of the embryo. Most of those cases which we alluded to in
former paper as instances of peritonitic adhesions in extensive
versions of the abdominal contents, are probably referable to
last class; and the same remark will, we believe, apply to a
of extroversion of the intestines depicted by Isidore St.
ire,[1] where the existence of films of coagulable lymph upon the
toneum is well shown in the appended plate, though the mor-
appearance itself does not seem to be adverted to in the text of
author.

iaphragmatic Hernia.—The diaphragm is not, as is now well
wn, so early formed in the course of development, as are the
cipal thoracic and abdominal viscera lying on either side of it.

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[1] Histoire des Anomalies de l'Organisation, pl. vi.

At an early period, therefore, of embryonic life, previously to this septum being completed, some of the contiguous viscera of the thorax and abdomen are necessarily in contact with one another; and the same will hold true to a more limited extent in most of those instances in which any part of the diaphragm remains open in consequence of arrested or defective development. In either of these cases, but more particularly in the former, the pleural and peritoneal surfaces of the contiguous viscera may, by taking on inflammatory action, become morbidly adherent to one another. If such adhesions are formed early in embryonic life, they will almost in all cases necessarily lead to the displacement of some of those organs that were the seat of them; for as the body elongates and enlarges in the course of fœtal growth, the more loose of the morbidly attached viscera will be gradually and mechanically dragged out of their natural situation by being obliged to follow the change of distance and position assumed by those more fixed viscera to which they are united. In this way a case of congenital diaphragmatic hernia may be formed either by the viscera of the thorax being partially drawn down into the abdomen, or what oftener happens, from some of the more loose abdominal viscera, as the stomach or intestines, being dragged into the thorax, in consequence of their having become morbidly attached to one or other of the lungs, or to some portion of the costal or mediastinal pleura, these being comparatively much more fixed parts. Probably, for example, we may refer to such an origin, a case of congenital diaphragmatic hernia recorded by Bartholin[1] and Clauder.[2] In this instance, the stomach, duodenum, pancreas, spleen, and part of the colon, were situated in the left cavity of the thorax, and the first of these organs (the stomach) adhered by strong morbid bands (*ligamentis*) to the diaphragm and pleura. In a similar case of diaphragmatic hernia published by Reisig,[3] the stomach was lying in the cavity of the thorax, and was intimately united to the left lung by a false membrane or *ligament* (as this author also terms the band of morbid adhesion). This false membrane was evidently of very old formation, inasmuch as it had so completely assumed the structure of serous tissue, that Reisig has been led to describe it as apparently formed by a duplication of the pleura itself (*videbatur duplicatio membranæ externæ quæ materiam pulmonum circumdat*).

Inguinal Hernia.—The production of other forms of congenital hernia, besides the umbilical and diaphragmatic, may, with even more justice and certainty, be traced to the previous existence and effects of peritonitis in the fœtus. Thus the morbid adhesions resulting from inflammation of the peritoneum during intra-uterine life may, we believe, sometimes act as the pathological cause of that form of congenital hernia, first accurately described by Haller and the two Hunters, and which consists in the descent of a portion of the intestines or omentum into the tunica vaginalis of the testicle,[1] through the unobliterated canal originally existing between that cavity and the general cavity of the peritoneum.

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[1] Histor. Anat. Rar. Cent. VI. tom. iii. p. 287.

[2] Ephemerid. Nat. Curiosor. Dec. ii. An. v. p. 193.

[3] De Ventriculo in Cavo Thoracis situ Congenito, Berlin, 1823, p. 8. It should have been stated that Reisig's case occurred in the body of a ferret (Viverra Zibetha).

Mr. Pott long ago remarked, that, in this variety of congenital hernia, morbid connections of the protruded bowels to the sac containing them, and particularly to the testicle, were more frequent than in any other form of rupture. "In this kind of hernia," he observes, "I have more frequently found connections and adhesions of the parts to each other, than in the common one; but there is one kind of connection sometimes met with, which can never be found in that which is a common hernial sac, which may require the dexterity of an operator to set free—I mean that of the intestines with the testicle, from which I have more than once experienced a great deal of trouble."

Surgical pathologists have very generally subscribed to the accuracy of the preceding observations of Mr. Pott, and the circumstance mentioned by him is itself, we believe, explicable upon the two collateral facts—1st. That the displaced viscera in this form of hernia are, on the whole, for a longer period protruded, and in contact of the walls of the containing sac, than in any other species of hernial disease, and hence more liable to the chances of adhesive inflammation; and, 2d. Adhesions in this species of hernia are frequent, because in some cases the hernial displacement strictly originates in, or is produced by, the previous existence of such adhesions, as an effect of peritoneal inflammation in the fœtus. We shall quote a case from Wrisberg, illustrative of this last point.[3]

CASE XXVII.—In a child that was born about the eighth month of utero-gestation, the author alluded to, found the right testicle descended into the scrotum, but the left was still situated at the inguinal ring. This latter (the left) was very mobile, and could be pressed slightly downwards, and still more easily upwards into the cavity of the abdomen. The child died a few days after birth. On opening the abdomen, the omentum was seen drawn into the left iliac region, and was united to the tunica albuginea of the testicle by three threads (*tribus filis albugineæ testiculi accretum*). The testicle descended through the abdominal ring on pressure, but it was drawn back when the omentum was stretched by having its cavity inflated through the foramen of Winslow. The process of peritoneum forming the tunica vaginalis stretched downwards nearly to the middle of the scrotum.

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[1] A hernial protrusion of the same kind occasionally occurs into the corresponding process of Nuck in the female.—See Wrisberg, Comm. Medici, Obstet., &c., p. 234.

[2] Chirurgical Works, 1779, vol. ii. p. 162. [3] Commentari Medici, Obstetrici, &c., p. 229.

Wrisberg does not mention any opinion as to the nature of the morbid threads which existed in this case; but with the more advanced knowledge that we possess at the present day with regard to the origin of pathological products, it is impossible to doubt that such adhesions and threads as he describes could only be the effect of previous adhesive inflammation in the morbidly accreted surfaces.

Cases analogous to that given by Wrisberg have been recorded by other authors. Thus Jobert[1] states that in one instance, he found in the fœtus the cœcum adhering to the testicle, and on the point of passing the ring; and Soemmering[2] and Schwenke[3] have each met with the appendix vermiformis adhering to the testicle. If, in any of these instances, the testicle had made its way down into the scrotum with the abdominal viscus, to which it was morbidly attached, still adhering to its surface, it is evident that it would have produced a case of that form of congenital inguinal hernia which has been referred to, namely, with the protruded abdominal contents placed in the serous cavity of the tunica vaginalis. Sandifort[4] has given the following history of a case of congenital hernia, in which the child was born with the hernia in this way already completely formed.

CASE XXVIII.—A hernia was present in the right side of the scrotum at birth. It was not, however, till the child was about three months old, that any remedial measure was resorted to. The hernia was then attempted to be completely reduced, and a truss was applied for the retention of the protruded viscera. This proceeding was immediately followed by violent symptoms of strangulation and death. On the post-mortem inspection of the body, it was discovered that the hernia was formed by the cœcum and the extremity of the ileum. The appendix vermiformis of the cœcum adhered in part to the testicle, and in part to the bottom of the hernial sac and tunica vaginalis; and at the place of its adherence to the testicle, this appendix was become harder, or somewhat more compact than natural (*duriusculus*)—a circumstance which renders it probable that the inflammatory action, and the effusion of coagulable lymph resulting from it, had been of so chronic a nature, as to have existed before the descent of the testicle, and consequently before the formation of the hernia.

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[1] Traité des Maladies Chirurgicales du Canal Intestinal, tom. ii. p. 332.

[2] See his German translation of Baillie's Anatomy, pp. 97 and 123, or Danz. Zergliederungskunde des Ungebohrnen Kindes, Bd. ii. s. 164.

[3] Ueber die Bruche, &c., or Meckel's Pathologischen Anatomie, Bd. iii. s. 419.

[4] Icones Herniæ Inguinalis Congenitæ, p. 12, tab. i., ii., and iii.

It is unnecessary here to multiply individual cases from Wrisberg, Meckel, Pelletan, Langenbeck, and others, for the purpose of proving that morbid adhesions between the intestinal viscera and testicle, similar to those that we have shown to be occasionally formed during the descent of the latter organ, still continue to exist, in congenital hernia, even (as in the case of Sandifort and others) after the testicle is completely descended, and the hernia fully formed. "The experience of most individuals," as Mr. Lawrence has remarked in his learned Treatise on Hernia, "must have furnished opportunities of observing how frequently the viscera are connected to the testes in congenital ruptures."[1]

In closing these remarks upon congenital herniæ, we would beg to add, that in pointing out the mode in which they are produced as a result of inflammation of the fœtus, we by no means wish to maintain the constancy of their origin in this manner. We are, on the other hand, perfectly convinced, that perhaps in the majority of cases they are formed independently of any such preceding morbid action. All that we here advocate is their occasional origin from inflammatory adhesions; and even those pathologists who may not entirely coincide with me in opinion upon this point will still find in the cases that I have adduced, a number of proofs of another fact in intra-uterine pathology that I would wish to enforce, namely, the great frequency of inflammatory action in the structures of the fœtus.

MALFORMATIONS OF THE ABDOMINAL AND PELVIC VISCERA.

Various malformations, both in the way of displacement, and of arrested development, in the digestive, urinary, and genital organs, may, I believe, be justly considered as indirectly produced by peritonitis in the fœtus.

The occurrence of abdominal inflammation during the earlier periods of intra-uterine life may lead to the effects which I here attribute to it, in various modes. We have already had occasion to remark, that the different chylopoietic viscera undergo numerous changes, both in their physical forms, and in their absolute and relative situations, from the time that the intestinal canal first appears in the embryo as a mere pouch or bag, formed by a segment of the yolk or umbilical vesicle, till the period of the full development of these organs in the more advanced fœtus. The ovaries and testes also present very considerable changes in their locality during the progress of embryonic evolution, and some of the pelvic organs of the adult, as the uterus and urinary bladder, are, in respect of position, abdominal organs in the fœtus.

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[1] Treatise on Hernia, 1838, p. 568.

Now, if peritonitic inflammation should happen to take place on the surface of any one or more of these viscera, at the time that they still occupy a position different from that which naturally pertains to them in extra-uterine life, and if this inflammation gives rise to an effusion of coagulable lymph binding them (as we know it is very liable to do) to any contiguous surface, it is evident that the inflamed organ or organs might in this way be prevented from undergoing any further change of situation, and subsequently come to present to us an example of malformation by displacement. In such a case we would find, in general, the displaced organ morbidly fixed in one or other of those situations which it normally but temporarily assumes during the course of development—unless indeed, where it happened to be dragged out of this position by the ulterior changes and movements in the part or viscus to which it was adherent. And, again, it seems probable, that, in other instances of displacement of the abdominal organs connected with peritonitis, one or more of them may be found drawn away from those positions which they occupy during intra-uterine life, in consequence of the morbid contractile power sometimes exerted by the false membrane formed upon their surface—all false membranes when they degenerate into fibrous structure having like the tissue of external cicatrices and other morbid fibrous tissues, a greater or less tendency to such contractions. In my former communication, I have already quoted a case from Morgagni showing the fact, that a peritonitic pseudomembrane in the fœtus sometimes exerts by its contractile influence such a power of displacement as we here allude to.

Altogether, then, in malformations by displacement among the abdominal viscera, resulting from fœtal peritonitis, the displaced organs may, as we believe, either be seen, *first*, in some of the temporary positions assumed by them, during their evolution, and which they occasionally continue to occupy in cases simply of arrested development, or, *secondly*, they may be found drawn to a greater or less extent out of these positions, in consequence of the operation of those agencies to which we have adverted.

When we detect in the adult an instance of partial displacement of some of the abdominal viscera, connected with the existence of an old pseudo-membrane upon their peritoneal surface, it may be difficult, or altogether impossible, to trace whether the formation of this membrane may have taken place during early fœtal life, and acted as the immediate cause of the displacement, or has only been the result of peritonitic inflammation, which has supervened at some period subsequent to the occurrence of the displacement.

In most of the cases of partial transposition among the intestinal viscera themselves that have been hitherto observed, the *origo mali* seems to have consisted in a displacement upwards, and towards the left side of the caput cœcum and ascending colon; and, in consequence of the right iliac and lumbar regions not being filled up as usual with these bowels, the looser folds of the small intestines come to occupy that portion of the abdominal cavity. Without entering into the details of the individual cases of this kind that have been put upon record by Salzmann,[1] Schacher,[2] Mery,[3] Sandifort,[4] and others,[5] I shall content myself with quoting, as a sufficient illustration of the doctrine which we propose, the following remarkable case, for the notes of which I am indebted to my friend Mr. Forbes Angus:

CASE XXIX.—In a strong middle-aged man, who died of continued fever, the caput cœcum was placed not in the right iliac, but in the lower part of the left iliac and lumbar region. From it the colon ascended nearly in the mesial line, and then, after making an acute turn, it occupied during the remainder of its course its natural position. The displaced caput cœcum thus lay contiguous to the sigmoid flexure, and was united to it by an old false membrane. The caput cœcum was further retained by other strong morbid adhesions, which stretched between it and the lining membranes of the pelvis. The small intestines occupied the position of the displaced cœcum and ascending colon, and were all situated towards the right side of the abdomen. Consequently the lower extremity of the ileum, instead of following its usual direction, had to run from right to left, in order to join the large intestine.

In bringing forward the above case, we freely admit, that we have no positive proof to offer, that the existing old morbid adhesions were formed at such an age in fœtal life as would have enabled them mechanically to effect the displacement of the caput cœcum. At the same time, however, we derive some probability of their formation at that early period, not only from considering that their necessary agency would have been such as to have produced the result in question, but from our having in other analogous cases decisive evidence that morbid peritonitic adhesions formed during intra-uterine life have, in the indirect mode that we have indicated, occasionally given rise to marked displacement of portions of the intestinal canal. In fact, the particular instances of congenital and fœtal hernia which we have considered under the previous section, may in reality be looked upon as so many examples of true malformation by displacement, resulting from such a cause. We make this remark in reference to the various examples of displacement of the intestines that we have cited from Bartholin, Reisig, Cloquet, &c., under the heads of umbilical and diaphragmatic hernia; and it perhaps applies in a still more direct manner to the cases that we have quoted from Sandifort, Jobert, and others, in which the inflammatory adhesions resulting from previous fœtal peritonitis produced various displacements of the cœcum, omentum, &c., by the testicle to which they were morbidly united, dragging them more or less along with it during its descent from the abdominal cavity. But, in addition to these, we may here also recall to our recollection that other analogous facts seem to show, that the occasional retention of the testicle within the abdomen for some years after birth, or, as sometimes happens, during the whole of life, may depend upon a similar cause, viz., on inflammatory adhesions formed between the testicle with the more fixed abdominal organs which it meets with during its descent. Thus Cloquet[1] mentions an instance in which he found, in an aged adult, the left testis natural in size, but retained within the abdomen, one inch above the upper opening of the inguinal canal, by a strong, short, rounded, *fibrous cord or false membrane*, which bound the epididymis to the sigmoid flexure of the colon. And we know further, from the researches of M. Serres,[2] that in cases, such as this, the cœcum will not in general occupy its own natural position within the abdominal cavity, since it has been shown that there exists a fixed relation between the position of that bowel and the position of the testicle in the male, and the ovary in the female.

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[1] Decas Observationum illust. Anatomicarum, p. 53.

[2] De Morbis a situ Intestinorum preternaturali.

[3] Memoires de l'Academie des Sciences, 1716, p. 179.

[4] Observationes Anatomico-Pathologicæ, lib. iii. p. 11.

[5] See some interesting original cases, together with references to others, by Dr. J. Reid, in the Edinburgh Medical and Surgical Journal, vol. xlvi. Dr. Reid informs me, that he very lately met with a case in which the caput cœcum was placed in the right lumbar region. It had apparently never been allowed to descend into its natural position in the right iliac fossa, in consequence of the vermiform process, which was completely on the stretch, having contracted very strong adhesions to the inferior surface of the liver. The case is another illustration of the remarks I have made in the text.

The following instance of morbid adhesion of one of the testicles within the abdominal cavity, and of partial displacement of the intestines in connection with the same cause, furnishes an apposite illustration of some of the preceding remarks.

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[1] Recherches sur les Causes et Anat. des Hernies Abdominales, p. 24, pl. v. fig. 2.

[2] Isid. St. Hilaire, Histoire des Anomalies de l'Organisation, tom. i. p. 378.

Case XXX.—On dissecting, with Mr. F. Angus, 1st November, 1838, the body of an anencephalic fœtus, I found, on opening the abdomen, particles of coagulable lymph on several points of the abdominal peritoneum, and upon the upper surface of the right lobe of the liver. The contiguous folds of the small intestine were in various places agglutinated by coagulable lymph. The small intestine was in several points, also, adherent to the large, as, for instance, very extensively along the transverse and descending colon, by the medium of a strong web of coagulable lymph. The caput cœcum was displaced. It lay fully an inch above its natural situation, and was bound down to the peritoneum covering the right kidney, by a quantity of false membrane. The surface of the large intestine was very adherent to the abdominal peritoneum at the top of the descending colon. At its sigmoid flexure this bowel was twisted forward; it formed one fold in the left iliac region, and then stretched upwards and across for more than an inch, till it reached the right lumbar region and touched the appendix vermiformis, when it again turned round at a very acute angle, and ran downwards behind the urinary bladder. The pelvic viscera were matted together by coagulable lymph; and the posterior surface of the bladder was intimately united by false membrane to the first mentioned fold of the sigmoid flexure, and to that part of it situated immediately above the rectum. A fold of small intestine lay in the angle of the second fold of the sigmoid flexure, and was morbidly adherent to the mesocolon. Two other folds of the small intestine were firmly bound down by morbid adhesions into the right iliac fossa, and thus occupied the natural situation of the caput cœcum. The right testicle lay nearly contiguous to them, and was there imbedded in a quantity of coagulable lymph, which in a similar manner strongly attached it to the peritoneal surface of the iliac fossa. The left testicle was not morbidly adherent. The lower surface of the right lobe of the liver was studded with numerous small masses, which, on section, had all the usual physical appearances of semicretaceous tubercle. A few such bodies were also seen on the lower surface of the left lobe, and some of them were imbedded two or three lines deep in the substance of the organ.[1] The spleen was small and healthy. The thoracic organs were sound. The lungs were solid, the child never having breathed.

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[1] Bonetus, in his Sepulchretum Anatomicum (tom. iii. p. 104), mentions a case exhibiting an analogous morbid appearance in the fœtal liver. "In jecore fœtûs abortivi sex mensium, ad lobum infimum detectus est tumor inequalis, asper tophorum particulis tanquam confractis cerasorum nucleis, absque pure, tamen plenus visu tactuque durus." Wrisberg (Sandifort's Thesaurus Dissertationum, tom. iii. p. 214), gives a case in which several biliary calculi were detected in the gall-bladder of a woman who died during parturition. The child died seven weeks after birth, and from the dissection it appeared that a tendency to the formation of similar morbid concretions in the same organ had been transmitted from the mother to her offspring, for, on the examination of its body, "vera concrementa calculosa,

CASE XXXI.—On the body of a dead male fœtus, sent to me by Mr. David Angus of Glasgow, I found numerous adhesions from coagulable lymph in different parts of the abdominal cavity. The spleen was covered on its convex surface with a web of false membrane. A fold of the sigmoid flexure of the colon crossed completely over to the right side of the vertebral column, and was firmly retained by the morbid adhesions to the peritoneum in that anormal situation. The caput cœcum and its vermiform process were placed in the right lumbar region.[1]

As one additional illustration, from among many others that might be quoted, of the strong effects of morbid contractions and adhesions in producing malformations by displacement in the manner alluded to, I may here refer to a remarkable case of congenital hernia of the ovary, represented by Billard[2] in the atlas attached to his work on the diseases of infants. The sketch which is given of the case shows that, in consequence of the great and apparently morbid shortening of the left broad ligament of the uterus, the ovary, which is naturally very intimately attached to this ligament, has been dragged during intra-uterine life through the inguinal canal, and the uterus itself, by a similar mechanism, has been partially displaced towards the left side.[3] The kidney also of the same side has, as is seen in his plate, been dragged downwards below the level of that of the opposite side by the contraction of the cellular tissue around it, and by a fold of membrane connecting it intimately with the abdominal orifice of the hernial sac. The renal artery and vein have yielded to the same cause of displacement, and are seen to have become elongated in consequence of it. In fact, after the ovary had once become displaced and fixed, it would seem that all those other more loose and movable viscera of the left side of the abdominal cavity, which were in any way intimately and organically connected with it, and the uterine ligaments, came themselves to be displaced in turn by being held in morbid approximation to the site of the protruded organ, and thus not allowed to follow those

salbuli instar," were found in the gall-bladder. The liver of this infant was at the same time large and indurated, a condition, which we may look upon as indicating a chronic, and hence probably an intra-uterine, diseased state.

[1] This and the preceding case were shown to the Anatomical Society; and I have preparations of them in my Museum.

[2] Atlas, Tab. x. Traité des Maladies des Enfans, p. 492.

[3] I have in my possession a preparation showing a well-marked instance of anteversion of the uterus in the fœtus, in consequence of either an original or a morbid shortening of both the round ligaments.

gradual changes of relative distance and position that naturally occur under the general enlargement of the abdomen in the course of the development.

In an essay on abortion as dependent on morbid organic conditions of the uterus, Madame Boivin has shown that the existence of old inflammatory adhesions between the uterus or uterine appendages, and any of the more fixed organs in their neighborhood, may lead to an early expulsion of the embryo, by preventing, during pregnancy, the uterine tumor becoming developed beyond a fixed and limited point. The same author has further suggested that the peritoneal inflammation producing such adhesions may occasionally occur during intra-uterine life, when the uterus is strictly an abdominal and not a pelvic organ.[1] In my former communication, I have detailed one instance (Case VII.), in which I found some patches of coagulable lymph adhering to the right Fallopian tube of a fœtus that had died in utero about the seventh month of pregnancy. Unless, however, this lymph was thrown out in large quantities, and formed very strong false membranes, it is doubtful whether it could have persisted for such a long period after birth as Madame Boivin supposes. At least we know, that, after the lapse of a considerable time, old suffused lymph or false membrane is often more or less entirely absorbed, as attested by numerous experiments and observations on adhesive inflammation of the peritoneum in the adult by Ribes, Beclard, Dupuytren, and others—by the ulterior stethoscopic history of some cases of pneumonia that had reached the stage of hepatization—and by what we have constant opportunities of seeing in cases of iritis, in the transparent portion of the eye—an organ which affords us a beautiful microcosm for the accurate and direct observation of this and other similar pathological phenomena.

The consideration of the above pathological point suggests it as an interesting question, whether in some of those cases of partial transposition of the viscera that we have described, the absence of all appearance of peritonitic adhesions or false membranes in the adult may not depend upon the absorption, during the intervening period, of those adhesive bands that led to this visceral displacement in the early fœtus? The facts adduced by the pathologists I have just named, give to such a supposition a degree at least of great probability, if not of inductive certainty; and hence a much greater number of visceral displacements may possibly have such an inflammatory origin as we have described, than can be proved by any anatomical examination of them in the adult subject.

We have already stated, that, in instances of malformation by displacement among the abdominal viscera, arising from peritoneal inflammation in the early fœtus, the intestines were, in some cases, retained by their morbid adhesions in those positions which they were intended to occupy for a transitory period only during intra-uterine life, and so far we may look upon these adhesions as liable also at the same time to produce malformation by arrestment of development. This last effect, however, of abdominal inflammation in the embryo may be even more marked when the inflammatory action extends from the peritoneal surface to the structure of some of the more solid organs which the membrane envelopes. Thus, for an example of this inflammatory origin of some cases of malformations by *deficient development*, let us return for an instant to the testicles. I have already alluded to the effect produced by fœtal peritonitis in causing malformation by displacement of the organs. If the same organs, from any general abdominal inflammation, had, on both sides, their substance as well as their surface attacked by inflammatory or other morbid action, so as to produce such lesions of them as would impede or destroy their full development, their own organization would not only be prevented from arriving at perfection, but the power which their development appears to exercise upon the development of the other sexual organs and of the system in general, would be more or less completely suspended. In old age, the natural atrophy, and during adult age, the destruction of these organs by disease or by operation, produces a very marked effect upon the stability of those anatomical and physiological changes which constitute the secondary sexual characters; —if they are disorganized or removed at or before puberty, then these secondary characters are only very imperfectly evolved, or remain altogether undeveloped; and if their disorganization were effected in an early stage of embryonic life, there are various facts, we conceive, which go far to show[1] that the different subordinate sexual organs themselves would be arrested in their further development, and present to us at birth in the reproductive apparatus, under a permanent form, one or other of those various but naturally temporary types of fœtal sexual structure, the persistence of which constitutes a large class of hermaphroditic malformations by deficiency which we occasionally meet with in the higher animals.

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[1] Recherches sur l'une des Causes de l'Avortement, p. 57.

INFLAMMATORY ORIGIN OF SOME MALFORMATIONS IN OTHER PARTS OF THE BODY.

There appears to us to be every reason to believe that inflammatory action in the other serous cavities of the embryo may lead to occasional malformations among the visceral contents of these cavities by a mechanism similar to that which I have endeavored to trace in the results of fœtal peritonitis. If we confine ourselves to the selection of one or two examples only in illustration of the connection of pleuritis and pericarditis with thoracic malformations, we may refer to the case of partial extrophy of the heart, lately published by Dr. O'Brien.[1] Between the umbilicus and lower end of the sternum, in this instance, there existed at birth a hernial sac containing some of the abdominal viscera in its inferior part, and a process of the sac of the pericardium in its upper. The apex of the left ventricle was drawn out in an elongated form into this process of pericardium, and attached to it "by old adhesions"—the result, no doubt, of pericarditic inflammation in the fœtus.

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[1] See my article on Hermaphroditism, in Todd's Cyclopædia of Anatomy, vol. ii. pp. 733–34, &c., given in a future page.

Other varieties of displacement and extrophy of the thoracic organs might be shown to be not unfrequently connected, in a similar and even more marked manner, with intra-uterine inflammation and adhesion of the thoracic viscera to one another, and to those surfaces with which they happened, either naturally or accidentally, to be placed in contact. Breschet[2] has described one case of extrophy of the heart, in which the apex of this organ was retained in its abnormal situation by an adhesion to the tongue; he mentions a second similar case, in which the serous surface of the same organ was morbidly adherent to the mucous surface of the palate; and in a third instance, reported by M. Bonfils,[3] in which the head and chest of the child were intimately united to the fœtal aspect of the placenta, the "heart had contracted adhesions with the connecting placentary membrane, and been dragged by them out of the thorax to the superior and anterior part of that cavity, where, by the medium of similar morbid bands, it was attached also to the anterior part of the head. It was connected likewise to the liver by a band stretched between these two organs."[4] In an analogous instance of visceral extroversion, Haan[5] found the surface of the heart connected to the umbilical cord by a thick thread of plastic or fibrinous pseudo-membrane.

Various pathologists, as Morgagni,[6] Penada,[7] Tiedemann,[8] Beclard,[1] and Duges,[2] have attributed a number of the most marked malformations of the head and upper part of the body to the destructive effects of hydrocephalus in the embryo. Though assuredly this doctine has been carried far beyond its just limits by some of the authors here alluded to, there is still, however, as we believe, much reason to suppose that one or two varieties of anencephalous malformations are truly referable to the purely physical effects of hydrocephalic inflammation and effusion.[3]

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[1] Transactions of the Provincial Medical Association, vol. vi. with plates.

[2] Repertoire Gen. d'Anatomie, tom. ii. p. 24.

[3] Ibid. p. 25, pl. ii. fig. A. B.

[4] Breschet, loc. cit. p. 26, pl. iii. figs. 1, 2, and 3. Possibly the strong adhesions of the heart to the pleural surface of the diaphragm and of the left lung, found by the same author in a case of congenital absence of the pericardium, may have been the result of intra-uterine inflammation. See Repertoire, tom. i. p. 67, pl. v.

[5] Medic. Jahrbucher des Ost. Staates, Bd. v. s. 56.

[6] De Sedibus et causis Morborum, cap. xii. 5–8, and xlviii. 50.

[7] Saggio d'Osservazioni e'Memoria, Padua, 1793, tom. ii. p. 55.

[8] Zeitschrift für Physiologie, Bd. iii. s. 35.

On other occasions I have endeavored to trace the production of some additional varieties of malformation to an inflammatory origin. Thus, in consonance with the doctrines of Geoffroy St. Hilaire, I have endeavored to show that morbid inflammatory adhesions sometimes occur between the internal surface of the amnion and the head and various parts of the body of the embryo;[4] and I have at the same time referred to numerous cases in which these adhesions seemed to me to have proved the proximate cause of different varieties of malformation by displacement and arrestment of development in the fœtus[5]—such as certain forms of extroversion of the encephalic, thoracic, and abdominal viscera, some defective conjunctions in the median line of the face, trunk, and spine, atrophy or defective development of one half of the body, or of particular limbs, disfigurements of the face and surface of the body, &c. I have elsewhere,[6] also, endeavored to trace to the inflammatory effusion of coagulable lymph upon the cutaneous surface of the fœtus, the occurrence of certain mechanical mutilations of its limbs, produced by the constriction and contraction around them of bands or cords of false membrane, formed by the effused lymph. I have in my possession the preparation of a malformed fœtus of about the fourteenth week, in which a similar inflammatory effusion exists on different parts of the surface of the body, and has firmly united one of the hands to the corresponding cheek of the embryo.

The happy idea that was first suggested by the master mind of

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[1] Bulletin de la Faculté de Med., 1817, p. 499.

[2] Revue Médicale, 1827, tom. iv. p. 428.

[3] At a meeting of the Obstetric Society of Edinburgh, 15th June, 1847, Dr. Simpson stated that, in his opinion, in anencephalous monsters, the malformation arises from intra-uterine disease, viz., from the bursting of the head when hydrocephalic. The brain is opened up and distended by fluid, so that it becomes gradually absorbed; and at length the enclosing membranes give way. The two small tubercles always seen in anencephalous cases, lying on the base of the cranium, seem to be nothing else than the remains of the membranes, shrunken up, and almost obliterated.—See Edinburgh Monthly Journal of Medical Science, September, 1847, p. 211.—Ed.

[4] Edinburgh Medical and Surgical Journal. vol. xlv. p. 307.

[5] For some additional cases see Gooch's Midwifery by Skinner, p. 91; Medico-Chirurgical Transactions of London, vol. ix. p. 433, and St. Hilaire's Histoires des Anomalies, tom. ii. p. 266–289.

[6] Dublin Medical Journal, Nov. 1836, p. 220.

Harvey, relative to certain malformations consisting, *not* in the *substitution* of an entirely new and anomalous type of structure in the malformed part, but only in the simple *permanence* of some of its transitory fœtal types, has been reduced, within the last thirty years, by the able investigations and labors of Wolff, Autenreith, Meckel, St. Hilaire, and others, into one of the most certain and comprehensive and at the same time one of the most beautiful laws in teratological anatomy. For our own part we entertain little doubt that physiologists will ere long be enabled to proceed with confidence one step further in investigating and generalizing the causes of the production of some of those malformations that are at present attributed, in accordance with the above principle, to deficient or arrested development merely, inasmuch as they will feel themselves entitled to trace, in a certain number of cases at least, the state itself of impeded development to the anterior influence of different inflammatory and other casual pathological conditions of the early embryo. It likewise appears to us probable that a limited series of those malformations that are now generally looked upon as the decided results of arrested development may come again to be regarded, as they were formerly, not as instances of absolute want of development of the defective part, but as cases in which this part had been, in the first instance, more or less fully evolved, and then subsequently destroyed by morbid action.

In prosecuting the study of malformations in reference to the above questions relative to the mode of their production, there appear to us to be several points highly worthy of consideration, which have not, we think, been hitherto sufficiently attended to in the inquiry; and if some of these points were once fully and satisfactorily established, they would certainly go far to remove several of the more prominent difficulties connected with this investigation. Thus, in endeavoring to trace the origin of different malformations to pathological conditions of the fœtus, we ought, as it appears to us, always to hold prominently in view the fact, that large divisions of the body and complete systems of the organs of the future individual, are represented by limited segments and small points in the organization of the early embryo. Hence, to speak rather paradoxically, inflammatory and other diseased actions in early embryonic life may be very *limited* in their actual extent, and yet be very *extensive* in their actual results. If, for example, the minute anterior fold of the germinal membrane happened to become, during the first stages of development, the seat of any destructive morbid action, this action might in reality be confined to a very small extent of structure, and yet lead to what would afterwards appear as great effects when the other parts of the body came ultimately to be fully evolved. In this way some even of the most marked cases of acephalous monstrosity may, we believe, be produced by a morbid action that is originally strictly confined to organic structures of only one or two lines in absolute extent. The anatomical structures also of the early embryo are all so simple and homogeneous in their nature, that any morbid actions that may arise in them will not probably be limited in their extension and effects, as they are in the adult, by existing differences in the pathological properties of the component tissues of the affected part; while again, in addition to their limited scale and homogeneous character, the softness and fragility of these same embryonic structures, and the frequent dependence of a considerable segment of them upon the integrity of a single vessel, will all contribute to render a small amount of morbid action or mechanical injury capable of producing a great and destructive local effect. At the same time, however, that such *local* destruction of parts, from the same amount of diseased action, may be very much greater in the embryo than in the adult, it seems, on the other hand, equally certain that the *general* effect of this diseased action and destruction of parts is always infinitely less upon the former than upon the latter—as respects either the life of the individual or the integrity of the other component parts and organs of the body. This last point appears to us to be one of much moment in the present investigation, and, in conclusion, we shall adduce one or two remarks as comments upon it.

The labors during the present century, of Meckel, Serres, and various other physiologists have, in accordance with the doctrine of unity of organization, shown, that at different stages in the course of development, in the embryos of man and of the higher animals, their individual organs pass through a series of transitory changes or types of structure, in each of which they bear a greater or less resemblance to some of the permanent types of structure presented by the same organs in some of the inferior classes of animals. In this manner the complete history of the embryonic development of any organ in the higher animals, comes to correspond more or less exactly with the history of the development of the same organ in the animal kingdom at large—or, in other words, its embryonic and comparative anatomy are, in most of their great outlines, repetitions of one another. But, besides this resemblance in mere anatomical structure between the early embryos of the higher, and the permanent normal states of the lower animals, we are further inclined to maintain that there exists between them an equally strong analogy with respect to some peculiarities in the *pathological* laws that preside over their organization.

We know it to be a principle generally admitted in comparative pathology,[1] that, in proportion as an animal is more simple in its structure, its power of repairing and restoring lost parts is the greater, and, on the contrary, that this power diminishes nearly in an inverse ratio with the original or acquired complexity of its organization. Thus, while a mere segment of a Polyp or Planaria will reproduce a perfect animal, other animals higher in the scale of life, as the Mollusca, Crustacea, and Batrachian reptiles, have only the power of reproducing individual, though still complex, parts, as an eye, a lower jaw, or an extremity; whilst in man, again, in his adult state, this property of reproduction of lost parts extends simply to the generation of single tissues, and, indeed, only to that of the more simple among these tissues. The larvæ of Insects, and of the Arachnida, have the power of regenerating a lost antenna or limb; but the very same animals, after they become more perfectly developed, and consequently have acquired a more complex organization, exhibit no such property of reproduction. Now, during the first stages of development, the human embryo, and that of the higher animals, approaches in simplicity and homogeneity of anatomical structure to the normal condition of the lower tribes of animal beings, and consequently, also, it will be found, we believe, to present at the same period a degree of analogy in its power of suffering and repairing injuries; and as this power, as we have already seen, diminishes with the complexity, and increases with the simplicity of animal organizations, so in the human and other embryos, the *earlier* that any morbid change, mutilation, or destruction of parts takes place, the more probable will it be, *first*, that it will not prove subversive of life, and, *secondly*, that nature will attempt some process of reparation.

The analogy to which we here advert may, in some respect, we think, explain to us the possibility of the fact already alluded to, and which we have so often occasion to remark in the study of monstrosities, namely, that when one organ or set of organs in the embryo, or even a large and entire segment of its body, is arrested in its development, or destroyed by disease, it frequently exerts no deleterious influence upon the general life of the embryo, or upon the development of the other remaining parts and organs of its body. The same analogy also may enable us to admit what we believe to take place in some cases in the human embryo—a certain, though comparatively very imperfect, attempt at the regeneration of parts that have been destroyed. I have been able, as I conceive, to trace marks of regeneration in several cases of the so-called spontaneous amputation of the limbs of the fœtus; and there is reason to believe that it may take place in other parts and under other circumstances; but it probably occurs, in the human embryo, only when the original mutilation is effected at a very early period of embryonic life.

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[1] We need only refer to the well-known experiments and papers of Spallanzani, Bonnet, Trembley, Reaumur, Baumgaertner, Heineken, Müller, Dugés, Johnson, Allen Thomson, &c.

It is almost unnecessary to add, that if, in the study of monstrosities by deficiency, we admit in this way of the possibility of a partial regeneration of lost parts, we shall necessarily, from the presence of such reproductions, have an additional difficulty in distinguishing between those malformations that consist of an originally imperfect or impeded development, and those in which the defect was the result of a secondary destruction of the anormal structures in consequence of disease, or of injury.[1]

At the Obstetric Society on the 9th of February, 1848,[2] Dr. Simpson showed the body of a new-born infant which had died a few days before birth, of acute peritonitis, as shown by quantities of coagulable lymph effused upon various parts of the surface of the peritoneum, and more particularly on the surfaces of the spleen and liver.

He stated also as the result of his observation and researches, the following

General Deductions regarding Peritonitis and Fœtus:

1st. Acute and fatal peritonitis appears to be a very common inflammatory disease in the fœtus in the latter months of utero-gestation.

2d. A large number of fœtuses dying in the seventh and eighth month of utero-gestation, present, as he had found on dissection, well-marked anatomical evidence of it, in the presence of effusions of coagulable lymph, adhesions between the folds of intestines, pus, &c.

3d. The child is sometimes, though rarely, born alive, and affected with it.

4th. Far more commonly the child is born dead, and the previous history of the mother shows that it had perished from one to three weeks before its expulsion, its movements having ceased about that time.

5th. Before the child's movements entirely cease, the mother very generally remarks that its movements are morbid and excessive for fifty or sixty hours previously—probably during the currency of the fatal disease.

6th. Peritonitis is occasionally apt to recur in successive children in the same mother, and seems in some, a result and remnant of syphilitic poison in the parents.

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[1] For further remarks upon the regeneration of lost parts, see a future page.—(ED.)

[2] See Edinburgh Monthly Journal of Medical Science for May, 1848, p. 837.

7th. But in most cases its occurrence is independent of syphilis, and occasionally it will not attack successive children in the same mother, or even both children in cases of twins. In an essay on the disease, published some years ago,[1] Dr. Simpson had described a case of twins, in which one was born living and healthy; the other was dead, and within the abdomen were found all the usual appearances following intra-uterine peritonitis.

8th. Whilst intra-uterine peritonitis is very common, intra-uterine pleuritis is very rare; Dr. Simpson had only seen two well-marked cases of it in the fœtus.

ON A CASE OF PERITONITIS, WITH PERMANENCE OF THE OMPHALO-MESENTERIC VESSELS.[2]

(London and Edinburgh Monthly Journal of Medical Science, Feb. 1845, p. 117.)

1. The state of the cuticle in the child, was such as is generally seen when the infant has been dead for several days before its expulsion—that is to say, it was peeling off extensively, though not anywhere thrown into the form of bullæ, as we sometimes find it. There was another evidence of putrefaction, namely a great swelling of the scalp, from, as we saw on making a section of it, a thick gelatiniform effusion of sero-sanguinolent fluid into the cellular tissue of that part. The swelling itself, and the effusion composing it, are, as you know, precisely of that kind which is known under the name of *caput succedaneum*, with this exception—that its edges are not so very precisely defined. I have seen many children who have evidently died in utero some time previously to the accession of parturition, present a similar cranial effusion to this; and in two or three cases I have observed it, as in A——'s child, even when the infant presented preternaturally. The effusion is so very like that of a caput succedaneum that most people would certainly take it for one, and hence I would have you deduce this observation, that a swelling of the scalp, with sero-sanguinolent effusion accompanying it, is not, as has been laid down by some medical jurists, either a mark of the

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[1] See page 152.

[2] The pages immediately following, formed part of a Clinical Lecture, the former portion of which, as especially treating of turning in cases of cross birth, we have placed in the section on Natural and Morbid Parturition. From the management of the labor of the woman A—— (see vol. i. p. 558), Dr. Simpson passes to consider the morbid conditions and malformations of her child.—(Ed.)

child having been alive at the commencement of labor, or of the presentation being that of the head. The fact is, in putrefying children, you have a sero-sanguinolent effusion into almost all the cellular tissue of the body, but it is greater in the scalp, and produces a more defined swelling there, in consequence of the cellular tissue of that part of the body being comparatively so loose in the child, and easily infiltrated.

2. Peritonitis in the fœtus in utero appears to be a frequent disease, and in consequence of the blanched state of the placenta and the swelled condition of the abdomen in A——'s child, I stated to the gentlemen present, before we opened its body, that we should in all probability meet with some evident traces of the previous existence of peritoneal inflammation. Accordingly, on exposing the abdominal cavity, we found, as you now see, a number of loose patches, and membranous shreds of coagulable lymph effused upon the surface of the peritoneum. These patches are in greatest abundance towards the lower part of the peritoneal cavity, especially upon the caput cœcum, and in its neighborhood. The surface of the spleen is coated at different points with an attached semi-membranous layer of coagulable lymph, and I have seen a few cases of intra-uterine peritonitis, in which the surface of the spleen did not, as in this instance, present the inflammatory effusion in a very marked degree. The spleen itself, in the child before us, is of an enormous size—an observation which I have made in two or three other instances of peritonitis in the fœtus.

3. The omphalo-mesenteric vessels persist as a malformation in this fœtus—that is to say, you perceive here, running from the inner or peritoneal side of the umbilicus, directly towards the mesentery of the smaller intestine, a firm band, nearly as strong and thick as a piece of saddler's silk, and which I now raise on the handle of the knife. You know the origin and course of the omphalo-mesenteric vessels to be exactly that of the band which I show you. The permanence of this band, as a malformation in the fœtus at birth, I have seen in one or two other instances:—in one case, in a fœtus in the museum, with appearances of intra-uterine hydrocephalus, and additional fingers, this malformation also exists. Spangenberg alleges that he found these omphalo-mesenteric veins present, and apparently partially pervious, in an adult who died of phthisis towards the age of twenty.

But returning to the Peritonitis, allow me to observe, that here as elsewhere, the only true evidence of previous existing inflammation in the fœtus consists in the presence of some of the *organic* products of inflammation such as coagulable lymph, and the pseudo-membranous adhesions which such lymph so readily produces when thrown out on serous surfaces, the presence of pus, ulceration, &c. We can in no degree, depend upon color alone, or upon serous or sero-sanguinolent effusions, as these are often the result of mere putrefaction after death.

I have just stated that Peritonitis as a disease of the fœtus in utero is very frequent. For my own part, I believe that you will find evidence of it in most of those children that are born in a semi-putrefied state, and that have perished several days previous to birth, while there is no disease in the placenta or other appendages to account for the death of the infant.[1] I formerly published some twenty or thirty instances in which the fœtus was found to present after birth, evidences of the previous existence of acute peritoneal inflammation; and since that time I have had occasion to observe the disease in a number of other additional cases. Two or three weeks ago I took an opportunity of showing you a recent specimen of it in a fœtus that was born about the sixth month of utero-gestation. Since my observations were published, Dr. West, of London, and Dr. Otto of Breslau, have each recorded a number of similar cases.

This special disease of the fœtus—*peritonitis*—is interesting and important in one point, to which I wish to direct your attention, namely, as being liable to occur successively in different children of the same mother, and thus sometimes producing a series and succession of premature still-born infants. In the case before us, the mother had a still-born child about a year ago.

The conclusion of this lecture, which proposes treatment for the various diseases of the placenta, will be found at a future page.—(Ed.)

BIRTH OF A DOUBLE MONSTER.[2]

ONE CHILD ALIVE.

(From Edinburgh Monthly Journal of Medical Science, Aug. 1848, p. 133.)

A case of double monstrosity some time ago occurred in the practice of Dr. Lyell of Dundee, who has kindly forwarded the infants for anatomical examination. This monster belongs to Geoffroy St.

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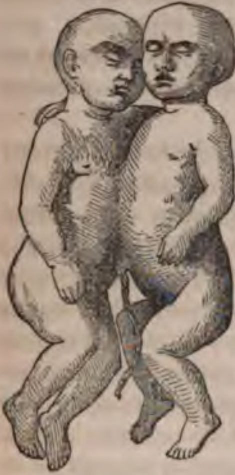
[1] Edinburgh Medical and Surgical Journal for 1838 and 1839.

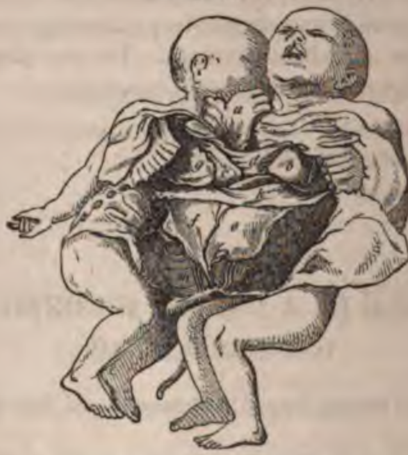
[2] Extracted from Proceedings of Edinburgh Obstetric Society, May 10, 1848.

Fig. 12.



Hilaire's[1] family of *Monomphaliens*, characterized by the of two almost perfect individuals having a common umbi and is of the genus *Sternopage*, in the classification. There is a junction or fusi the two fœtuses, extending from immed below the umbilicus to the top of the (Fig. 12). The united sterna are divid cleft in the middle, and thrown to either so as to make, as it were, one vast th cavity for the two bodies. In this cavity is a common pericardium (*d*, Fig. 13), co ing two distinct and perfect hearts, *b c* four perfect lungs are present. The ca however, of the pleuræ are not fused thrown into one, but remain separat a double serous layer, composed by th flection of the pleuræ. There is a singl phragm, *k*, and a large liver, *ef*, common to lying in the mesial line, and over whic two peritoneal coatings are reflected, one over each half, fo also its suspensory ligament in the mesial line above, and the se between the two abdominal cavities by their union below. the conjoined or common liver, at both sides of the serous di ment of the two peritoneal cavities, an umbilical vein, *m n*, i These two umbilical veins, with their corresponding four um arteries, converge to form the cord, which issues single fro

Fig. 13.



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[1] St. Hilaire, Histoire des Anomalies, Paris, 1832, tom. iii. p. 93.

umbilicus; but at a length of about eight inches, this compound umbilical cord divides into two stems, each about two inches long, which were inserted separately into a common or single placenta.

The history of the labor is detailed in the following letter from Dr. Lyell, received a few days after the woman's delivery:

"On the evening of Monday, the 2d November, 1846, I was called, about half past eight P. M., to attend Mrs. A——, in labor with her second child. She had her first child, a boy, twenty months ago; and then her labor was so quick that no doctor was sent for. On my arrival about nine o'clock, I ascertained that she had been ill about three quarters of an hour. On examination, I found the vertex presenting, the os uteri dilated to the size of a crown-piece, and very dilatable. She complained of the pains being strong; but as they were not at all expulsive, I ruptured the membranes. As this did not mend matters, I exhibited a dose of ergot, but it had no better effect. A hand was felt coming down along the side of the head. From the circumstances of the pains being so strong, and there being no appearance of the head being impacted, I began to suspect there were twins, and that the one child was, in some way, preventing the delivery of the other. I, therefore, thought it would be necessary to give her some assistance with the forceps. At 12 P. M. I proceeded to apply them, and succeeded in effecting the delivery of the head in a few minutes, but I could not make the body descend and when I stopped pulling, the head was forcibly drawn back against the external parts. I tried pretty strong traction again, and the neck yielded, as if the head had partially separated from the body. On throwing aside the bed-clothes and examining the head, I discovered the presence of harelip and cleft palate in the child, and as it made several convulsive movements, I was anxious to effect the delivery as soon as possible, that its life might be saved. I brought down first one arm and then another, but still could not get away the body. The face looked towards the pubis. At this time I felt a third hand descending, and now there was no doubt of there being a second child. I introduced my right hand for the purpose of changing its position, but found it so wedged, lying across the brim of the pelvis, that its position could not be altered. The lower part of the body of the half-delivered child lay across the loins of the second, and was thus prevented from entering the brim. I withdrew the right hand, and, after reflecting for a little, introduced the left hand along the spine of the first child to the nates, and succeeded in dragging its body into the pelvis, but all advance was again stopped. Anticipating the necessity of turning the second child, I seized and dragged its limbs along with the still undelivered limbs of the first child, and at last, after many efforts, succeeded in completing the delivery at half-past twelve P. M., the two infants revolving, as it were upon their common sternum, around the symphysis pubis of the mother. From the first application of the forceps to the completion of delivery, from twenty to twenty-five minutes elapsed. The first child having the harelip, was alive, and lived eighteen hours. The second was born dead. They were of full size, eighteen inches long, and, on the whole, well-formed. The mother made a good recovery."

The preceding case is most interesting *obstetrically*, from the accurate description of the mechanism of the delivery as described by Dr. Lyell—there being few details yet on record of the mechanism of parturition in instances of double monstrosity. The practice adopted by Dr. Lyell seems the very best that could possibly have been adopted under the circumstances. The wonder is, the frequency with which double monsters have been born without almost a mother ever being lost in consequence of this complication. In most cases, the fœtuses seem to overlap, and adapt themselves to the inequalities of, each other; and, when turning or extraction by the feet is adopted, it is usually found that pulling by the feet of *one* body—after all the four are down—is preferable, as the one body is thus allowed to overlap the other, and the head of the first child, for instance, to be pulled down, and adapted to the neck of the second, so far very greatly saving size and space.

The case is interesting *physiologically*, from one of the children having survived for eighteen hours after birth. Isidore St. Hilaire cites five or six cases of *Sternopage* monstrosity, as the whole yet reported by different authors; and Otto[1] has more lately described nine additional instances; but in only one known example did any of the children survive its birth. This exceptional case is described by Beaussier,[2] who states that one of the fœtuses was dead born, but the other lived sufficiently long for the malformed being to be carried to church for baptism. The possibility of the survival of either of the two children after birth, has, in almost all cases, been prevented by the hearts of the two fœtuses being more or less conjoined and fused together, and by their intercommunicating by one or more cavities. In one instance recorded by Uccelli,[3] the hearts were separate, but of unequal size. In Dr. Lyell's monstrosity—though the two hearts lay in a common pericardium, they presented this rare peculiarity, that they were quite distinct from each other, of the same size, perfect in their individual anatomical formation, and consequently perfect also and independent so far as regarded their physiological action.

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[1] Monstrorum Sexcentorum Descriptio, p. 172, &c. [2] Journal de Médecine, &c., for 1770, p. 9.

[3] Memor. dell. Societa Italiana, tom. xi. p. 123.

HEPATA SUCCENTURIATA.[1]

(From Edinburgh Monthly Journal of Medical Science, Feb. 1855, p. 179.)

Cases of malformation in which there exist small additional spleens, or speen-lobes, are sometimes met with. Instances of analogous malformations of the liver are infinitely more rare. I lately found a specimen of additional or accessory hepatic lobes, more marked and complete, I believe, than has been described in any of the very few cases hitherto mentioned by authors. It occurred in a fœtus which I dissected with Dr. M'Cowan.

The child was born with a large umbilical hernia or abdominal extroversion. The umbilical cord was provided with only one artery. The mass of the liver was of the ordinary size; but two additional lobes projected from its anterior edge, and were affixed to the inner surface of the abdominal walls. These two additional lobes were flat and button-shaped, being each about the size of a sixpence. They were connected to the anterior border of the liver by prolongations or thin bands of tissue, the thickness of whip-cord, and about an inch long. On microscopic examination by Dr. Priestley, they were found to consist of hepatic lobules, identical in general conformation and structure with those of the liver itself.

The supernumerary lobes seemed, in fact, like two small portions of the liver which had become morbidly adherent during development to the opposite peritoneum, and been gradually drawn out and pediculated, during the further growth of the parts. The intestines were also in several parts morbidly and firmly adherent to the abdominal peritoneum.

The case was probably an illustration of the origin or commencement of some malformations, in the results or effects of peritoneal inflammation and inflammatory adhesions in the early fœtus.[2]

HERMAPHRODITISM.

(From the Cyclopædia of Anatomy and Physiology, 1839, vol. ii. p. 684.)

Hermaphroditism, or Hermaphrodism;[3] *Hermaphrodisia; an-*

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[1] Extracted from Proceedings of the Edinburgh Obstetric Society, session xii.

[2] See a preceding discussion on this point at p. 191.

[3] From the well-known mythological fable of the union into one, of the bodies of Hermaphroditis, the son of Ερμης, *Mercury*, and Αφροδιτη, *Venus*, and the nymph Salmacis.—See Ovid's Metamorphoses, lib. iv. fab. 8.

drogynisme, *gynandrisme; hermaphroditisme*, &c., of the French; *ermaphrodismo* of the Italians; *Zwitterbildung* of the Germans, &c.

Many different definitions of hermaphroditism, and almost an equal number of different classifications of the malformations usually comprehended under it, have been proposed by the various authors, ancient and modern, who have directed their attention to this subject. Without stopping to discuss the merits or errors of these definitions and classifications, and without inquiring as some have done, into the propriety of the word itself, we shall content ourselves with stating that, under it, as a convenient generic term, we purpose in the present article to include an account—1st, of some varieties of malformation in which the genital organs and general sexual configuration of one sex approach, from imperfect or anormal development, to those of the opposite; and 2d, of other varieties of malformation, in which there actually coexist upon the body of the same individual, more or fewer of the genital organs and distinctive sexual characters both of the male and female.

To separate from one another, by as strong a line as possible, the two distinct varieties of hermaphroditic malformation marked out in this definition, we shall divide hermaphroditic malformations, considered as a class, in the two orders of *Spurious* and *True;* the spurious comprehending such malformations of the genital organs of one sex as make these organs approximate in appearance and form to those of the opposite sexual type; and the order, again, of true hermaphroditism including under itself all cases in which there is an actual mixture or blending together upon the same individual of more or fewer of both the male and female organs.

Spurious hermaphroditism may occur either in the male or female; that is, there may be, from malformation of the external sexual organs, an appearance of hermaphroditism in persons actually of the female sex, or from a similar cause there may be an appearance of hermaphroditism in persons actually of the male sex. The differences derived from the diversity of sex in which spurious hermaphroditism occurs, and the particular varieties of malformation in each sex which may give rise to it, will serve as bases on which we shall found some further subdivisions of this order.

True hermaphroditism, as above defined, comprehends also, as will afterwards be more particularly shown, several very distinct varieties of malformation. If we conceive for a moment all the reproductive organs to be placed on a vertical plane, as we may suppose them to be, though not with strict correctness, in the human body when in the erect posture, we shall find that the principle of these varieties may be all referred to three sets of cases:—1st, those in which, if we draw a vertical median line through this supposed the two lateral halves are seen to present organs differing in ɟpect, that they belong to opposite sexual types; 2d, others ch, if we bisect the same plane by a transverse horizontal ıere exist organs of a different sex in the upper from those t in the lower segment; or, in other words, in which the ıl genital organs belong to one sex, and the external to r. In the two preceding classes of cases there is not neces- as we shall afterwards more fully point out, any malformation *licity* in the sexual apparatus of the malformed individual; .s only one set of sexual organs present, but in some parts ırgans are formed upon the male, and in others upon the type. In the 3d, and remaining, set of cases, however, there y present, to a greater or less, though most generally only to partial, extent, a double set of sexual organs, having oppo- ɛual characters, so that upon the same body, and usually upon ne side, or upon the same vertical line in our supposed plane, l coexisting two or more of the analogous organs of the two

In accordance with this view, we shall consider the cases of ermaphroditic malformation under the three corresponding ns of—1st, *lateral;* 2d, *transverse;* and 3d, *vertical*, or, more ly, *double* or *complex hermaphroditism;* and each of these will admit of some further convenient subdivisions. But ›de in which we propose to classify and consider the subject obably be at once more accurately gathered from the follow- ›le, than from any more lengthened remarks upon it in the t place.

Classification of Hermaphroditic Malformations.

ıroditism.	Spurious.	In the Female.	From excessive development of the clitoris, &c.
			From prolapsus of the uterus.
		In the Male.	From extroversion of the urinary bladder.
			From adhesion of the penis to the scrotum.
			From hypospadiac fissure of the urethra, &c.
	True...	Lateral....	Testis on the right, and ovary on the left side.
			Testis on the left, and ovary on the right side.
		Transverse.	External sexual organs female, internal male.
			External sexual organs male, internal female.
		Vertical or Double.	Ovaries and an imperfect uterus, with male vesiculæ seminales, and rudiments of vasa deferentia.
			Testicles, vasa deferentia, and vesiculæ seminales, with an imperfect female uterus and its appendages.
			Ovaries and testicles coexisting on one or both sides, &c.

ɔommenting upon and illustrating the different varieties of hermaphroditism, in the particular order in which they are placed in the above table, we shall, we believe, by following that order, be able to take a graduated, and, at the same time, a correct and comprehensive view of the subject, beginning with the more simple, and ending with the more complex and complete species of hermaphroditic malformation, as seen in the primary sexual characters, or the structure of the genital parts themselves. We shall then consider at some length the curious and important physiological subject of hermaphroditism as manifested in the secondary sexual characters of the system. After having done so, we shall endeavor to show how far the diversified forms of hermaphroditic malformation can be explained upon our present knowledge of the laws of development; point out the actual anatomical and physiological degree of sexual duplicity which is liable to occur, and the numerous fallacies with which the determination of this question in individual cases is surrounded; and lastly, in conclusion, we shall offer some general observations upon the causes, &c., of this class of abnormal formations.

A.—SPURIOUS HERMAPHRODITISM IN THE FEMALE.

There are two circumstances in the conformation of the genital organs of the female, the existence of each of which has occasionally given rise to doubts and errors with regard to the true sex of the individual on whom they were found—namely, 1st, a preternaturally large size of the clitoris; and 2d, a prolapsus of the uterus; the enlarged clitoris in the one case, and the protruded uterus in the other, having been repeatedly mistaken for the male penis.

1. *Abnormal development or magnitude of the clitoris.*—In the earlier months of intra-uterine life the clitoris of the human female is nearly, if not altogether, equal in size to the penis of the male fœtus; and at birth it is still relatively of very considerable dimensions. From that period, however, it ceases to grow in an equal ratio with the other external genital parts, so that at puberty it is, as a general law, found not to exceed six or eight lines in length. But in some exceptional instances the clitoris is observed to retain up to adult age more or less of that greater proportionate degree of development which is presented in the embryo of the third and fourth month, thus exhibiting in a persistent form the transitory type of structure belonging to the earlier stages of fœtal life. In some instances where this occurs, the resemblance of the external female to the external male parts is occasionally considerably in-

creased by the apparent absence of the nymphæ. Osiander[1] endeavored to show that at the third or fourth month of fœtal life the nymphæ are very imperfect, and so very small as not to be easily observed. Meckel,[2] however, has pointed out that these organs are not in reality of a small size at that time, but they are liable to escape observation from the folds of skin of which they consist, making, at the period alluded to, a perfectly continuous membrane with the prepuce of the clitoris, and forming indeed, in their origin, only one common mass with this latter body. When the ulterior changes, therefore, which these parts ought to undergo in the natural course of development in the latter stages of fœtal existence, are suspended or arrested from about the end of the third month, there may not only coexist with the enlarged clitoris an apparent want of nymphæ, but the resemblance of the female to the male parts may be still further increased by the persistence of the original intimate connection of the nymphæ with the prepuce and body of the clitoris, and by the consequently continuous coating of integuments, as well as the greater size and firmness of this organ.

Excessive size of the clitoris would seem to be much less common among the natives of cold and temperate than among those of warm countries. The frequency of it in the climate of Arabia may be surmised from the fact, of directions having been left by Albucasis and other surgeons of that country, for the amputation of the organ; an operation which Ætius and Paulus Æginetus describe as also practised among the Egyptians. According to the more modern observations of Niebuhr[3] and Sonnini,[4] circumcision would seem to be still practised upon the females of that country.

This variety of conformation of the female parts appears to have been well known to the ancient Greeks, and several of their authors have mentioned the women so constituted under the names of τριβαδες and εταιριστριαι, a class in which the celebrated poetess Sappho (*mascula Sappho*) is well known to have been included. Martial, Tertullian, and other Roman authors, have noticed the same malformation (*fricatrices*, *confricatrices*), and have alluded to the depravity to which it led.[5]

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[1] Abhandlungen über die Scheidenklappe, in Denkwurdigkeiten für die Heilkunde, Bd. ii. pp. 4–6.

[2] Manuel d'Anat. Gen., tom. iii. p. 666.

[3] Beschreibung von Arabien, s. 77.

[4] Voyage dans la Haute et Bassa Egypte, tom. ii. p. 37.

[5] Mart. Epigr. lib. i. ep. 91; see also lib. viii. ep. 66. The frequency of this crime in the ancient gentile world may be inferred from the pointed manner in which the Apostle Paul alludes to it, Romans, chap. i. 26. In Greece it was in some places forbidden by law, and in others, as in Crete, tolerated by the state. Seneca, in his 95th ep. when speaking of the depravity of the women of his own age, remarks, "non mutata fœminarum natura, sed vita est . . . Libidine vero. nec maribus quidem cedunt pati natæ. Dii illas deæque male perdant, adeo perversum commentæ genus impudicitiæ viros ineunt" Op. Om. Genev. 1665.

The dimensions which the clitoris occasionally presents are such as to render it, in respect of size alone, not unlike the male penis. It is not unfrequently found of two or three inches in length, but sometimes it is seen five and six inches long. Dr. Clarke frequently found the organ an inch long, and thick in proportion, among the Ibbo and Mandigo women.[1]

Haller[2] and Arnaud[3] have collected numerous instances of preternatural size of the clitoris. The former author alludes, among others, to two cases in which the organ was stated to have been seven inches in length; and to another, mentioned by Chabart, in

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p. 787. Clemens Alexandrinus, in his Pædagogus, exposes the same vice: "et contra naturam fœminæ, viros agunt (*ανδριζονται*) et nubunt et etenim uxores ducunt." Also Athenæus, Deipnosoph, lib. xiii. p. 605. Justin Martyr, in his Second Apology, makes a still broader accusation. This author lived in the second century, and in declaiming against the vices of that licentious age, he alleges that multitudes of boys, females, and hermaphrodites (*androgyni ambigui sexus*) "nefandi piaculi gratiâ per nationem omnem prostant." Op. Om. Col. 1686, p. 70. See also Marcus Antoninus, De Seipso, ed. Gatakeri, Cambr. 1652, lib. iii. note at the end by Gatakar. On the extent, among the ancients, of the vices above alluded to, see Meiner's Geschichte des Verfalls der Sitten und der Staatsverfassung der Römer, Leipzig, 1791; Neander's Denkwurdigkeiten, Bd. i. s. 143; Professor Tholuck's, of Halle, Exposition of St. Paul's Epistle to the Romans, in the Edinburgh Biblical Cabinet, vol. v. p. 102, and in an Essay on the licentious vices, &c., of the ancients, translated into Robinson's American Biblical Repository, vol. ii. p. 441. In the essay last referred to, Tholuck incidentally mentions (p. 422), that the Deity Mitra (Mithras of the ancient Persians) was hermaphrodite. For our own part, we are inclined to believe that many of the idols of the heathenish mythology of Asia could be traced to the deification of various monstrosities in man and quadrupeds. (See the figures of these idols *passim* in Coleman's Mythology of the Hindus, Lond. 1832; and Upham's History and Doctrine of Budhism, Lond. 1829.) It is perhaps not unworthy of notice that the Jewish Talmudists, taking the Hebrew noun in the Pentateuch answering to man in its individual and not in its collective sense, considered from Genesis, chap. i. v. 21, that our original progenitor was hermaphrodite. (See Jus Talmud. Cod. Erwin., c. 2; Heidegg. Hist. Patriarch., tom. i. 128; C. Bauhin, De Monstrorum Naturâ, &c., lib. i. c. 24; and Arnaud's Mémoire, p. 249.) It is further interesting to remark that Plato, in his Symposion, introduces Aristophanes as holding the same opinion. "The ancient nature," he observes, "of men, was not as it now is, but very different; for then he was androgynous both in form and name (*ανδρογυνον και ειδος και ονομα.*)" Probably from the licentious purposes alluded to by Justin Martyr, or from the weak and imbecile character of hermaphrodite individuals, the word *ανδρογυνος* came in latter times to signify effeminate and luxurious. The ancient lexicographer Hesychius gives it this meaning; and Theodoret, in his Therap., speaks of Bacchus as being licentious, effeminate, and androgynous—(*γυννις ων, και θηλυδριας, και ανδρογυνος*).

[1] Home's Comp. Anat. vol. iii. p. 317. On the peculiarities of the external genital organs in various African tribes, see a learned paper by Prof. Muller in his Archiv fur Anatomie for 1834. Heft. iv. s. 319, with ample references to the observations and opinions of Levaillant, Barrow, Peron, Lesner, Lichtenstein, Burchel, Somerville, &c. See also Otto, in his Neue Seltene Beobachtungen zur Anatomie, p. 135, showing the very prominent external female parts of different African tribes to consist differently—1, of enlarged nymphæ, 2, of enlarged labia, and 3, of the enlarged clitoris.

[2] El Phys. tom. vii. part. ii. pp. 81-2.

[3] Dissertation sur les Hermaphrodites, p. 372. See also Homberg, De Excrescentiâ Clitoridis nimiâ, Jena, 1671; Tronchin, De Clitoride, Lugd. 1736; and Ploucquet's Literatura Medica, art. Clitoris Magna, tom. i. p. 299.

which it was alleged to have been twelve inches—a size which we can only conceive to have been the result of disease.

When the female clitoris is increased greatly in size, it is not wonderful that it should be sometimes mistaken for the male penis —the female organ in the Mammalia naturally differing from the male only in regard to its smaller dimensions, its not being perforated by the urethra, and its wanting the corpus spongiosum—a peculiarity of defect of structure that exists as the natural type of formation in the penis of male reptiles. In the human subject, the organs are composed internally of the same kind of erectile tissue, and when we descend in the animal scale, and examine their relations in the male and female of the same species, we find some still more striking analogical peculiarities of structure. Thus, in several of the Carnivora and Rodentia, as in the lioness, cat, raccoon, bear, marmot, &c., the clitoris contains a small bone like that belonging to the penis of the male of the same species; and amongst the Monotremata and Marsupiata, the clitoris of the female, like the penis of the male, is surmounted by a bifid glans. In a species of lemur (*Loris gracilis*, or *Stenops tardigradus*), the clitoris is of a very large size; and the urethra, as first pointed out by Daubenton,[1] runs forward and opens at its anterior extremity between the branches of its glans, imitating, in this point of structure, the penis of the male among the Mammalia.

In the human subject the mere enlargement of the clitoris alone has seldom of itself given rise to errors with regard to the sex of the individual, except in young children; but it has frequently happened that along with it other minor malformations have coexisted so as to render the sexual distinction much more ambiguous. In women possessing this peculiarity of structure we sometimes observe, for instance, the clitoris not only resembling the penis in size, but it has an indentation at the point of the glans, imitating the orifice of the urethra; and occasionally the glans is actually perforated to a certain extent backwards, or the body of the clitoris is drilled more or less imperfectly with a canal like that of the male urethra. In other instances the canal and orifice of the female vagina are, by an excess of development in the median line of the body, much contracted or nearly shut up, the vulva being closed by a strong membrane or hymen, and the labia cohering, so as to give the parts a near resemblance to the united or closed perineum and scrotum of the male. Further, in one or two very rare cases which have been put upon record, the ovaries and Fallopian tubes seem to have descended through the inguinal rings into the labia, thus giving an appearance of the presence of testicles; and a fallacy seems to have occurred in some cases from the presence of roundish masses of fat in this situation, simulating more or less the same male organs.

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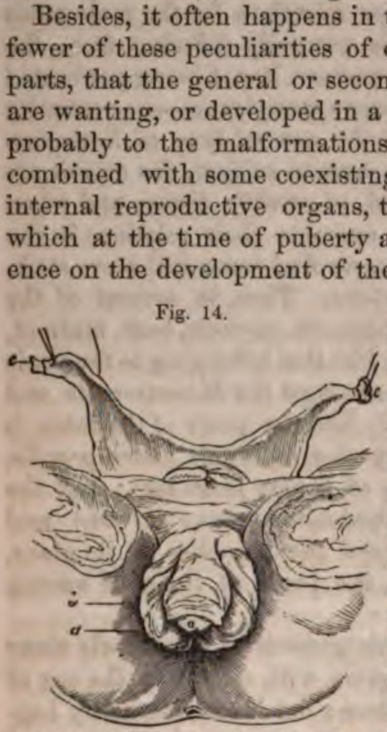
[1] Audibert, Histoire Nat. des Singes, tab. ii. fig. 8.

Besides, it often happens in those women who present more or fewer of these peculiarities of conformation in the external genital parts, that the general or secondary sexual characters of the female are wanting, or developed in a slighter degree than natural, owing probably to the malformations of the external organs being often combined with some coexisting anomalies in those more important internal reproductive organs, the healthy structure and action of which at the time of puberty appear to exercise so great an influence on the development of the peculiar general conformation and moral character of the female. Thus the features are sometimes hard, the figure and gait rather masculine, the mammæ slightly developed, the voice is deep-toned, and the chin and upper lip are occasionally covered with a quantity of hair. In fact, in some marked cases the whole external character approaches to that of the male, or, more properly speaking, occupies a kind of neutral ground between that of the two sexes. Some of the more striking examples of this first variety of spurious hermaphroditism in the female will sufficiently illustrate the above remarks.

Fig. 14.



Dr. Ramsbotham[1] has briefly described the genital parts of an infant, that was christened and looked upon as a boy, until dissection after death showed that the sex was actually female. The uterus and other female organs (Fig. 14, *c c*) were present and apparently naturally formed; but the clitoris, *b*, was fully as large as, and in appearance closely resembled, the penis of a male of the same age. At its anterior extremity there was a sulcus, *a*, which was not the entrance of the urethra, but terminated in a cul-de-sac.[2]

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[1] Medical Gazette, p. 184.

[2] In Guy's Hospital Reports for 1840, p. 243, there is a succinct account and excellent drawings of a case of this variety of spurious hermaphroditism. The subject was an adult. The ovaries are small; but the Fallopian tubes and uterus were otherwise normal. The vagina, about three inches long, entered inferiorly into the canal of the urethra. About an inch after this junction the urethra opened externally, as in the female; but there was no vulva. The labia externa were united together, so as to represent a scrotum, and projected on each side as if they contained testes; on dissection, however, these projections were found merely masses of fat. The clitoris was elongated to two inches, and proportionally

Columbus[1] and De Graaf[2] give two similar examples of the same form of spurious hermaphroditism in young children, in which the true sex was only fully ascertained by dissection after death. In relation to the clitoris in the case described by Columbus, that author states that this organ was furnished with two muscles only, and not with four, as in the perfect female.

In a reputed hermaphrodite woman, Gallay[3] found after death the clitoris to be three and a half inches long, and three inches and four lines in circumference. The glans and prepuce were well developed. The urethra ran as in man through the body of the penis and its glans. The labia, nymphæ, vagina, &c., were natural, and the internal female organs, the ovaries, Fallopian tubes, and uterus, are described as scirrhous. This woman had been married, but never had any children; her catamenia, however, had been very regular. She had a considerable quantity of hair upon her face, and her voice was harsh and masculine.

In a child, two years of age, Schneider,[4] on dissection after death, could find neither the labia externa nor interna, nor any trace of the ordinary cleft between them. The clitoris was an inch and a half long, and externally resembled most perfectly a male penis furnished with a glans and prepuce; but it was imperforate, having only at its anterior extremity a small spot marking the situation of the opening of the urethra in the male. Some lines below there was an opening by which the urine was evacuated. This opening formed the entrance to the vagina, which was found of the usual length and with the characteristic rugæ. The canal of the urethra was found entering its roof, but in such a manner that the urine was always evacuated very slowly and by drops only, from the external opening. All the internal female sexual organs were natural.

M. Beclard[5] has left us a very detailed and interesting description of an example of spurious hermaphroditism referable to the present variety, and exhibited at Paris in 1814. The subject of the case, Marie Madeleine Lefort, was at that time sixteen years of age. The proportions of the trunk and members, and of the shoulders and pelvis, and the conformation and dimensions of the last part of the body, were all masculine; the volume of the larynx also, and

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increased in thickness; its glans was large. The case is described by Sir Astley Cooper. Another analogous instance with an autopsy is given by Dr. Neill in the American Journal of Medical Science for 1851, p. 558. All the internal organs were female; the representative penis was five inches long; the urogenital opening at its root was very small.

[1] De Re Anatomicâ, lib. xv. p. 493.

[2] Op. Om. cap. iii. xv., or De mulierum organis gen. inserv., with a plate.

[3] Arnaud, loc. cit. p. 309.

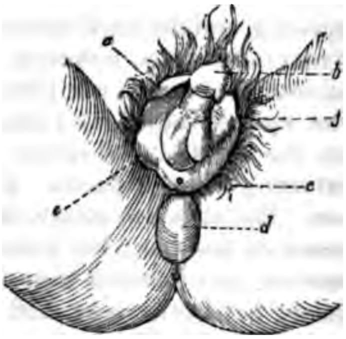
[4] Jahrbücher der Staatsarzneikunde, 1809, s. 193.

[5] Bulletin de la Faculté for 1815, p. 273.

the tone of the voice, were those of an adolescent male; a beard was appearing on the upper lip, chin, and region of the parotids; some hairs were growing in the areola around the nipple; and the mammæ were of a moderate size. The inferior extremities were furnished with an abundance of long hard hairs. The symphysis pubis was elongated as in man; the mons veneris rounded, and the labia externa were covered with hair. The clitoris was 10½ (?) inches (27 centimetres) in length when at rest, but somewhat more when erect; its glans was imperforate, and covered, in three-fourths of its circumference, with a mobile prepuce. The body of this enlarged clitoris was furnished inferiorly with an imperfect canal, which produced a depression in it, instead of that prominence of this part which exists in the male penis. This canal was pierced along its under surface and median line by five small holes, capable of admitting a small stylet; and one or more similar apertures seemed to exist in it after it passed backwards within the vagina. The labia were narrow and short, and the vulva or sulcus between them were superficial, being blocked up by a dense membrane, which, under the pressure of the finger, felt as if stretched towards the anus over a cavity. At its anterior part, or below the clitoris, there was an opening capable of admitting a sound of moderate size, and this sound could be made to pass backwards behind the membrane closing the vulva, which, when felt between the point of the instrument and the finger, seemed about twice as thick as the skin. The urine was passed by this opening, and also, according to the report of the individual herself, through the cribriform holes in the canal extending along the inferior surface of the urethra. By the same opening the menstrual fluid escaped, as Beclard ascertained on one occasion by personal examination. She had menstruated regularly from the age of eight years, considered herself a female, and preferred the society of men.

In this interesting case, we have present all the secondary sexual characters of the male, with some of the female genital organs developed in so excessive a degree as to approach in several points their more perfect structure in man. The impossibility, however, as mentioned by Beclard, of finding any bodies like testicles in the labia, or in the course of the inguinal canals, and more particularly the well-ascertained fact of the individual menstruating, can leave no doubt as to the nature of her sex. The perforation of the enlarged clitoris by the imperfect urethra is interesting, when compared with the peculiarities, that we have formerly alluded to, of this part in the female Loris, as pointing out—what we have so often occasion to observe in human monstrosities—a type of structure assumed by a malformed organ similar to the normal type of structure of the same organ in some of the inferior animals.

Fig. 15.



Arnaud[1] has represented and described at great length an interesting example of hermaphroditic malformation that seems referable to the head of spurious hermaphroditism in the female, although there are two circumstances in the history of the case which have led some authors to doubt the accuracy of this opinion; and the opportunity that was afforded of ascertaining the true structure of the parts after death was unfortunately lost through carelessness and neglect. The subject of the malformation, aged 35, passed in society for a female, and came to Arnaud complaining of a small tumor (Fig. 15, *e*) in the right groin which had much incommoded her during her whole life. On examining this body, Arnaud was led to believe that it was a testicle, and he found a similar tumor, *f*, situated nearer the inguinal ring on the left side. The bags that contained them represented very exactly the labia externa. The clitoris, *a*, was two inches and nine lines in length, and placed between the labia at their upper angle. The glans, *b*, was well formed, and, though imperforate at its extremity, it presented a small depression which ran backwards along the whole inferior border of the clitoris, indicating the situation of a collapsed urethral canal, that seemed pervious for some length at its posterior part, as it became distended when the patient evacuated the bladder. The orifice, *c*, however, from which the urine actually flowed, occupied the situation in which it exists in the perfectly formed female. There was not any vaginal opening, and the individual menstruated per anum. At each menstrual period a tumor, *d*, always appeared in the perineum, which gradually increased in size, becoming, in the course of three or four days, as large as a small hen's egg. When the perineal tumor had reached this size, blood began to flow from the anus, although no hemorrhoids or other disease of the bowel was present. At these periods the individual had often experienced very alarming symptoms, and in order to avert these, Arnaud was induced to make an opening into the soft yielding space at which the perineal tumor above alluded to appeared; and at a considerable depth he found a cavity, two inches in circumference, and about two and a half in breadth, having projecting into it at one point an eminence which was supposed from its situation to be possibly the os uteri. At the next period the menstrual fluid came entirely by the artificial perineal opening, and the usual severe attendant symptoms did not supervene. From inattention, however, to the use of the tent, the opening was allowed to become completely shut, so that at the sixth return of the menses they flowed again by the anus, and were accompanied by the old train of severe symptoms. The individual lived for several years afterwards. Her conformation of body was remarkable. Her skin was rough, thick, and swarthy; she had a soft black beard on her face; her voice was coarse and masculine; her chest narrow; her mammæ were flat and small; her arms lean and muscular; her hands large, and her fingers of very considerable length and strength. The form, in fact, of the upper part of her body was masculine, but in the lower part the female conformation predominated. The pelvis was wide and large, the os pubis very elevated, the buttocks large, the thighs and legs round, and the feet small.

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[1] Dissertation sur les Hermaphrodites, p. 265, pl. x.

In this remarkable instance, if we do not go so far as to conceive the coexistence of some of the internal organs of both sexes, we must, from the well-ascertained fact of the menstrual evacuations, allow the person at least to have been a female. In that case we can only suppose the tumors in the labia to be the ovaries descended in that situation; and to the same excess of development which had produced this effect, we may attribute the closure of the vaginal orifice, and the formation of the imperfect urethral canal in the body of the clitoris.

Spurious hermaphroditism from preternatural enlargement of the clitoris has been recognized among some of the lower animals. Rudolphi[1] has noticed a mare of this kind that had a clitoris so large as almost to shut up the entrance into the vagina. Lecoq[2] has detailed the case of a calf, which Gurlt[3] believes to belong to the present head. Neither testicles nor scrotum were observed externally, and the penis or enlarged clitoris, which occupied its normal situation, was apparently perforated by the urethra, and crooked upwards, so as to throw the urine in that direction. Mery[4] showed by dissection the true sex of a monkey, the length of whose clitoris had deceived some observers with regard to the true sex of the animal. The enlarged clitoris was furrowed on its inferior surface. The clitoris of the female Quadrumana is, as will afterwards be more particularly mentioned, relatively larger than in the human subject, and retains in a greater degree the size and type of structure of this organ in the embryo.

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[1] Bemerkungen auf einer Reise, &c., Bd. i. s. 79. See also a case figured by Ruysch in his Thesaurus Anat. lib. viii. No. 53.

[2] Journ. Prat. de Méd. Vet. 1827, p. 103.

[3] Lehrbuch der Pathol. Anat. Bd. ii. s. 193.

[4] Hist. de l'Acad. 1686, tom. i. p. 345.

Ve may here further mention, that, as pointed out by Blumen- h,[1] the clitoris and orifice of the urethra are placed at some dis- ce from the vagina and in front of it, in the rat, mouse, hamster,

This normal structure has sometimes been mistaken for an maphroditic malformation.[2]

. *Prolapsus of the uterus.*—It may at first appear strange that occurrence should ever lead to any difficulty in ascertaining the of the individual, though not only non-professional observers, even the most intelligent medical men, have occasionally been far misled by the similarity of the protruded organ to the male iis, as to mistake a female for a male. Of this circumstance some ious illustrations are on record.

I. Veay, physician at Toulouse, has inserted in the Philosophical insactions of London,[3] a brief account of the case of Marguerite lause or Malaure, who was entered as a female patient in the ulouse Hospital in 1686. Her trunk, face, &c., presented the ieral configuration of a female, but in the situation of the vulva re was a body eight inches in length when on its fullest stretch, l resembling a perfectly formed male penis in all respects, except not being provided with a prepuce. Through the canal perforat- ; this body she was alleged to evacuate her urine, and from its fice M. Veay had himself an opportunity of seeing the menstrual id flow. After being examined by several physicians she was nounced to be more male than female, and ordered by the civil horities to exchange the name of Marguerite for that of Arnaud, l to wear male attire. In 1693 she visited Paris in her male oiliments, and reputed herself endowed with the powers of both ces. The Parisian physicians and surgeons who examined her, m all to have accorded in opinion with the faculty of Toulouse, til M. Saviard[4] saw her, and detected the supposed penis to be rely the prolapsed uterus. He reduced the protruded organ, and red the patient. Upon the enigma of her hermaphroditism being is solved, she was permitted by the king, at her own request, to ume again her female name and dress.

Sir E. Home[5] detected a case of reputed hermaphroditism of the ne description as the last, in a French woman, twenty-five years age, who exhibited herself in London, and pretended to have the vers of a male. The cervix uteri was uncommonly narrow, and jected several inches beyond the external opening of the vagina. e everted mucous surface of the vagina had, from constant expo-

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Comp. Anat. p. 335.

[2] Doebel, in Nov. Liter. Maris Balthici, 1698, p. 238.

Phil. Trans., vol. xvi. p. 282.

Recueil d'Observations Chirurgicales, p. 150.

[5] Comp. Anat. vol. iii. p. 318.

sure, lost its natural appearance, and resembled the external skin of the penis. The orifice of the os tincæ had been mistaken for the orifice of the urethra. The prolapsus had been observed at an early age, and had increased as the woman grew up.

Valentin[1] mentions another analogous instance of sexual ambiguity produced by a prolapsus of the uterus. In this case the husband mistook the displaced organ for the penis, and accused his wife of having "cum sexu virili necquicquam commune."

A case quoted at great length by Arnaud[2] from Duval, of reputed hermaphroditism in a person that was brought up as a woman, and married at twenty-one years of age as a male, but who was shortly afterwards divorced and imprisoned, and ordered again by the Court of Rouen to assume the dress of a woman, appears to us to belong very probably to the present division of our subject, the reputed penis being described as placed *within* the vagina. The recorded details of the case, however, are not so precise as to leave us without doubt in regard to its real nature.

In cases such as those now mentioned, in which the prolapsed uterus, or, more properly speaking, the prolapsed uterus and vagina, have been mistaken for the penis, it appears probable that the neck of the uterus must have been preternaturally long and narrow, otherwise it would be difficult to account for the apparent small diameter and great length of the prolapsed organ. In Professor Thompson's collection of drawings of diseased anatomical structures, there is one of a uterus containing in its body a fibro-calcareous tumor, and having a neck three inches in length. M. Cruveilhier[3] has represented a similarly diseased uterus with a neck of between five and six inches. An organ shaped in this manner, whether from congenital malformation or acquired disease, would, when prolapsed for some time, represent, we conceive, a body resembling in form and size those observed in Saviard's and Home's cases.[4] The prolapsus arising from the protrusion of an ordinarily shaped uterus is generally of a greater diameter and roundness.

This second species of spurious female hermaphroditism is not observed among the lower animals.

B. SPURIOUS HERMAPHRODITISM IN THE MALE.

Malformed males have more often been mistaken for females than the reverse. The varieties of malformation in persons actually nale, that are liable to lead to mistakes with regard to their true ex, appear to be, 1st, extrophy or extroversion of the urinary bladler; 2d, adhesion of the inferior surface of the penis to the scrotum; nd 3d, and principally, fissure of the inferior part of the urethra nd of the scrotum and perineum.

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[1] Pandectæ Medico-Legales, tom. i. p. 38, Casus xii.

[2] Mém. sur les Hermaphr. pp. 314–18.

[3] Anat. Pathol. liv. xiii. pl. iv.

[4] On this longitudinal hypertrophy of the cervix uteri, see vol. i. of this work, p. 78. Some years ago there was a case of it in the Edinburgh Infirmary, where the prolapsed and elongated cervix uteri very exactly resembled a male penis.

1. *Extroversion of the urinary bladder.*—For a full description of his malformation, we must refer elsewhere.[1] This malformation is nown to occur more frequently in the male than in the female, and hen present in the former, it has occasionally given rise to a supposition of hermaphroditism, the red fungous mass formed by the nucous membrane of the protruded posterior wall of the bladder, nd situated above the pubis, having been mistaken for the female ulva. This error has probably been the more readily committed, rom the uterus and seminal ducts—and sometimes also, as in an nstance described by A. Fraenkel[2]—a part of the intestinal canal, pening upon the surface of the exposed portion of bladder. In ome instances of this malformation occurring in man, the external nale sexual organs are very imperfectly formed, or can scarcely be aid to be at all present. In other cases the scrotum is of the atural form, with the two testicles in it; and the penis is of considerable size, though almost always fissured on its upper surface from the epispadiac or open state of the urethra.

An example of supposed hermaphroditic malformation briefly described by Rueffe,[3] which seems referable to this variety, will be sufficient to illustrate it. "In the year 1519, an hermaphrodite or androgynus," he remarks, "was born at Zurich, perfectly formed from the umbilicus upwards, but having at this part a red mass of flesh, beneath which were the female genitals, and also under and in their normal situation those of the male."

2. *Adhesion of the inferior surface of the penis to the scrotum by a band of integument.*—This state of the parts has occasionally given rise to the idea of hermaphroditism, the penis being so bound down as not to admit of erection, and the urine passing in a direction downwards, so as to imitate the flow of it in the female.

In a boy, seven years of age, regarding whom Brand[4] was consulted, the penis was confined in this manner to the scrotum by abnormal adhesions. He had been baptized and reared as a girl, but by a slight incision the adherent organ was liberated, and the parents were convinced of the mistake that they had committed in regard to the sex of their child. The difficulty of determining the true sex of the boy was increased by the testicles not having descended into the scrotum.

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[1] Cyclop. of Anat. and Phys. Articles BLADDER and MONSTROSITY.

[2] De Organorum Generationis Deform. rarissimâ, Berlin, 1825, with a plate.

[3] De Conceptu et Generatione Hominis, p. 44.

[4] Case of a boy who had been mistaken for a girl, London, 1788.

Wrisberg[1] mentions two similar instances in persons of the respective ages of nineteen and forty-six. He relieved the adherent penis in the first case by operation.

3. *Fissure of the inferior part of the urethra, perineum, &c.*—HYPOSPADIAS.—This species of malformation, which has perhaps more frequently than any other given rise to the idea of the person affected with it being the subject of hermaphroditism, evidently consists in an arrest of the development of the external male sexual parts.

At an early stage of the development of the embryo, the various central sexual organs are, like all the other single organs situated in the median line of the body, found to be composed of two separate and similar halves, divided from each other by a vertical fissure, which, after the originally blind extremity of the intestinal canal has opened upon the perineum, forms a common aperture or cloaca for the intestinal canal, and also for the urinary and genital apparatus, both of which are, in their primary origin, prolongations from the lower part of that canal. After a time (about the second month in the human embryo), the opposite sides of this cloaca gradually approximate, and throw out two corresponding folds, which by their union constitute a septum that separates the rectum from the canal, or portion of the fissure, that still remains common to the urinary and generative organs; and, in the same way, by two similar and more anterior folds, the urethra of the female, and the pelvic portion of that of the male, is subsequently produced. After this, in the female the process of median reunion does not proceed further, and the primary perineal fissure remains, forming the vulva and vagina. In the male, however, the development, when normal, goes on to a greater extent, and the sides of the opening become so far united as ultimately to leave only the comparatively contracted canal of the urethra to serve as a common passage for both the internal urinary and genital organs; and the situation of the line of junction of the opposite sides of the original perineal cleft remains still marked out in the adult, by the raphé existing in the median line of the scrotum. The two lateral parts of the female clitoris unite together into one solid body, having on its under surface a slight groove or channel indicative of the line of conjunction of its two component parts; and the urethra is left to open at the root of

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[1] Comment. Med. &c., p. 534.

ɜ imperforated organ. In the male, on the contrary, the two
mitive halves of the penis, consolidated together at an early
ge along the course of their upper surfaces, come, about the
d month of development, to unite inferiorly in such a manner
h one another as to form a tubular prolongation of the pelvic
tion of the canal of the urethra, which is gradually extended
wards, first along the body of the penis, and ultimately through
glans. In the earlier periods of evolution, the præputium is still
iting; but as the penis becomes perforated, the præputium grows
apidly, as at last to cover and enclose the glans.

lany of the malformations to which the male genital organs are
le, may be traced to stoppages in the above process of develop-
nt, the character of the malformation depending upon the period
he development at which the arrest takes place, and varying con-
uently in degree from the existence of a cloaca or permanent
nitive fissure common to the intestinal, urinary, and generative
ans,[1] to that want of closure, to a greater or less extent in different
tances, of the inferior surface of the canal of the urethra in the
ly of the penis, or in its glans, which is generally known under
name of *Hypospadias*. When the development of the male or-
ıs, is arrested, immediately after the two septa respectively sepa-
ng the canals of the intestine and urethra from the original peri-
l cleft are formed, and consequently when this perineal fissure
l that running along the inferior surface of the penis are still
n, the external genital parts often come to present at birth, and
ing the continuance of life, a striking resemblance to the con-
mation of the external organs of the female, and the resemblance
frequently rendered greater by the coexistence of other malfor-
tions of the male organs. In these cases the imperfect and un-
eloped penis is generally of small size, and at the same time,
m being imperforate, may readily be mistaken for the clitoris; the
halves of the divided scrotum have the appearance of the two
ia externa; the two labia interna or nymphæ are sometimes re-
sented by the lateral divisions of the penis, forming two folds,
ich run backwards along the internal surfaces of the split scro-
ı; and the cleft in the perineum corresponds in situation and di-
tion, and occasionally also in size and form, with the canal of the
ina; this cleft is generally lined also by a red mucous membrane,
t is kept, like the natural female parts, constantly moistened by
secretions of the follicles with which it is provided; its mucous
mbrane occasionally presents irregular elevations, imperfectly
resenting the carunculæ myrtiformes; and further, the opening of

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[1] See on this malformation in the human subject (the normal form of structure in birds, &c.)
kel on Kloakbildung in his Path. Anat. Bd. i. s. 693.

the urethra at the root of the diminutive and imperforate penis serves still more to assimilate the malformed parts to the natural conformation of the female organs. In a number of cases, however, the apparent analogy to the female parts is rendered less striking by the perineal cleft being small or altogether absent, the urethral orifice at the root of the penis often forming the only opening leading to the internal urinary and generative parts, and the halves of the scrotum in such instances being frequently more or less united. Generally, the seminal ducts, and sometimes also the ducts of Cowper's glands, are seen opening on the surface of the urethra or supposed vaginal canal, at a short distance from its external orifice.[1]

In males malformed in the manner described, the testicles are seldom found in the divided scrotum at birth, but commonly they descend into it through the inguinal rings towards the period of puberty; and in several instances on record, in which the sex of the individual had been mistaken for that of a female, the tumors formed in the groin at that time by the organs in their descent have been erroneously regarded and treated as hernial protrusions. At the same time it occasionally happens that with the descent of the testicles, and the arrival of puberty, the diminutive penis enlarges in size, and the individual assumes more or less fully the habits and attributes of the male. In several instances on record this change has, under venereal excitation, appeared to occur suddenly, and persons formerly reputed female have thus unexpectedly found themselves provided with an erectile male penis. These various changes are occasionally postponed for a considerable period beyond the usual term of puberty.

In a few rare instances one testicle only descends through the inguinal ring, and occasionally they both remain throughout life within the abdomen, in or near the situation in which they were originally developed, imitating in this abnormal state the normal position of the same organs in many of the males among the lower animals. In a number of instances in which the testicles are thus retained within the cavity of the abdomen, they are found small and imperfectly developed, and from the want of their usual physiological influence upon the constitution, the whole physical and moral character of the malformed individual frequently presents a considerable approximation to that of the female, or, as we should perhaps more justly express it, never attains the perfection of the male, but preserves that kind of common or neutral state exhibited by the constitution of both sexes before the specific sexual characters of each are developed at the time of puberty.

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[1] In the well-known case of Gottlieb Göttlich, Mulder used a small *vaginal speculum* to see these orifices, and obtained seminal fluid, as shown by the microscope, from the orifices of the vasa deferentia.

Numerous curious examples of mistakes having been committed with regard to the sex of males affected with the above species of malformation have now been put on record, from the time at which Iphis, the daughter of Ligdus, King of Crete, was conceived to be changed into a man by the miraculous interference of Isis, down to the present day. Pliny[1] has noticed several cases; and in the treatise of Duval on hermaphrodites, a number of additional instances are collected from Livy, Trallian, and others, some of them no doubt invested (as most of the details regarding hermaphrodites in the older authors are), with much misrepresentation and fable, but others bearing every mark of accuracy and authenticity. In more modern times the sexes of individuals have often been mistaken in consequence of this variety of malformation. Jean Chroker[2] relates, in apparently the most authentic manner, the case of Magdelain Mugnoz, a nun of the order of St. Dominique in the town of Ubeda, who was changed, as he supposes, into a male, seven years after having taken the vows. He was expelled the convent, assumed the male dress, and took the name of François. The sequel of the story, as told by Chroker, would seem to show that his sexual desires became extremely strong, and he is said to have been ultimately condemned, whether justly or not, under an accusation of rape.[3]

Portal[4] quotes from Tigeon the story of a person who was brought up as a female, and afterwards was considered to be suddenly changed by a surprising metamorphosis into a male; and in citing this case, Dr. Hodgkin[5] of London mentions, on the authority of a friend, a recent instance of an equally sudden development of the male sex in a previously reputed female. Similar instances in which the proper sex of malformed males was unexpectedly discovered under the excitement of sexual passion at the period of puberty, are mentioned by Parè, Tulpius, and others.

Schweikard[6] has recorded an instance of a person baptized and brought up as a female, and whose true sex was only at last disclosed by his requesting, at the age of forty-nine, permission to marry a young woman then pregnant by him. On examination, it was discovered that the penis was slender and scarcely two inches long; the right testicle only had descended into the scrotum, and the urethra opened at the root of the penis, but its orifice was placed in such a manner that during micturition the urine was thrown along the groove or channel on the under surface of the penis, so as to appear to issue from its anterior extremity. The two halves of the scrotum were so far united that they left only a small oval opening between the anterior part of the raphé and the roots of the corpora cavernosa. In this opening, the orifice of the urethra was situated.

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[1] Loc. cit. Lib. vii. chap. iv.

[2] Fax. Histor. cent. i. and Arnaud, Dissertation sur les Hermaphrodites, p. 200.

[3] After this essay was first published in 1839, I received, through Dr. Cowan, a long account and drawings of a similar case, where a child taken into a convent in Malta as a female, turned out at puberty to be an amorous hypospadiac male; and subsequently became a sailor instead of a nun. I have in practice been consulted in three cases where hypospadiac male children have been baptized as girls.

[4] Hist. de l'Anat. tom. ii. p. 52. [5] Catalogue of Guy's Hospital Museum, part ii. sect. 11.

[6] Hufeland Journal der Prak. Heilkunde, Bd. xvii. No. 18.

Dr. Baillie[1] has mentioned a case which appears to belong in all probability to the present division. The subject of it was twenty-four years of age. She had always passed in society as a woman, and came for consultation to the Nottingham Hospital on account of her menses never having appeared; a circumstance, however, that had in no way affected her health. The spurious vagina consisted of a cul-de-sac, two inches in depth. The penis was of the size of the female clitoris, but there were no nymphæ. The labia were more pendulous than usual, and each of them contained a body resembling a testicle of a moderate size with its cord. The look of the individual was remarkably masculine, with plain features, but no beard. The mammæ resembled those of a woman. The person had no desire or partiality for either sex.

Adelaide Preville, who had been married as a female, died in the Hôtel Dieu of Paris. In examining the body of this individual after death, Giraud[2] found that, except a perineal cleft or false vagina, consisting of a cul-de-sac placed between the bladder and the rectum, nothing else resembling the female sexual apparatus could be detected, while all the organs belonging to the male sex were present.

Otto[3] has described and represented (Fig. 16) a case of the present species of hermaphroditism, in an individual whose history is remarkable. The person had lived ten years in the state of wedlock with three different men; but at the age of thirty-five, an action of divorce was brought against her by her third husband, accusing her of being affected with some disease of the sexual parts that rendered the connubial act on his part extremely difficult and painful. After some difference of opinion between the two medical men to whose professional examination the wife was submitted, it was at last considered that she was in reality a male; and the case finally came under the investigation of the members of the Royal Medical

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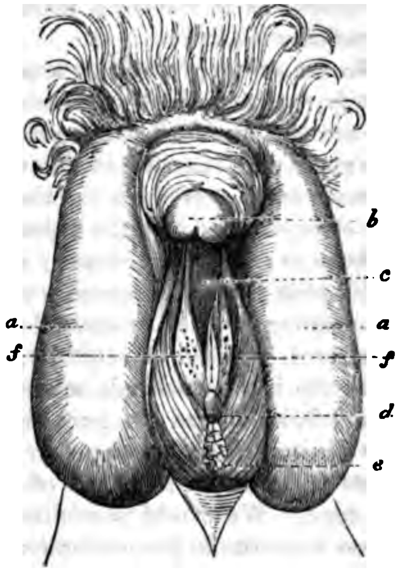
[1] Morbid Anatomy, 2d edit. p. 410.

[2] Recueil Period. de la Soc. de Méd. tom. ii. p. 315, or Moureau's Hist. Nat. de la Femme, tom. i. p. 243 (with a figure of the parts).

[3] Neue Seltene Beobachtungen zur Anatomie, &c., p. 123.

College of Silesia, who confirmed this opinion. The imperforated penis, *b*, was one inch and a half in length; the perineal fissure, *e*, forming the false vagina, was at the posterior part of its orifice, bounded by a distinct frænulum, but was of a size sufficient to receive the glans of the husband for an inch and a half in depth. This cavity, as well as the internal surfaces of the two lobes, *a a*, of the divided scrotum, were lined with a vascular mucous membrane. At the bottom of it, the round orifice of the urethra, *d*, was seen to open; and at the same point a hard mass could be felt, probably consisting of the prostate gland; and more upwards and outwards, nearly in the natural situation of the bulb, was seen the split urethra, *c*, with a row of three considerably sized openings, *f f*, which under pressure and irritation of the genital parts, gave out several drops of a transparent mucous fluid. Otto considers these openings as the extremities of the ducts of the prostate and Cowper's glands, and of the seminal canals. The right half of the scrotum contained a small testicle, about the size of that of a boy ten years of age; the left testicle lay likewise external to the abdominal ring, and was still softer and smaller than the right. Both were furnished with spermatic cords. The general configuration of the individual was strong, muscular, and meagre; the beard was thin and soft, and the face, mammæ, thorax, pelvis, and extremities, were evidently masculine.

Fig. 16.



Hypospadiac Male.

Along with the preceding instances we are inclined to classify the case of Maria Nonzia, as detailed by Julien and Soules.[1] This individual was born in Corsica, in 1695, was twice married as a female, and at last divorced in 1739 by her second husband, after having lived sixteen years in wedlock. The penis was two inches in length, but imperforate, and the meatus urinarius was placed at its root. Two bodies, like ordinary-sized testicles, and furnished with spermatic cords, were felt in the divided scrotum; and there was a narrow false vaginal or perineal canal, one inch and three lines in depth, and crossed at its upper extremity by two small traversing membranous bridles. The character and appearance of the person were masculine; the visage was bearded; the mammæ were as fully developed as in the adult woman; but the nipples were each surrounded with hair.

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[1] Observ. sur l'Hist. Nat. sur la Physique et sur la Peinture, tom. i. p. 18, with a plate.

So far as the preceding details go, they seem amply sufficient to justify us in considering Maria Nonzia as a malformed male; and we are still inclined to take this view of the case, notwithstanding the statement inserted in the report of Julien and Soules, that the menses were present as in other women. For, not to insist upon the circumstance that the reporters do not show that they made any minute or satisfactory inquiry into this alleged fact, and not improbably took it upon the mere word of the subject of the case, who was necessarily greatly interested in maintaining the reputed female character, it would be requisite, in any such paradoxical instance, to ascertain if the discharge actually agreed in character with the menstrual fluid, or was not pure blood, the result of a hemorrhage from the genito-urinary passages, or from the rectum, where, as in other parts of the body, this form of disease frequently assumes a periodical type. We would be inclined to apply even still more strongly these remarks to the celebrated case of Hannah Wild, detailed by Dr. Sampson.[1] This person had evidently the male genital organs malformed in the manner mentioned with regard to the other cases included under the present section, and possessed all the secondary sexual peculiarities of the male; so that we can only receive with great doubt and distrust the alleged existence of the menstrual discharge, and the more so, as this is evidently stated on the report of the subject of the case alone, who, deriving a precarious subsistence from the exhibition of his malformations, had a deep interest in amplifying every circumstance that could enhance the public curiosity with respect to the reality of his hermaphroditic character.

At the same time, however, it must be remarked, that in some instances of spurious hermaphroditism, it is found extremely difficult or even impossible during life to determine with precision the true or predominant sex of the malformed individual; and in regard to several well-known cases on record, we find on this point the most discrepant opinions offered by different authors. Thus, while Morand,[2] Arnaud,[3] and Delius[4] described Michel-Anne Drouart as a male; Guyot,[5] Ferrein,[6] and Caldani[7] maintained that this person was a female; and Mertrud[8] regarded the individual as an example of a real hermaphrodite.

A useful lesson of caution to us against forming too decided and dogmatic an opinion in cases in which the sexual conformation appears in any marked degree doubtful, has lately been offered in the instance of Maria-Dorothée Duriée, or as this individual was named in the latter years of his life, Charles Durge. While Metzger[1] considered this person as a specimen of that kind of equivocal sexual formation to which the designation of hermaphroditism is truly applicable, Hufeland,[2] Mursinna,[3] Gall, Brookes,[4] and others,[5] declared the sex of Duriée to be in reality female; and Stark,[6] Mertens,[7] and the Members of the Faculty of Medicine at Paris,[8] were equally positive in regarding the individual as merely a malformed male. The dissection of the body of Duriée by Professor Mayer has, as we shall afterwards state more in detail, shown the sexual conformation of this individual to consist of a mixture of both the male and female organs.

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[1] Ephem. Nat. Curios. Dec. i. an. iii. p. 323.

[2] Mém. de l'Acad. des Sc. 1750, p. 165.

[3] Dissert. sur les Hermaphr. p. 298.

[4] Frank. Sammlung, Th. viii. s. 398.

[5] Mém. de l'Acad. des Sc. 1756, p. 71.

[6] Ibid. 1767, p. 205.

[7] Mem. della Societa Italiana, tom. vii. p. 130.

[8] Arnaud. loc. cit. p. 298.

In attempting to determine the true sex in such doubtful instances of sexual formation as those we have now been considering, we are inclined to attribute very little weight to the nature of the sexual desires of the malformed individual, as we have already found Adelaide Preville, the dissection of whose body showed him to be in reality a man, living for some years before death in the capacity of a wife; and the same remark might be further illustrated by a reference to Otto's and other cases.

A species of spurious hermaphroditism similar in character to that which we have just described in man, is occasionally met with in the males of our domestic quadrupeds, and has been amply illustrated, as it occurs in these animals, by Professor Gurlt in his work on Veterinary Medicine. In instances of this malformation among the animals to which we refer, the hypospadiac male penis has usually been found of a tortuous and winding form and of small size. In the cases in which the fissure of the parts extends through the scrotum, a false vagina is seldom formed, as in man, for the scrotum in most quadrupeds lies too remote from the perineum, and consequently from the normal situation of the vagina, for this purpose; but in some examples this division appears to be carried upwards into the perineum itself, leaving a vaginal-like opening in which the urethra terminates. The testicles, as in man, are sometimes retained within the abdomen, and in other instances descend into the scrotum. They are frequently small in size. The mamma or udder seems to be often well developed.

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[1] Gericht.-medic. Abhandlungen, Bd. i. s. 177.

[2] Journ. der Praktischen Heilkunde, Bd. xii. s. 170.

[3] Journ. für die Chirurgie, Arzneikunde, &c. Bd. i. s. 555.

[4] Medical Gazette for October, 1836.

[5] Von dem Neuangekommen Hermaphrod. Berl. 1801.

[6] Neuen Archiv. für die Geburtshülfe, Bd. ii. s. 538.

[7] Beschreibung der männlichen Geschlechtstheile von M. D. Durrier, Leipzig, 1802, with two plates.

[8] Med. Gaz. for October, 1836.

This variety of hermaphroditic malformation has been met with in the horse by Penchenati;[1] in the he-goat by Haller;[2] and in the ram by the same author,[3] and by Wagner,[4] Wepfer,[5] Stark,[6] Gurlt,[7] Kauw Boerhaave,[8] and A. Cooper.[9] We have seen an excellent specimen of this malformation in the last-mentioned animal in the museum of Dr. Handyside of Edinburgh. In this instance the internal male organs are all perfect; the large testicles are situated in the halves of the split scrotum; the penis is small and imperforate, and a furrow running along its inferior surface, is continued backwards and upwards along the perineum to within a short distance from the anus, where it leads into a canal, into which the urinary bladder and seminal ducts open. This canal is evidently formed of the dilated pelvic portion of the male urethra; its orifice is comparatively contracted but corresponds in situation with the vulva of the female. We have seen a second similar case in a ram in the possession of Professor Dick of the Veterinary School of Edinburgh.

There is another variety of malformation of the male parts occasionally found in quadrupeds, which is allied in its nature to the preceding. In this second species, all the external male sexual organs are small; the short penis lies, when not in a state of erection, upon the posterior surface of the enlarged udder, and the imperfectly developed testicles are generally retained within the abdomen; or, if they have passed out of that cavity, they are found situated in the substance of the udder. The vasa deferentia, prostate, and Cowper's glands, are usually of their normal size and appearance. This imperfect hermaphroditic formation appears to be not rare among horses, several instances of it in this animal having been now described by Arnaud,[10] Gohier,[11] Volmar,[12] Pallas,[13] Virey,[14] and Gurlt.[15] Anselmo[16] and Lecoq[17] have met with this variety of malformation in the bull; and Sandford[18] has described an instance in the calf, which

[1] Mém. de l'Acad. de Turin. tom. v. p. 18.

[2] Comment. Soc. Reg. Sc. Gotting. tom. i. p. 2, tab. i.

[3] Ibid. p. 5, tab. ii.

[4] Ephem. Nat. Curios. Cent. i. ii. p. 235.

[5] Miscell. Nat. Curios. Dec. i. ann. iii., 1672, p. 255.

[6] Ibid. Dec. iii. ann. v. vi. p. 669.

[7] Lehrbuch, p. 193.

[8] Nov. Comment. Acad. Petropolit. tom. i. 1750, p. 315, tab. xi.

[9] Catalogue of Guy's Hospital Museum, No. 2546.

[10] Sur les Hermaphrodites, p. 282.

[11] Mém. et Observ. sur la Chir. et la Méd. Vet. tom. i. p. 18.

[12] Archiv für Thierheilkunde, Bd. iii. s. 292.

[13] Beschaft. der Gesellschaft naturforsch. Freunde zu Berlin, Bd. iii. s. 296.

[14] Journal Compl. des Sc. Méd. tom. xv. p. 140.

[15] Lehrbuch der Path Anat. Bd. ii. p. 189; and tab. viii. fig. 6.

[16] Mém. de l'Acad. des Sc. de Turin, tom. ix. p. 103, figs. 1-3.

[17] Journ. Prat. de Méd. Vet. 1827, p. 102.

[18] Med. and Phys. Journal, vol. ii. p. 305, with two drawings.

seems referable to the same head. Gurlt[1] also notices the preparation of an analogous case in the calf, as preserved in the museum at Berlin.

TRUE HERMAPHRODITISM.

True hermaphroditism exists as the normal type of sexual conformation in several classes of the vegetable and animal kingdom. Almost all phanerogamic plants, with the exception of those included under the class Diœcia, are furnished with both male and female reproductive organs, placed either upon the same flower, or, as in the Linnæan class Monœcia, upon different flowers in the same individual. In the class Polygamia various exceptional genera are included, which present indiscriminately upon the same individual, or upon different individuals of the same species, male, female, and hermaphrodite flowers, and which thus form a kind of connecting link between the general hermaphroditic form of phanerogamic vegetables, and the unisexual type of the monœcious flowers, and the diœcious plants.

From anormalities in development these normal conditions of the sexual type in the different members of the vegetable kingdom are occasionally observed to be changed. Thus, among the Diœcia, individual plants are sometimes, in consequence of a true malformation, observed to assume an hermaphroditic type of structure; or, on the other hand, in hermaphroditic plants, more or fewer flowers are occasionally found unisexual, in consequence of the arrested development of one order of their sexual organs; and again, though still more rarely, from an excess of evolution, a double set of male parts, or a double set of stamens, is seen developed on some of the individual flowers.

In the animal kingdom we find instances of a perfect hermaphroditic structure as the normal form of the sexual type in the Trematodes and Cestoides among the Entozoa, in the abranchial Annelida, in the Planaria, and in many of the Mollusca, particularly in the Pteropoda, and in several families among the Gasteropoda. In some of these animals that are thus naturally hermaphroditic, the fecundation of the female organs of the bisexual individual is accomplished by its own male organs; but in others, although the anatomical structure is strictly hermaphroditic, yet the union of two, or, as sometimes happens, of more individuals, is necessary to complete the sexual act; and during it the female organs of each are respectively impregnated by the male organs of the other.

In the Nematodes and Acanthocephali among the Entozoa, and in the Cephalopoda and Pectinibranchiate Gasteropoda among the Mollusca, as well as in all symmetrically formed animals, or, in other words, in those whose bodies are composed of a union of two similar halves, as in Insects, and the Arachnida, Crustacea, and Vertebrata, the male and female organs of reproduction are placed each upon a different individual of the species, constituting the basis of distinction between the two sexes. In such animals, a mixture of more or fewer of the reproductive organs of the two sexes upon the same individual appears occasionally as a result of abnormal formation; but the male and female organs that coexist in these cases have never yet been found so anatomically perfect as to enable the malformed being to exercise the proper physiological function of either or of both of the two sexes. This form of true hermaphroditism or abnormal mixture upon the same individual of the organs of the true sexes in the higher animals, has been termed "unnatural" or "monstrous," in opposition to the natural hermaphroditism which exists as the normal type of sexual structure in some of the lower orders of animals, and in phanerogamic plants. The malformation itself is observed to differ greatly, both in nature and degree, in different cases, varying from the presence or superaddition of a single organ only of the opposite or non-predominant sex, up to the development and coexistence of almost all the several parts of the two sexes upon the same individual. In describing the malformation, we shall classify its various and diversified forms under the three general orders pointed out in our table (p. 205), including, 1st, *lateral;* 2dly, *transverse;* and 3dly, *double* or *vertical hermaphroditism.*

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[1] Loc. cit. p. 191.

A. TRUE LATERAL HERMAPHRODITISM.

According to the opinion of many physiologists of the present day, the two lateral symmetrical halves of the body, and even the two halves of all its single mesial organs, are originally developed in a great degree independently of one another. Granting this point in the doctrine of eccentric development, we can easily conceive how, in the same embryo, an ovary might be formed on one Wolffian body, and a testicle on the other; or, in other words, how female organs might be developed on one side, and male organs on the other. It is the existence of such an unsymmetrical type of sexual structure upon the two opposite sides of the body of the same individual, that constitutes the distinctive characteristic of lateral hermaphroditism.

Instances of this species of true hermaphroditic malformation have been observed in many different classes of animals, as well as in the human subject.

ual examples are sometimes observed among insects, parti-
nong the Lepidoptera, in which all the different parts of
des or lateral halves of the body are formed after opposite
es. We shall afterwards have occasion to notice different
of this form of lateral hermaphroditism as seen in the
nformation of the body, but may here state that in two or
nces such malformed insects have been carefully dissected
l to present, in the anatomical structure of their sexual
mixture of the organs of the male and female.

litæa didymus described by Klug,[1] the general external cha-
re those of the male, but the left eye, palpus, and antenna,
ft sexual fang, were smaller than in individuals belonging
; and the left antenna was annulated with white and yellow
x, while the right was of one color. On dissection, the
ale sexual parts were present, and they had appended to
ee female ovary situated upon the left, and united to no
n.

strophaga quercifolia dissected by Schultz, and described
phi,[2] the left side appeared externally male, and the right
ith a distinct line of separation throughout the whole body.
ection, Schultz discovered an ovarium upon the right side,
estes upon the left. The oviduct of the ovary joined the
he vasa deferentia about two inches before its termination;
permatheca was connected with the common evacuating
e two testicles on the left side were placed one behind the
l connected by a thin vessel. The spermatic duct belong-
of the testicles immediately received, as in the Lepidop-
spiral vessel; further beyond, and on the opposite side, a
ssel, which appeared to consist of the rudimental sperma-
f the other testicle, opened into it. The oviduct of the
ed the canal of the vasa deferentia about two inches before
ation in the penis, and the female spermatheca was con-
th the common distended evacuating duct.[3]

-marked example of lateral hermaphroditism among the
has been recorded by Dr. Nicholls.[4] In a lobster (*Astacus*
he found on the right side of the body a female sexual
in its normal situation at the root of the third leg, and
l with a regularly formed oviduct, full of ova. On the
f the animal, there was a male sexual aperture, placed, as

Notizen, Bd. x. p. 183. [2] Abhandlung. der Konig. Akad. zu Berlin, 1825, s. 55.
drawings of the body and genital organs of an hermaphrodite *Sphinx populi* in
ctographie du Gouvernement de Moscou, Moscow, 1830.
ns. for 1730, No. 413, vol. xxxvi. p. 290, with drawings of the animal, and of
ve organs.

usual, at the root of the fifth leg, and connected internally with an equally perfect testicle and spermatic cord. The general external conformation of the animal corresponded with its internal sexual structure, the right lateral half of the body presenting all the secondary characters and peculiarities of the female, and the left all those of the male; so that if split from head to tail, to use Dr. Nicholls' mode of expression, the animal would have been perfectly female on the right side, and perfectly male on the left.

The investigations of Sir E. Home[1] led physiologists some years ago to believe that among Fishes, lateral hermaphroditism constituted the natural type of sexual formation in the genera Myxine and Petromyzon; but the later and more accurate observations of Rathke[2] have shown that these species are strictly bisexual, and that the opposite opinion had arisen from the kidneys of the female having been mistaken for the male testicles. Various instances, however, are on record of fishes, known to be normally bisexual, presenting from abnormal development a lateral hermaphroditic structure, or a roe on one side, and a milt on the other. Such an hermaphroditic malformation has been met with in the genera *Salmo*,[3] *Gadus*,[4] and *Cyprinus*,[5] and in the *Merlangus vulgaris*,[6] *Acipenser huso*,[7] and *Esox lucius*.[8]

Of lateral hermaphroditism in Birds, we have one instance recorded by Bechstein,[9] in a chicken that had a testicle on the right side of the body, and an imperfect reniform ovary on the left. The external appearance of the bird presented a mixture of the characters of the two sexes.

Rudolphi has referred to a second and more ancient example of lateral hermaphroditism in the hen, mentioned by Heide.[10] The case, ed by the author "galli qui putabatur hermaphroditus anatome ," is so imperfectly detailed as not to be entitled to much atten-

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[1] Phil. Trans. for 1823. Art. xii.

[2] Bemerkungen über den Innern Bau der Pricke, s. 119. See also additional observations by the same author in Müller's Archiv für Anatomie, &c., for 1836, Heft. ii. s. 171. The older error of Cavolini, who supposed that he had detected two ovaries and two testicles in the *Perca marina* and *Labrus channa* (Sulla Generazione dei Peschi et die Granchi, Nap. 1787), had been previously shown by Rudolphi to depend upon his having mistaken undeveloped portions of the ovaries for testicles. Schweigger's Skeletlose Thiere, s. 204; and Abhandlungen. König. Akad. der Wissenschaft zu Berlin, 1825, p. 48.

[3] Commercium Litter. Norim. 1734, Hebd. 39.

[4] Pipping, Vetensk. Akad. nya Handl. 1800, Bd. xxi. s. 33, tab. i. fig. 1; Leuwenhoeck, Experim. et Contempl. p. 150; Eph. Nat. Cur. Dec. i. ann. i. obs. 125; Du Hamel, Traité des Poissons, Part ii. p. 130.

[5] Alischer, Breslau Sammlung. 1720, p. 645; Morand, Mém. de l'Acad. des Sc. 1737, p. 72; Schwalbe, Commer. Lit. Norimb. 1734, p. 305.

[6] Marchant, Mém. de l'Acad. des Sc. 1737, p. 12; Baster, Opusc. Subcesiva. tom. i. p. 138.

[7] Pallas, Reise durch Russe, &c., Theil. ii. s. 341.

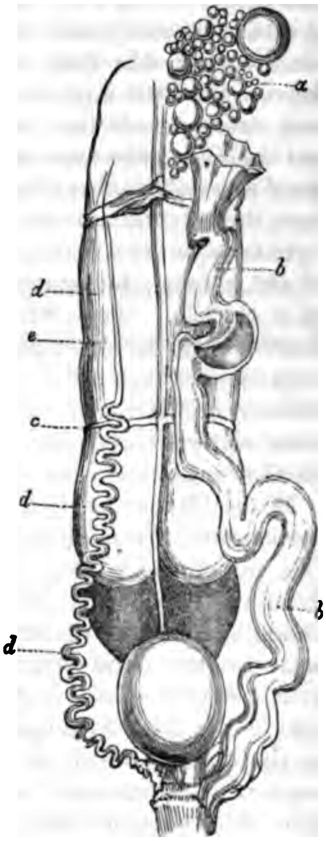
[8] Reaumur, Mém. de l'Acad. 1737, p. 51; Starke, Eph. Nat. Cur. Dec. iii. ann. vii. and viii. obs. 109.

[9] Naturgeschichte der Vögel, &c., 1807, Bd. ii. s 1219.

[10] Anatome Mytuili: subjecta est Centuria Obser. Amsterdam, 1684, p. 193, obs. 95.

ave been fortunate enough to meet with two domestic fowls presented in their sexual form and organization examples of ıl hermaphroditism. In the first of these cases (Fig. 17), the .e sexual organs were placed on the left side of the body, and rary, *a*, and oviduct, *b*, were in all respects apparently naturally ɜd. On the right side, a male vas deferens, *d*, of about half ormal length, ran up the cloaca posite the origin of the iliac ves- :, and during this part of its course bent into those short transverse g folds which characterize the ture of this part in the common [1] When it reached the middle of the kidney, *d d*, it lost this :ular form, became membranous, d after proceeding upwards for : an inch, in the common course ɜ canal, at last disappeared. The ɔluted or contorted portion ran a space of about two and a half s, and if unrolled, would have .ded three or four times that h. Its canal was about the usual of the same part in the perfect and perhaps at some parts even dilated. Its cavity was filled a whitish, seminal-looking, albu- us fluid, which at first prevented a urial injection from readily pass- :hrough it. There was not any ·ent vestige of a testicle. The that was the subject of this mal- ıtion possessed in an imperfect ·e the plumage, comb, spurs, and ·al appearance of the cock, and young was considered to be a until the time it commenced to lay eggs, which it did very con- y, except during the moulting season, up to the time of its Its eggs were remarked to be very large. They had re- lly been tried to be hatched, but always without success. The

Fig. 17.



Lateral Hermaphroditism in Domestic Fowl.

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[1] See Cycl. of Anat. and Phys., vol. i. p. 354. Article AVES.

bird itself was never known to incubate. It was peculiar in its habits in so far that in the barn-yard it did not associate with the other poultry, and at night roosted separately from them. It crowed regularly, especially in the morning, and often attempted copulation with the hens.

In the second case, the ovaries and oviduct on the left side of the body were, as in the former example, natural in themselves; but in the mesometry of the oviduct, a tube of the size of the male vas deferens was found. This tube, like the normal vas deferens, was thrown into the distinctive angular folds. It ran for about an inch and a half through the upper portion of the mesometry, was blind at either extremity, and admitted of being injected with quicksilver. On the right side there was also a male vas deferens, marked with the characteristic angular folds. The contorted portion of this canal only stretched—in this instance—to about an inch above the cloaca; but the folds were even stronger than in the first case, and the tube itself was rather more dilated. Above or anterior to this convoluted part, the tube became straight and membranous, and ran up in this form for about two inches in its usual track over the abdominal surface of the kidney; but there was not at its upper extremity any trace of a testicle. This bird presented during life, in a very slight degree only, the appearance of a cock, its comb and spurs being even less developed than in the previous case. It showed the same solitary habits in the poultry-yard. It laid eggs regularly. On three different occasions I had a number of them submitted to incubation, but in none of them was a chick produced.

In the Quadruped, Schlump[1] has mentioned an instance of lateral hermaphroditic malformation. In a young calf he found on the left side, under the kidney, a small testicle, having attached to it a vas deferens, which was connected with the peritoneum towards the abdominal ring of the same side, and there became lost in the cellular texture of the part. An ovary and Fallopian tube, with a uterus consisting of a single horn only, were connected to the right side of the loins by a ligament. The neck of the uterus lost itself in the cellular substance beneath the rectum, and there was no vagina. The external organs were male, but imperfectly formed. The udder occupied the place of the scrotum.[2]

In the human subject, several different instances of sexual malformation have now been met with, referable to the head of lateral hermaphroditism. In these cases, along with a testicle on one side, and an ovary on the other, there has generally coexisted a more or less perfectly formed uterus. The external parts have differed in their sexual characters, in some instances being female, in others male, and in others again of a neutral or indeterminate type.

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[1] Archiv. für die Thierheilkunde, Bd. ii. Hft. ii. s. 204.

[2] In the Gazette Medicale de Paris for 1844, p. 530, Bertrand describes an hermaphroditic sheep, having a testis and epididymis in the left side of the scrotum, and with a vas deferens leading from it to a fully developed ovary on the right. There was a uterus, and blind Fallopian tubes, and an imperforate vagina; and on the right or female side, a rudiment also, apparently of a vas deferens.

In man, and in the higher quadrupeds, we have not unfrequently exhibited to us a slight tendency to this unsymmetrical type of sexual structure, constituting true lateral hermaphroditism, in the testicle of one side only descending, whilst the other, in consequence of imperfect development, remains within the inguinal ring. In the single unsymmetrical ovary of most female birds and some fishes,[1] we see a still nearer approach to the state; and it is worthy of remark, that among birds at least, the single ovary is always placed upon the left side. In lateral hermaphrodites in the human subject, the left side also appears to be that on which we most frequently meet with the female type of the sexual organs. We shall divide the following cases according to the particular sides which were respectively male and female in them.

1. OVARY ON LEFT SIDE, AND TESTICLE ON THE RIGHT.

a. M. Sue met, in 1746, with an instance of this subdivision of lateral hermaphroditism in the human subject, in a young person, thirteen or fourteen years of age, whose case was the subject of a Thesis sustained by M. Morand.[2] Of the *internal* genital organs, there existed on the *left* side a very distinct ovary, a round ligament which ran outwards to the groin of the same side, and a well-formed Fallopian tube with its usual fimbriated extremity. The other extremity of the Fallopian tube terminated in the fundus of the uterus, which occupied its usual situation between the bladder and rectum. On the *right* side, again, there was a slender elongated testicle, which had moved forwards to the corresponding inguinal canal, but had not proceeded so far as to pass out of the abdominal cavity. On the superior part of the testicle was a body resembling the epididymis, and the testicle itself sent off two tubes, which afterwards united into one immediately before their insertion into the uterus. The *external* genital organs were those of a hypospadiac male, and during life the person had been always looked upon as belonging to the male sex. The perineal canal or vagina terminated between the scrotum and root of the imperforate penis, in a very small opening, which was common to it and to the meatus urinarius.

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[1] In the early embryo of birds, the ovaries are originally double, as pointed out by Emmert (see Reil's Archiv for 1811); and as was previously known to Wolff and Hochstetter (Anat. Phil. tom. i. p. 349).

[2] De Hermaphroditis, Paris, 1749. This, according to Arnaud (loc. cit. p. 323), is the same case of lateral hermaphroditism with that described by Lecat. If so, the latter author (probably from drawing his description from memory, and not, as Morand seems to have done, from the parts placed before him), has stated that, along with the testicle and vas deferens on the one side, there existed a vesicula seminalis, and that both sides were provided with round ligaments, the one on the male side forming probably one of the two tubes described by Morand as arising from the testicle. Meckel (Reil's Archiv, Bd. xi. s. 322), considers Morand's and Lecat's as two different cases, and points out that what is described as the male side in the one, was the female in the other, and *vice versa*. It is perhaps not unworthy of remark, that, in the colored plate accompanying the translation of Morand's case by Gautier, the male and female sides have been reversed from an error in the engraving; and this circumstance may have contributed to mislead Lecat in his description, provided he happened to look at this notice of the case.

b. In 1754,[1] a young person of about eighteen years of age died in the Hôtel Dieu of Paris; and in dissecting his body, the anatomist Varole found the reproductive organs malformed in the following manner:—On the *right* side the scrotum contained a testicle, and the vas deferens arising from it opened, not as usual into the neck, but into the middle of the external border, of the corresponding vesicula seminalis. On the *left* side the scrotum was empty; and internally on this side there were found an ovary, a Fallopian tube with its fimbriated extremity, a small oval uterus without a neck and somewhat flattened, and a broad and a round ligament, the last of which ran outwards, and was lost in the cellular tissue of the left half of the scrotum. The vesicula seminalis on the right, and the imperfect uterus on the left side, communicated by a canal of an inch and a half in length. The external organs were male; but the penis was very small, had no corpus spongiosum, and was imperforate for half an inch at its anterior extremity. The mammæ were as large as in women of the same age. The individual had been regarded during life as a male.

c. In 1825, the late Professor Rudolphi[2] detailed to the Academy of Sciences at Berlin the case of an infant who was reported to have died seven days after birth, and whose sexual organs exhibited the following interesting instance of lateral hermaphroditic conformation. On the left side were discovered an ovary (Fig. 19, *a*), without a distinct broad ligament, and a Fallopian tube, *b*, which communicated with the superior and left portion of a uterus, *c*. The left side of the scrotum (Fig. 18, *a*) was empty; the right, *b*, contained a testicle (Fig. 19, *d*) furnished with an epididymis, *e*, and tortuous vas deferens, *f*. Below the uterus there was a hard flattened ovoid body (Fig. 19, *g*, and Fig. 20, *b*), which, when divided, was found to consist of a cavity with thick parietes, and was considered by Rudolphi as the prostate gland in a rudimentary state. The mouth of the uterus (Fig. 20, *a*) terminated below in the parietes of this ovoid body, and on the right the vas deferens, *d*, penetrated into its substance, but without opening into its cavity. At the inferior part of the uterus there was a true vagina, *c*, which terminated in a cul-de-sac. The anus, rectum, and other organs were natural. The external sexual parts were male, but the penis was divided inferiorly (Fig. 18, *c*). The testicle and ovary were supplied with the usual spermatic arteries (Fig. 19, *h h*).

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[1] Mém. de la Soc. Méd. de Paris, tom. iv. p. 342.

[2] Abhandlung. König. Akad. der Wissenschaft. zu Berlin, 1825, s. 60.

Fig. 18.

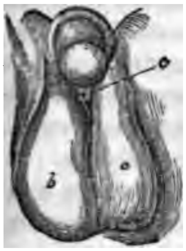


Fig. 19.

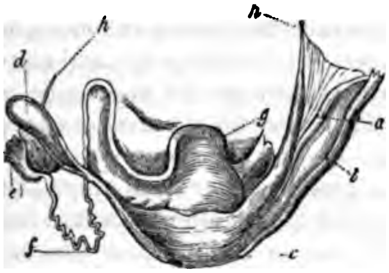
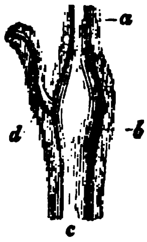


Fig. 20.



d. In 1851, a well-marked case of lateral hermaphroditism was found in Dublin, and I have seen and examined the malformed parts in the College of Surgeons' Museum in that city. The subject of it, a supposed male convict, aged 26, was dissected by Dr. Banon, who has published an accurate and excellent account of the case, with illustrative drawings.[1] Externally the penis was of the usual size and structure of that organ in the male adult; but it was imperforate—the urethra opening inferiorly at its root, between the bifurcations of a substance resembling the corpus spongiosum. The divided scrotum or labia gave lower down the appearance of the female vulva, with nymphæ and a small vaginal orifice contracted by a crescentic hymen. Internally, this vaginal orifice and a very narrow vaginal canal led upwards to a small well-formed uterus, which bore the exact relations to the bladder, rectum, and peritoneum, that this organ has in the normal female, and was provided laterally, like it, with broad ligaments. The uterus opened into the vaginal cavity below, and from its left corner superiorly there was given off a very tortuous and twisted, but perfectly permeable Fallopian tube, which terminated, as usual, at its further extremity, in a corpus fimbriatum. On lifting up this fimbriated extremity, it was found to rest on an ovary bound to the peritoneum, and thrown somewhat across to the right side in the recto-vaginal space. There

Fig. 18. External organs. Fig. 19. Uterus, turned downwards and forwards to show its Posterior surface and connections, &c. Fig. 20. Os uteri, vagina, prostate, and vas deferens.

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[1] Dublin Journal of Medical Science for 1852, p. 66.

was no trace whatever of a right Fallopian tube or right ovary; but in their stead there was a male testicle, lying also in the recto-vaginal space, and provided with a long epididymis which ended in a permeable vas deferens that took an unusual course, for it first passed forwards and outwards in its normal course, nearly in the direction of the internal abdominal ring, and then suddenly turned backwards and downwards towards the uterus, ran along between the layers of its right broad ligament, became more and more slender, and perforated at last obliquely through the right side of the organ into the uterine cavity. There was no appearance of a vesicula seminalis or prostate gland, and no trace of a second testis or vas deferens. The testicle which was present seemed as perfect as in the normal male; it was provided with a tunica albuginea, and beneath this was found the delicate tunica vascula, and internally the tubular structure peculiar to this organ. A small quantity of fluid taken from the commencement of the vas deferens had the peculiar odor and consistence of the human semen, but on microscopic examination no spermatozoa, only numerous cells containing granules, were seen in it. The shape and connections of the ovary with the fimbriated extremity of the Fallopian tube, were such as are seen in the normal female; and on division it presented the usual stromal tissue; but no appearance of Graafian vesicles could be detected in it. The subject of the preceding malformation was baptized as a girl, but afterwards, in consequence of the increasing size of the organ representing the penis, he was brought up as a male, and died of phthisis, when under sentence of transportation for sheep-stealing. His voice was masculine; he excelled in several of the manly exercises; his sexual desires appear to have been slight, but those of a male; and his general configuration and appearance were those of a man, with the exception that there were but slight traces of beard. The pelvis was fan-like, and somewhat feminine in form.

e. Under the present section of lateral hermaphroditism, we may also, according to Mayer's report, include the celebrated case of Maria Duriée, or Charles Durge.[1] This person was baptized and brought up as a female, but at forty years of age was persuaded to change his name and dress to those of a man. We have already (p. 225) alluded to the great diversity of opinion which was entertained by the medical men of Europe in regard to the true sex of this individual. Even the different parts of his body were at one time referred to the male type, and at another time, and by other persons, to the female. The pelvis was the only part that was generally considered as decidedly female, yet the inspection of the body,

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[1] Gazette Méd. de Paris, 1836, No. 39; Lancet, vol. i. for 1836–37, p. 140; or London Medical Gazette for October 29, 1836.

eath by Professor Mayer showed that even in this respect all
n error.

he female sexual organs there existed a uterus, vagina, two
ian tubes, and an ovary; and of the male, a testicle, prostate
and penis. The uterus was placed in its normal situation,
n the urinary bladder and rectum, but with its fundus directed
e degree to the left. The organ was extremely narrow, and
d a half inches in length. The cavity of its cervix presented
inner surface some slight folds, but would scarcely admit
; the cavity of its fundus was nearly half an inch across. The
anals of two Fallopian tubes opened into the fundus uteri.
abdominal extremities were shut, but the corpora fimbriata
present. Near the extremity of the right Fallopian tube,
was four inches and four lines in length, a small, flattened,
l-shaped body was placed, which on examination proved to
inctly a testicle. It was completely enveloped in peritoneum,
eived a cord composed of muscular fibres and of a spermatic
nd artery. Its internal structure was yellow and filamen-
ke that of the testicle, and its seminiferous tubes could be
separated. The left Fallopian tube was an inch shorter than
ht; and a little outside and behind its abdominal extremity
r small flattened body was found enclosed in the peritoneum.
mbled an ovary rather than a testicle. Its tissue was com-
of small granules conglomerated together. The penis was two
and nine lines in length, and was for the greater part con-
beneath the mons veneris. During life it was capable of
n, and was then elongated to more than three inches. The
e covered only half of the glans. There was not any corpus
osum. A fossa or groove, representing a urethral canal
l inferiorly, ran along the under surface of the penis. The
lds of skin forming the sides of the groove separated from
ther posteriorly, and might be compared to nymphæ. To-
the root of the penis, by uniting inferiorly with a puckering
skin of the labia majora or divided halves of the scrotum, they
a circular orifice not larger than a quill, having some bodies,
ed to be vestiges of the carunculæ myrtiformes, at its lower
nd leading to a short vestibule, or common canal, into which
thra, surrounded by a firm but small prostate, entered from
and the vagina, encircled at its entrance by a vascular ring of
e veins, opened from below. The vagina was two inches
ht lines in length, and only ten lines at its greatest breadth.
r surface was somewhat wrinkled anteriorly, but smooth
It terminated above in a kind of spongy isthmus, repre-
the blind orifice of the uterus and from four to six lines in

length. The diameters and form of the pelvis were, on dissection, found to be most evidently masculine.

The general character of Durge was a mixture of the male and female type. When between twenty and thirty, he had been examined by different medical men in Germany, France, and England, and, as we have already mentioned, the most contradictory opinions were offered upon his real sex. The breasts were not much developed, and there was no distinct mammary glandular structure. His stature was small. As he had advanced in age, his voice had become more firm and grave, and a slight trace of beard had appeared; but his head and face presented the aspect of an old woman. His neck was short, and the thyroid cartilage did not project much: his chest was fat and full. During the last few years of his life he was subject to epistaxis and hemorrhoids, but did not present any trace of sanguineous discharge from the genital organs—a phenomenon which was alleged to have manifested itself three times during his twentieth year.

The right hemispheres of the cerebrum and cerebellum, particularly that of the latter, were smaller and less developed than the left, and the left side of the occiput was externally more prominent than the right. He is stated by Professor Mayer to have shown a certain predilection for females, without, however, feeling any sexual desire.

2. TESTICLE ON THE LEFT, AND OVARY ON THE RIGHT SIDE.

An instance of malformation of the reproductive organs minutely described by Maret,[1] and which is in all its more essential anatomical points an example of lateral hermaphroditism, may be included under this head.

a. The subject of the case, Hubert Jean Pierre, died in the hospital at Dijon in 1767, at the age of seventeen. On the left side a perfect testicle was discovered, with its usual spermatic vessels, vas deferens, and vesicula seminalis, all occupying the natural situation in which they are placed in the male adult. The vesicula seminalis contained a fluid of the color and consistence of semen. On the *right* side an oblong cystic tumor was found lying in the iliac fossa, and stretching outwards into the inguinal region. On opening it, a quantity of reddish limpid fluid escaped, and then the solid contents of the tumor were seen to consist of a somewhat flattened body, that gave off from the upper part of its right side a short Fallopian tube; and at the fimbriated extremity of this tube an ovary of the natural size, consistence, and figure, was situated. The roundish shaped body to which the tube was attached was about an inch and a half in its greater, and an inch in its smaller diameter. It contained in its centre a small cavity continuous with that of the tube —a circumstance which, along with the structure of its walls, left little doubt that the body itself was an imperfectly formed uterus. No other opening except that of the tube could be traced into its cavity. Its external surface was attached to the ovary by a kind of ligament. On this same side of the body (the right), there existed also a vesicula seminalis, but smaller and more shrivelled than that on the left. It gave off a vas deferens, which became gradually smaller as it was traced backwards, and at last disappeared altogether, without being connected with any structure resembling a testicle. In regard to the external organs of generation, the penis was four inches long and imperforate, but in all other respects perfectly formed. It possessed a corpus spongiosum, which does not exist in the female clitoris. On raising the penis, it was observed to cover a large fissure, the sides of which resembled the labia of a female. In the left labium or left half of the scrotum, the testicle already alluded to was placed, but there was none in the right. When the labia were separated, two red spongy bodies were seen, resembling the nymphæ in appearance, and seemingly consisting of the sides of the split urethra. Between these bodies and at their upper part, the urethra opened as in the female; while below, there was a very narrow aperture covered by a semilunar membrane, and presenting on one side of its entrance a small excrescence somewhat resembling in figure a caruncula myrtiformis. This orifice led into a membranous canal or cul-de-sac, an inch in depth, and half an inch in diameter. On the lower part of this canal the verumontanum and orifices of the seminal ducts of both sides were discovered.

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[1] Mém. de l'Acad. de Dijon, tom. ii. p. 157.

During life, Pierre had been considered a male, but was not known to have shown any partiality to the female sex. His countenance was more delicate than we ordinarily see in the male sex. There was no beard on the face; the larynx was not enlarged as in man; and the mammæ, each of which was furnished with a very large areola, were of a moderate size and roundish form. The configuration of the lower part of the body was more decidedly masculine, and there was none of that enlargement of the buttocks and projection of the thighs, from the increased width of the pelvis, which is observable in young females.

In this case we have on the *left* side of the body, male sexual organs, consisting of a perfect testicle, vas deferens, and vesicula seminalis. On the *right* side, again, we have a female ovary and Fallopian tube, with a rudimentary uterus, together with an imperfect male vesicula seminalis and vas deferens.

Arnaud mentions a very imperfect form of lateral hermaphroditism as having been recognized by M. Boudou, surgeon to the Hôtel Dieu of Paris, on the person of a monk, who died in that hospital in 1726. The external genital parts were those of a hypospadiac male. In one of the halves of the scrotum a testicle was found; the other was empty. The seminal canals and vesiculæ seminales on the side on which the perfect testicle existed were natural in their course and situation. Those of the opposite side lost themselves between the bladder and rectum in a small body, which in M. Boudou's opinion was a shrunk uterus.[1]

Among the preceding cases of lateral hermaphroditism in the human subject, there are five in which the left side, and one only in which the right was the female. In the last instance, quoted from Boudou, the respective sides on which the male and female organs were placed are not stated by Arnaud.

B. TRUE TRANSVERSE HERMAPHRODITISM.

In the variety of hermaphroditic malformation which we have last considered, we have found upon the same individual the reproductive organs of one side disagreeing in their sexual type from those of the other. In the present division we have a similar sexual antagonism following a different direction; for supposing the internal sexual apparatus to be divided from the external by a transverse line, we have, in transverse hermaphroditism, on each side of this partition, organs of an opposite sexual type; in other words, the organs of reproduction (in the more correct sense of the word), or the internal sexual organs, do not, in the present species of hermaphroditism, correspond in type with the organs of copulation, or the external sexual parts—a circumstance, the occasional occurrence of which tends to show that these two portions of the generative apparatus are in some degree independent of each other in their normal development and existence, and consequently also in their abnormal formations.

Transverse hermaphroditism varies in its character according to the relative positions occupied by the coexisting male and female organs; the external organs, or all those exterior to the supposed transverse line, being sometimes female, and the internal male, and *vice versa*.

1. TRANSVERSE HERMAPHRODITISM WITH THE EXTERNAL SEXUAL ORGANS OF THE FEMALE TYPE.

In the cases included under this subdivision, the external genital organs consist of a clitoris, vagina, and uterus; the uterus is often dimentary and sometimes altogether absent and replaced by the ale vesiculæ seminales. The male internal organs are the testicles, enerally small and imperfectly developed, and placed either within without the abdomen, with vasa deferentia terminating in the terus and vagina.

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[1] Arnaud, loc. cit. p. 283.

This variety of sexual malformation has been repeatedly observed nong our domestic quadrupeds, particularly among black cattle. r. John Hunter, in an essay read before the Royal Society in 1779, d published in their Transactions,[1] and in his Observations on e Animal Economy, showed that, as had been long known among griculturists, when among black cattle the cow brings forth twin lves, one of them a male, and the other apparently a female, the ale is a perfect bull calf, but the female, while it has all the exteral marks of a cow calf, as the teats and udder, is still, with a very w exceptions, imperfectly formed in its internal sexual organs, and ery generally presents a mixture of the organs of the two sexes in arious degrees. Such hermaphroditic twin cattle have long been disnguished in this country under the name of *free-martins*. In some ceptional cases only have they been observed capable of breeding;[2] d generally they show no sexual desire for the bull, or the bull for em. In appearance they resemble the ox or spayed heifer, and ve a similar or still greater disposition to become fat under the e of good food.

In the paper to which we have referred, Mr. Hunter has described e dissection of three free-martins; and one of these seems to elong to our present division of female transverse hermaphroditism. he clitoris and external parts appear to have been strictly of the emale type, and there was a small udder with four teats. The agina terminated in a blind end, a little beyond the opening of the rethra, and from this point the vagina and uterus were impervious. he uterus at its superior part divided into two horns, and at the terination of these horns, not ovaria, but bodies resembling the male esticles, were found. These had not a perfect internal structure, ike that of testicles, but resembled these organs in so far that—1st. hey were nearly as large as the male testes, and much larger than he female ovaries; 2d, they were supplied with tortuous spermatic rteries like those of the bull or rigdil; and 3d, cremaster muscles assed up to them, as in rigdils, from the abdominal rings. There ere two small vesiculæ seminales placed behind between the bladder nd uterus, with their ducts opening into the vagina. Nothing, acording to Mr. Hunter, similar to the vasa deferentia, was present;

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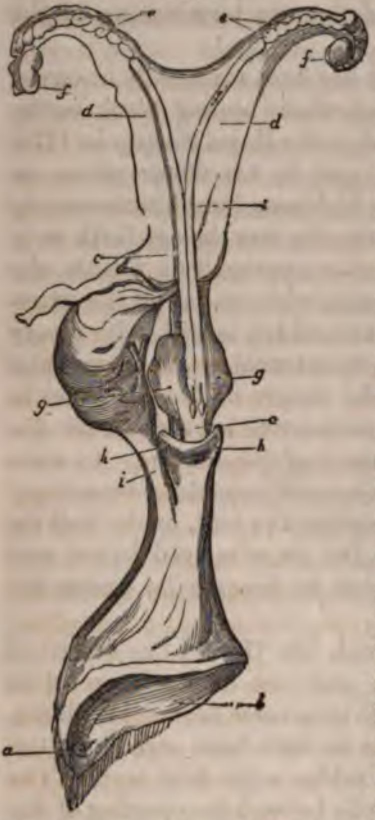
[1] Trans. Royal Society, vol. lxix.

[2] See vol. i. of this work, p. 286.

but Gurlt is inclined to believe that the parts which Mr. Hunter has described as the horns of the uterus were really the deferent vessels.

Fig. 21.



Professor Gurlt[1] has himself given, from a preparation in the Museum of the Berlin Veterinary School, the accompanying sketch of the malformed sexual organs of a five-year old free-martin (Fig. 21), which presents to us an illustration of Mr. Hunter's supposed mistake, at the same time that it affords a well-marked example of transverse hermaphroditism. The detail of the anatomical peculiarities of the case has been unfortunately omitted by the author, but from the short explanations appended to the drawing, it appears that the clitoris, *a*, and external pudenda, *b*, were perfectly feminine, and that the vagina, short and funnel-shaped, terminated at its superior contracted extremity in two vasa deferentia, *c*, *c*, *c*, which were carried upwards in a duplicature of peritoneum, *d*, *d*, resembling the broad ligament, until they joined the unrolled and lengthened epididymes, *e*, *e*, of two small testicles, *f*, *f*, placed in the position of the ovaries. Near the junction of the vagina and vasa deferentia, bodies resembling the male vesiculæ seminales, *g*, *g*, and Cowper's glands, *h*, *h*, were situated, and the urethral canal, *i*, opened into the vagina, and was shorter than it usually is in the cow.

We have found upon a free-martin cow, a state of the sexual apparatus very much resembling that figured in the above case by Professor Gurlt. The two vasa deferentia, as they ran in the duplicature of the peritoneum, had very much the appearance and shape of an imperfectly developed uterus. The vesiculæ seminales were large; the vasa deferentia were quite impervious throughout their whole course; and the bodies placed at their abdominal extremities were large, but of so indeterminate a structure as not to enable us to pronounce them to be either true testicles or ovaries.

M. Geoffroy St. Hilaire published in 1834 a very distinct case of an

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[1] Lehrbuch der Pathol. Anat. der Saug. Th. Bd. ii. s. 186.

maphroditic goat which had two male testicles and epididymes
h a two-horned uterus and female external parts.[1] M. Isidore
Hilaire[2] mentions a nearly analogous case in the same animal,
. quotes a third from Bomare which was observed upon a deer.[3]
'o the present division of transverse hermaphroditic malforma-
ı with external female and internal male organs, we may proba-
also refer the case of the hermaphrodite dog detailed by Sir E.
me,[4] and three instances in the sheep described by Ruysch,[5] Her-
dt,[6] and Gurlt.[7] In all these instances, imperfectly developed
icles were situated either within the abdomen or without it upon
udder, at the same time that the external parts exhibited in a
re or less marked degree the peculiarities of the female sex; the
;ina was, however, narrower, and the clitoris more developed than
the perfectly formed female; and in the dog mentioned by Home
ı latter organ was very large, being three-quarters of an inch
g and half an inch broad, but still it could not properly be
ısidered as an imperfect penis, since the bone, which forms the
tinguishing mark of that organ in the dog, was wanting.

Few well-marked instances of transverse hermaphroditism with
ernal female organs, have been hitherto described as observed
the human subject, unless we regard as an approach to it the
merous cases, already referred to, of spurious hermaphroditic
lformation in the male from hypospadiac division of the urethra,
otum, and perineum.

ı. In his essay on hermaphroditism, however, Steghlehner[8] has
ailed at great length the particulars of a case belonging to the
sent variety, which he met with on the body of a woman who died
phthisis at the age of twenty-three. The external sexual organs
re all of the female type and in general well formed, though the
toris and nymphæ were perhaps smaller than natural, and the
ficium vaginæ was rather contracted and half shut up by a hymen.
e fossa navicularis was very distinct, and the vagina normally
ated, but extremely short and narrow. Its internal surface pre-
ited an appearance of transverse and longitudinal rugæ, but its
per extremity formed a blind sac, and no traces could be found
yond it of the uterus, nor indeed any vestiges whatever of the
er internal female organs, the ovaries and Fallopian tubes. On

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[1] Nouv. Ann. du Museum d'Hist. Nat. tom. ii. p. 141.

[2] Histoire des Anomalies, tom. ii. p. 128. [3] Journ. de Phys. tom. vi. p. 501.

[4] Phil. Trans. for 1795, p. 157; Comp. Anat. iii. 323.

[5] Thesaur. Anat. viii. n. c. iii. tab. 115.

[6] Viborg's Sammlungs für Thierartze, 1797, s. 25.

[7] Lehrbuch, &c. Bd. ii. s. 186, tab. ix. 2, and xxii. s. 2.

[8] Tract. de Hermaphr. naturâ, p. 120.

more minute examination, a testicle with its spermatic cord was found in each inguinal region, placed outside the external ring, and surrounded with their cremaster muscles and vaginal coats. The testicles were flaccid and small, but their internal structure and that of their epididymes was natural; and the slender pervious vasa deferentia arising from them entered the abdomen, descended into the pelvis, and were joined behind the urinary bladder by two vesiculæ seminales of considerable size. Their common ejaculatory ducts opened into the vagina. The form of the thorax and pelvis, and of the body in general, was feminine; and the mammæ and nipples were well developed, but the larynx was rather more protuberant than in females, and the voice approached in tone to that of a man. There had never been any menstrual discharge, but the periodical molimina indicative of its appearance were said to have been observed regularly. There were some hemorrhoidal tumors situated around the anus.

b. If possible, a still more perfect example of the present variety of transverse hermaphroditism in the human subject has lately been observed at Naples. The malformation occurred in the person of an individual, Maria E. Arsano, who died at the age of eighty in one of the pauper charities at Naples, and who had passed through life as a female and been married as such. No suspicion of the malformation existed during life, and it was only accidentally discovered when preparing the dead body for demonstration in the anatomical theatre of Professor Ricco, who afterwards carefully dissected the malformed parts in company with Professors Sorrentino and Grosetti. We have taken the following account and sketches from Ricco's published description of the case.[1]

The external organs of generation were those of the female in their natural or normal state, consisting of the mons veneris, with a scanty quantity of hair (Fig. 22, *a*); of the labia externa (Figs. 22 and 23, *b b*) naturally formed, and the nymphæ (Figs. 22 and 23, *d d*); of the clitoris (Figs. 22 and 23, *c*), which was perfectly imperforate, and of the ordinary size of the same organ in the adult female; of the orifice of the urethra (Figs. 22 and 23, *e*), situated below the clitoris; and of the os vaginæ (Figs. 22 and 23, *f*), which was of the usual size and diameter. Altogether, the aperture of the vulva was natural. The canal of the urethra was of the usual length, as seen at *u* in the section of the pelvis represented in Figure 23, in which *s* marks the divided symphysis pubis, and *p* the peritoneum. The os vaginæ showed no vestiges of the membranes of the hymen, or, in other words, was without carunculæ myrtiformes. The canal of the vagina (Fig. 23, *v*) was about two inches long, but without

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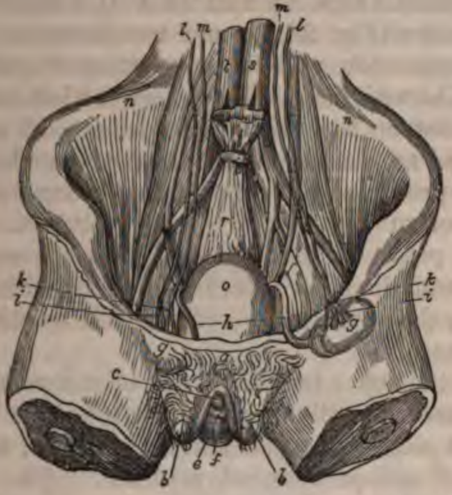
[1] Cenno Storico su di un Neutro-Uomo, pp. 5, 7.

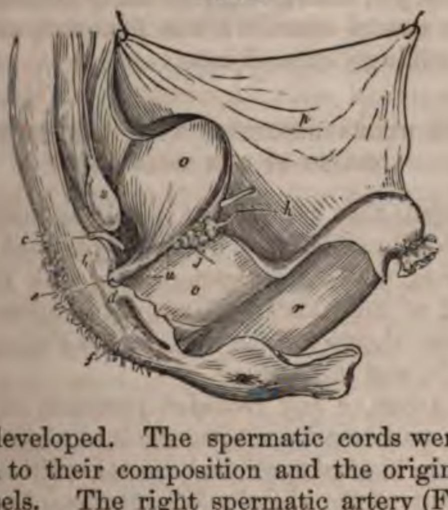
, and it terminated internally in a completely blind extremity l-de-sac. The uterus was entirely wanting, as were also the pian tubes and uterine ligaments.

Fig. 22.



e internal organs of reproduction were, on the other hand, letely male. The two testicles (Fig. 22, *g g*) were situated in egion of the pubis, and were scarcely clear of the inguinal . They were of the usual ovoid figure, and natural in size. had internally the structure of the tubuli seminiferi, but it

Fig. 23.



not well developed. The spermatic cords were quite normal in regard to their composition and the origin and course of bloodvessels. The right spermatic artery (Fig. 22, *l*) arose, sual, from the renal, and the corresponding vein, *m*, after forming the pampiniform plexus, *k*, opened into the vena cava inferior; while on the left side, the artery, *l*, arose from the aorta, and the vein, *m*, terminated in the left emulgent. The epididymes of the testes were also of the usual vermiform figure, and the corresponding vasa deferentia (Figs. 22 and 23, *h h*), coursed towards their vesiculæ seminales (Fig. 23, *j*), and terminated in an attenuated membranous expansion without any external aperture or ducti ejaculatorii. The vesiculæ seminales (see the left one, *j*, in Fig. 23) were placed between the urinary bladder, *o*, and rectum, *r*; they were smaller and more shrunk than those of the adult male, though certainly they preserved their naturally oblong form. Their internal hollow or tubular structure was indistinct. The prostrate gland was not present. The urinary bladder, *o*, and ureters, *n n*, the rectum, *r*, and the other intestinal viscera, with the abdominal bloodvessels (*s*, the aorta, *t*, the vena cava, Fig. 22) seem to have been all quite natural.

The head of the above individual was of the usual size, the neck long, and the stature ordinary. The periphery of the thorax was so expanded as almost to equal that of the male, notwithstanding the presence of well-pronounced mammæ. The face, although entirely free from hair, had yet neither the expression of that of a female nor of a male, but showed more of that mixed character which is seen in the eunuch. The pelvis was altogether that of a male in its form and dimensions, and the limbs were perfectly masculine. According to information collected after death, the voice was deep, and the temperament strong and firm. Though there was never any menstruation, yet, from being constantly employed in domestic occupation, the mental character was feminine, and the married state had been willingly entered into.

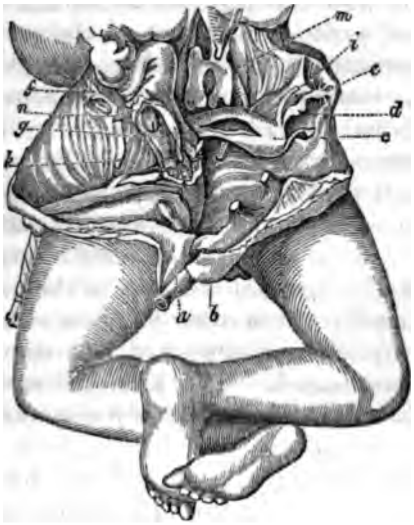
2. TRANSVERSE HERMAPHRODITISM WITH THE EXTERNAL SEXUAL ORGANS OF THE MALE TYPE.

The male organs that are present in this subdivision, consist of the penis, which is provided with a regularly formed prepuce, glans, corpora cavernosa, and corpus spongiosum, with the urethra perforating it, and of the prostate gland, verumontanum, &c. The coexisting female organs are the ovaries, the Fallopian tubes with their infundibula, and the uterus.

We are not aware of any recorded instances of this variety of hermaphroditic malformation among the lower animals. We have already, under the head of spurious hermaphroditism in the female, from enlargement of the clitoris, &c., mentioned several cases, in which, from excessive development, the external organs in women had assumed some of the characters of the corresponding parts in man; but the two following cases described by Professor Eschricht of Copenhagen, and Bouillaud of Paris, present instances of malformation in which the more exterior sexual organs were all formed upon the male, and the internal upon the female type.

a. The subject of the case described by Eschricht[1] was a twin child that died very shortly after birth, and in whom the external sexual organs were of the male type, and the internal female. The penis (Fig. 24, *a*) and scrotum, *b*, were well developed, but the usual raphé seen upon the latter was absent. The urethral canal of the glans and body of the penis was pervious throughout, and admitted of a sound being easily passed into the bladder. The glans was remarkably thin and slender. The prepuce could be easily pushed back. No testicles could be felt in the scrotum, and internally there was a uterus with Fallopian tubes and ovaries. The uterus, *c*, was about an inch in length, and had the general form presented by this organ in female infants. It contained a cavity marked with rugæ, but had no orifice inferiorly, nor any vagina attached to it. Its blind or imperforate neck was firmly attached to the posterior walls of the urinary bladder, *g*, while its fundus was directed very obliquely downwards and over to the left side. From the left side of the fundus of the uterus, a twisted Fallopian tube, *d*, proceeded, having well-developed fimbriæ, *e*, at its abdominal extremity, and the broad ligament or fold of peritoneum along which it ran, contained an oblong soft body, *i*, which Eschricht considered

Fig. 24.



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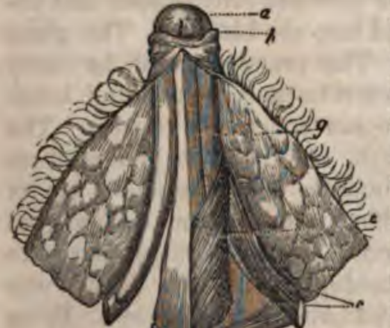
[1] Müller's Archiv für Anatomie, &c., 1836, Heft ii.

as distinctly an ovary, and a round ligament that took its course through the inguinal canal of the same side. On the right side, an ovary, *k*, and Fallopian tube, *f*, were likewise discovered, but they were displaced and separated from the body of the uterus. The ovary lay in the iliac region, and above it and towards its outer side was placed the fimbriated extremity of the corresponding Fallopian tube. The tube presented towards this extremity a vesicular swelling of the size of a small pea, which admitted of being inflated and filled with quicksilver through a small opening between the fimbriæ. Below this it was impervious, and apparently diverged off into two prolongations, one of which (the round ligament) passed down into the inguinal canal, and the other crossed over with a fold of peritoneum to where the rectum and urinary bladder were preternaturally connected together. Professor Jacobson suggested that this latter part was a rudiment of the right half or horn of the uterus. It may, perhaps, however, be more properly regarded as the commencement of the right Fallopian tube, and in this case it would, if continued onwards, have been joined to the neck of the uterus—an arrangement which would be quite in accordance with the usual deep and displaced origin of one of the tubes in instances of congenital obliquity of the uterus.

The child was malformed in other respects also. The anus was imperforate, and the rectum, *n*, opened into the urinary bladder, which was very contracted. The kidneys, *m*, were irregularly formed, and lay near the promontory of the sacrum. There was an accessory spleen, and the formation of the heart and large vessels was abnormal. The other twin child was well formed, and lived.

Fig. 25.



b. The case of transverse hermaphroditism observed by Bouillaud[1] was even still better marked than that of Eschricht. Valmont, the individual who was the subject of it, died in one of the hospitals of Paris of the epidemic cholera. He was a hatter by trade, and had been married as a male. No further particulars of his history or habits could be obtained. The following was found by MM. Manec and Bouillaud to be the state of the external and internal sexual organs.

Externally there was a penis (Fig. 25) of medium size, termiating in a regularly formed glans, *a*, and furnished with a pre-uce, *b*.

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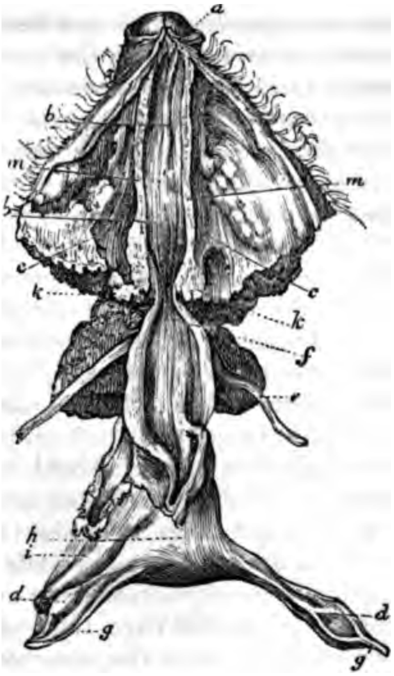
[1] Journ. Hebdom. de Med., tom. x. p. 466. "Exposition Raisonnée d'un cas de nouvelle et singulière variété d'hermaphroditisme observée chez l'homme."

The urethra (Fig. 26, *b b*) opened on the inferior side of the glans Figs. 25 and 26, *a*). In its course from this point backwards to he bladder, it perfectly resembled the urethra of the male, and was urrounded at its origin by a well-formed prostate gland (Fig. 26, *k k*). owper's glands were also present (Fig. 25, *d*). The verumontanum r caput gallinaginis was distinct, as well as the orifices of the pro-tatic follicles; but the usual openings of the seminal canals could ot be found. The corpus spongiosum urethræ (Fig. 25, *g*) and the orpora cavernosa (Fig. 26, *m m*) were as well developed as in the per-fect male subject. The scrotum was small, and did not contain any testicles; it presented on its middle a line or raphé extending from the prepuce to the anus, and which was harder and better marked than it usually is upon male subjects. The various muscles of the male perineum (Fig. 25, *c c*) were present, and very perfectly formed. The constrictores urinæ muscles, *e*, were particularly long and thick.

Fig. 26.



In the cavity of the pelvis, two ovaries (Fig. 26, *d d*), similar in form and structure, according to M. Manec, to those of a girl of fifteen or sixteen years of age, or, to adopt M. Bouillaud's statement, two bodies, in some sort fibrous, and perhaps intermediate in their structure between ovaries and testicles, were found, along with two Fallopian tubes (Fig. 26, *g g*), having each a fimbriated extremity at one end, and opening by the other into the cavity of a uterus, *h*, which occupied the usual situation of that organ in the female, and opened inferiorly into a kind of vagina, *e*. The internal surface of the uterus showed the usual arborescent wrinkles of this organ in the unimpregnated state; the os tincæ was regularly formed; the vagina was about two inches long, and of medium size, and presented internally numerous ridges, such as are met with in virgins. This canal, when opposite the neck of the bladder at *f*, became much contracted, and was continued downwards in the form of a small tube to the membranous portion of the urethra, into which it entered by a narrow orifice. The broad ligaments of the uterus were normally formed; the round ligaments passed through the inguinal canal, accompanied, each, by an artery larger than that of the corresponding one in the female sex.

The external appearance and form of Valmont are described by M. Bouillaud as having been intermediate between those of the male and female sex. The stature was short; the mammary glands and nipples were well developed; the face was bearded; but the general physiognomy was still delicate. The body was fat; the hands and feet were small; the pelvis was shallow; and the haunches were wider than in a well-formed man.

C. TRUE DOUBLE OR VERTICAL HERMAPHRODITISM.

In the two divisions or orders of true hermaphroditism which have been already considered—the *lateral* and the *transverse*—we have seen reunited upon the body of the same individual more or fewer of the organs of the two sexes, but so arranged as not necessarily at least to present the occurrence of actual duplicity in any of the corresponding male and female parts. In both lateral and transverse hermaphroditism, the type of the sexual apparatus is in fact *single*, in so far that it consists in almost all cases in the presence, at one part, of an organ or organs differing in sexual type from those that are present at other parts, without there necessarily coexisting at any one point the two corresponding male and female organs. In the present or third variety, however, of true hermaphroditism, we come to a tendency to actual sexual *duplicity*, in the coexistence of two or more of the analogous organs of the two sexes upon the same side, or in the same vertical line of the body. At the malformed point or points the sexual apparatus is *double*, and one part male, the other part female, in type. For, supposing we viewed, either from before or behind, the reproductive organs belonging to the two sexes all stretched out upon the same plane, so that their corresponding organs should be exactly superimposed upon one another—as the two female ovaries upon the two male testicles, the Fallopian tubes upon the vasa deferentia, the uterus upon the vesiculæ seminales and prostate gland, &c.—we find in vertical or double hermaphroditism more or fewer of those analogous organs of the two sexes that were thus placed upon one another, and that consequently lay in the same vertical line, or upon the same side of the body, coexisting together at the same time upon the same individual.

Double, vertical, or complex hermaphroditism differs much in variety and degree in different cases, from the imperfect repetition of two only of the corresponding organs of the male and female upon the same body, to the reunion or coexistence of almost all the analogous internal genital organs of both sexes upon one individual.

For the purpose of contrasting and collecting together as much as possible the more analogous cases, we shall arrange the instances of double hermaphroditism under three genera or divisions; the *first* including cases in which there coexisted a female uterus and male vesiculæ seminales, with a general female type; the *second*, those in which a female uterus, occasionally provided with Fallopian tubes, was added to an organization that was in other repects essentially male; and the *third* comprehending all examples in which ovaries and testicles are alleged to have been repeated together upon one or both sides of the body. Other divisions of double hermaphroditism may become necessary under the accumulation of new varieties of cases, but we believe it will be possible to arrange all the instances hitherto recorded under one or other of the above divisions. In classifying and describing these instances we shall in the meanwhile offer no observations on the probable anatomical mistakes that have been committed in the examination of individual cases; and we shall describe the various malformed parts according to the designations usually given to them in normal anatomy, and by the authors who have described the cases. We reserve the true anatomical and morphological value of the different structures named and involved, for special consideration under a separate and future chapter, where we shall endeavor to show the numerous sources of error with which the observation of individual examples and varieties of complex hermaphroditism is beset.

1. MALE VESICULÆ SEMINALIS, ETC., SUPERADDED TO ORGANS OF A FEMALE SEXUAL TYPE.

In this first genus or division of double hermaphroditism we find two female ovaries, or bodies resembling ovaries, and an imperfect uterus, coexisting with two male vesiculæ seminales, which are occasionally accompanied, also, with rudiments of the vasa deferentia. One of the free-martins described by Mr. Hunter[1] is referable to this variety of double hermaphroditism. The external genital organs and mammæ resembled those of the cow, but were smaller in size. The vagina, beyond the opening of the urethra into it, was, with the uterus itself, impervious. The imperfect uterus divided into two horns, at the end of which were the ovaria. On each side of the uterus there was an interrupted vas deferens, broken off in several places; and between the bladder and vagina these vasa deferentia terminated in two vesiculæ seminales. The ducts from the vesiculæ and the vasa deferentia opened into the vagina. In this instance we have all the female organs present, but imperfect in their development; and at the same time there is superadded to them a tubular structure, formed, according to Mr. Hunter's opinion, of the male vesiculæ seminales and vasa deferentia.

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[1] See An. Econ., p. 64, Mr. Well's free-martin.

In a free-martin cow, which I had an opportunity of dissecting, an arrangement of sexual parts very similar to that described in the preceding case was found. The uterus, however, though small, was pervious for a distance of some inches above the vagina; and at the abdominal end of each blind Fallopian tube there was a dilated sac of considerable size, lined by peritoneum, and opening into the abdominal cavity by a small orifice. These sacs we considered as abortive attempts at the formation of the fimbriated extremities. Imperfect bodies which we considered as ovaries were placed in their normal situation near the cavities which we mention. They were small in size, and of an oblong shape. On a section being made of them, they showed internally a kind of dense homogeneous yellow tissue, dotted or crossed with strongly marked white lines. The male vasa deferentia could be traced along each side of the uterus in the form of broken dense cords. The vesiculæ seminales were large and partially hollow, and near them on each side there was an oblong body of considerable size, having the appearance of Cowper's glands. The tubes from them, and from the vesiculæ seminales, opened near the os tincæ into a vagina of nearly the usual size.

2. AN IMPERFECT FEMALE UTERUS, ETC., SUPERADDED TO A SEXUAL ORGANIZATION ESSENTIALLY MALE.

In the cases included under this second division of double or vertical hermaphroditism, there exist a male testicle or testicles, vasa deferentia, and vesiculæ seminales, along with a female uterus. The uterus occupies its normal situation between the bladder and rectum. It is sometimes defectively developed, and of a membranous structure; and occasionally it is not provided with Fallopian tubes, or, in the quadruped, with cornua. The cavity of the uterus communicates with a vagina that either opens in its usual situation externally, or, as happens more frequently, joins the male urethra. In some cases the vagina is wanting, and the uterus opens directly into the canal of the urethra.

Several cases of sexual malformation in the ram, goat, and dog, referable to this variety of double hermaphroditism, have been described by different authors; and various analogous instances have now also been observed in the human subject.

In a lamb described and delineated by Mr. Thomas,[1] all the external parts were male, but the scrotum was divided or hypospadiac. Internally there were two perfect male testicles in the situation of the ovaries, with their epididymes, vasa deferentia, and vesiculæ seminales; and a well-formed two-horned uterus furnished with its usual ligaments, and with Fallopian tubes that ran up and terminated in a tortuous convoluted manner upon the testicles. The body of the uterus possessed the common rugose structure, but the horns were lined by a smooth membrane, without their usual glandular bodies internally. At the anterior extremity of the fundus uteri, a thick semilunar valve, which seemed to correspond to the os tincæ, passed across, and hardly allowed a fine probe to be entered over its upper edge. The vagina scarcely existed, and formed only a short, smooth pouch, terminating below in a cul-de-sac. The male vesiculæ seminales and vasa deferentia entered the male urethra in their normal situation at the caput gallinaginis.

Gurlt[2] has described and delineated the sexual parts of a goat, in which all the internal male genital organs were found, with the exception of Cowper's glands (Fig. 27). There was also present a uterus, *e*, provided with long but narrow and curved cornua, *f f*, that accompanied the vasa deferentia and testicles through the abdominal rings, and ended blind at the epididymes. The testicles lay externally upon the udder, which was of considerable size. The scrotum was absent; the penis, *g*, was short, tortuous, and imperforate; and there was a fissure in the perineum into which the urethra, *h*, opened.

Stellati[3] has recorded an analogous case in the same animal. The male sexual organs were not entirely complete, and there were superadded to them a female vagina and an imperfectly developed uterus, the Fallopian tubes of which ran towards the inguinal rings, and terminated with them upon the epididymes of the testicles.

Another instance of hermaphroditic malformation in the goat, detailed at great length by Meckel,[4] seems also in its principal points justly referable to the present division of cases, although there was at the same time a tendency, in the unequal size of the two cornua uteri, &c., to a degree of lateral hermaphroditism.

Professor Mayer of Bonn[5] has detailed at length the dissection of three hermaphroditic goats, in all of which the conformation of the sexual parts resembled in its more essential features the preceding cases of Thomas and Gurlt. In all the three instances there were found two male testicles with their epididymes, vasa deferentia, and vesiculæ seminales; and at the same time there was present a well-marked two-horned uterus, with a vagina, opening into the urethra.

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[1] London Med. and Phys. Journ. vol. ii. 1799, p. 1; with a good drawing of the malformed organs of generation.

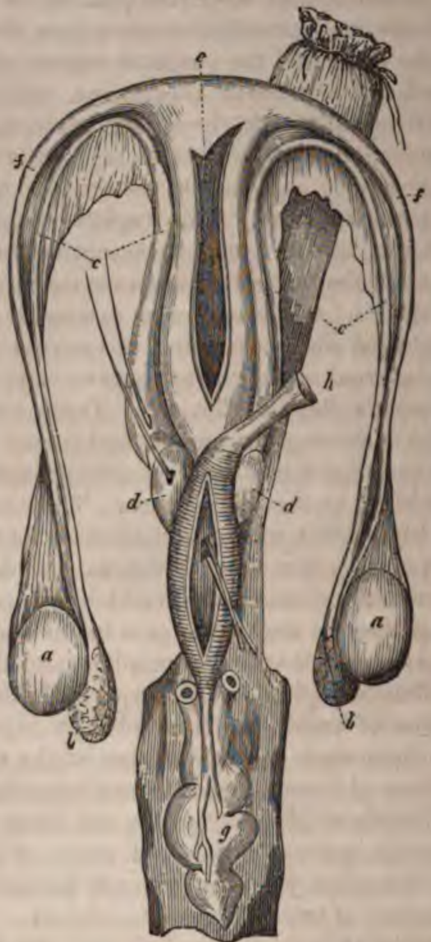
[2] Lehrbuch der Pathol. Anat., Bd. ii. s. 195, pl. ix. figs. 1 and 2, and pl. xxii. figs. 3 and 4.

[3] Atti del Real Instit. d' Incoragg. alle Sc. Nat., Naples, tom. iii. p. 380.

[4] Reil's Archiv für die Physiologie, Bd. xi. s. 334–38.

[5] Icones Select. Præparat. Mus. Anat., Bonn. pp. 17–20, tab. iv. fig. 5, and tab. v. figs. 1, and 3.

Fig. 27.



In the first case the large hollow cornua uteri terminated in blind extremities, and there were only very short impervious rudiments of the Fallopian tubes. In the second case, at the extremity of the right horn of the uterus, a blind appendiculum was situated, formed by a vestige (according to Mayer) of the Fallopian tube; and from this

Fig. 27. *a a*, the testicles; *b b*, epididymes; *c c*, vasa deferentia; *d d*, vesiculæ seminales.

ligament was sent off to the corresponding testicle; a similar ligament, but no appendicula, existed on the left side. In the third case, both Fallopian tubes were present, and each ended in a bursa formed by the lamina of the peritoneum, and partly surrounding the testicle and epididymes. In two of the instances, the ejaculatory ducts seem o have opened into the urethra near the point at which the vagina erminated in it; and in one of the cases they opened into the canal of the vagina itself before it joined that of the urethra. All the external organs were male, but malformed in so far that the penis was short, and in two of the cases somewhat twisted; and the scrotum was either small or wanting.[1]

The same author[2] has described the dissection of a dog, the sexual organs of which exhibited a similar variety of hermaphroditic malformation. The Fallopian tubes were pervious throughout in this instance, and at their further extremities opened upon the neighboring cellular tissue. The body of the two-horned uterus was very small. On compressing the epididymes and vasa deferentia, a fluid resembling semen issued from the openings of the latter into the urethra. The external sexual parts were those of a hypospadiac male.

Several cases of hermaphroditic malformation in the human subject, similar in their anatomical characters to the preceding, have been described by Columbus, Harvey, Petit, Ackermann, Steghlener, and Mayer.

a. In a person with external hypospadiac male organs, Columbus[3] found two bodies like testicles in the situation of the ovaries, and larger in size than the latter female organs naturally are. From each of these testiform bodies two sets of tubes arose, one of which, like the male vasa deferentia, passed on to the root of the penis and opened into the urethra; while the other, like the female Fallopian tubes, were inserted into a uterus. The prostate gland was absent.

b. Harvey[4] has mentioned a very small hermaphroditic embryo, on which he found a two-horned uterus with two testicles of a very small size, and, near the diminutive penis, some traces of a prostate gland.

c. The observation of M. Petit[5] of Namur is still more complete. On the body of a soldier, aged twenty-two, who died of his wounds, and whose external organs appear to have presented no deviation from the male type except in the absence of the testicles from the scrotum, these bodies, with male vasa deferentia, vesiculæ seminales, and a prostate, were found to coexist with female Fallopian tubes, and a uterus that was attached to the neck of the urinary bladder, and opened into the urethra between this neck and the prostate. The form of this imperfect uterus, M. Petit remarks, merited for it rather the name of a vagina than of a uterus, and it resembled more this organ in the female quadruped than in woman. From the body of the uterus, at three inches from its entrance into the urethra, two Fallopian tubes arose. These tubes were perforated, and were three inches and a half long; their abdominal extremities were not loose and provided with fimbriæ, but were attached to a small soft body on each side, occupying nearly the natural situation of the ovaries, but having the substance or structure of the testicles, and provided with an epididymis and vas deferens. The vasa deferentia were each seven inches and a half long, and were attached to two long and rather slender vesiculæ seminales placed alongside of the uterus. The vesiculæ opened into the urethra by two ducts.

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[1] For other similar cases in the goat, see Leuckhart, in Cyclopædia of Anatomy, vol. iv. p. 1426; and Kobelt's Neben-Eierstock des Weibes, &c., p. 38.

[2] Loc. cit. p. 16, tab. iv. fig. 3, external parts of generation; fig. 4, internal.

[3] De Re Anat. lib. xv.

[4] De Gen. Anim. Exerc. lxix. p. 304.

[5] Hist. de l'Acad. Roy. des Sc. for 1720, p. 38.

In a note appended to this case, M. Petit states that he had been consulted by a man who passed blood by the penis regularly every month, without pain or any troublesome symptom. Perhaps, adds M. Petit, this man had also a concealed uterus. I have been informed, on credible authority, of two similar cases, the one in a young unmarried man of seventeen years of age, and the other in a person who had been married for several years without his wife having had any children. In both of these cases the discharge was in very considerable quantity, and perfectly regular in its monthly occurrence. Did it consist in a periodical hemorrhage from the urinary bladder or passages only? Or was it, as M. Petit seems to suppose in his instance, of a true menstrual character, and produced by a female uterus, &c., existing internally, and communicating as usual with the canal of the urethra?

d. Professor Ackermann[2] of Jena published in 1805 the following interesting case of the present variety of hermaphroditic malformation. It occurred in an infant that lived about six weeks after

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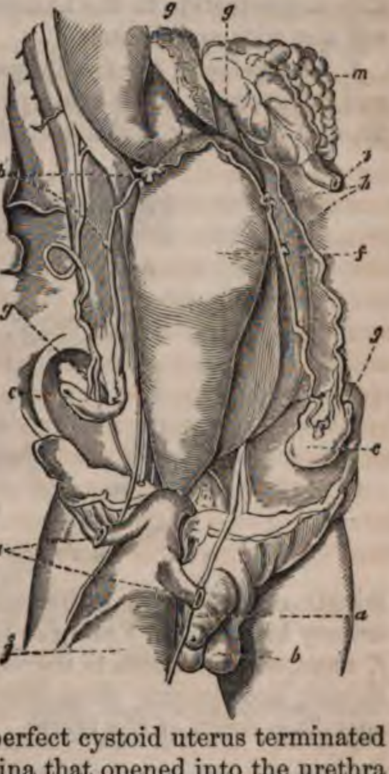
[1] More lately, several similar cases have been recorded of apparent menstruation though a perforate penis, as by Harris, in American Journal of the Medical Sciences for July, 1847; two cases by Blackman, in Ibid. for July, 1843, &c. In an American case, that of Suydam, there was one descended testis, and a hypospadiac penis. This person is said to have had a regular catamenial discharge. The question was raised as to whether this person was a male or female, and entitled or not entitled to vote as a *freeman* at a political election. See Dr. Taylor's Medical Jurisprudence, p. 613. See also Ibid. p. 256.

[2] Infantis androgyni historia et iconographia; Edin. Med. and Surg. Journal, vol. iii. p. 202.

On dissection, two testicles were found; one of them had led into the scrotum or labium; the other had advanced no than the groin. Both were perfectly formed, and had their ppendages complete. In the natural situation of the female there was found a hollow pyriform organ, which, from its and connections, was supposed to be a uterus, though its ere finer and thinner, and its cavity greater than naturally s to that viscus. Duplicatures of peritoneum, resembling the nta lata, connected this imperfect uterus with the sides of vis, and its cavity opened into a kind of short vagina, which nited with the urethra, and formed one common canal with *ina urethralis.* The vasa deferentia ran from the testicles s the superior angles of the uterus, and penetrated into its ce at the points where the Fallopian tubes are usually placed. ıt opening here, however, they passed onwards under the l mucous-like membrane of the uterus and vagina, and at terminated, by very small orifices, in the vagina urethralis. iately previous to entering amenta lata, each vas deformed a number of conns, conglomerated into a sembling a vesicula semi-

Fig. 28.



eghlener[1] has described at ength the case of an infant ırvived only for half an ter birth, upon whose body nd perfect external male (Fig. 28, *a b*), and interwo small elongated testic, with their epididymes, d the convolutions of imte vasa deferentia, *b b*, dismarked. The testes were, ı in the sketch, supplied permatic arteries followir usual course. Between um and bladder there was a very large pear-shaped pouch, *f*, with firm, coriaut not thick, walls, and diswith fluid. This bag or imperfect cystoid uterus terminated ly by a narrow neck in a vagina that opened into the urethra in the situation of the verumontanum, and was there dilated into a large bag or ampulla, occupying exactly the site of the prostate gland, and resembling this organ also in its form and position. The internal membrane of the uterus was collected at its neck into numerous valvular-like folds, and that of the vagina had also a rugous or plicated arrangement. From the fundus of the large sac of the uterus, and not from its angles, but from near its middle, two impervious solid ducts (vasa deferentia) arose, and after a somewhat flexuous course reached the testicle, *c c*, lying in the superior part of the iliac fossæ. These ducts had attached to them at one or two points a number of small reddish nodules, *b b*, consisting, according to Steghlener, of glandular granules, and described by Ackermann in his case as vesiculæ seminales. The canal of the urethra was obliterated for a short distance towards the fossa navicularis, and the urinary bladder, *j*, and uterus, *i i*, were extremely distended, and the left kidney, *m*, was vesicular.

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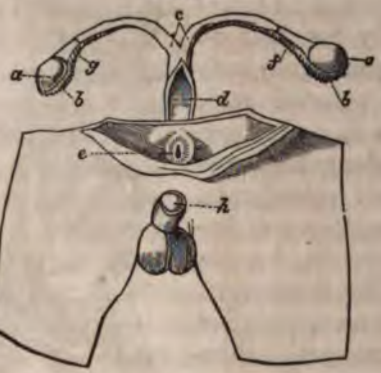
[1] De Hermaphr. Nat. p. 104.

Mayer, in the work already referred to,[1] has described and delineated the following five cases of the present species of hermaphroditic malformation in the human subject, all of which he had himself met with and dissected.

f. In a fœtus of the fourth month, and affected with omphalocele and extroversion of the urinary bladder, he found male testicles (Fig. 29, *a a*) with their epididymes, *b b*, and a two-horned uterus, *c*, terminating in a vagina, *d*, that opened into the posterior part of the urinary bladder, *e*. From the left testicle a contorted vas deferens, *f*, arose, and ran down to the vagina; the right vas deferens, *g*, was shorter, and became threadlike, and disappeared near the corresponding cornu of the uterus. A rudiment only of the left male

Fig. 29.



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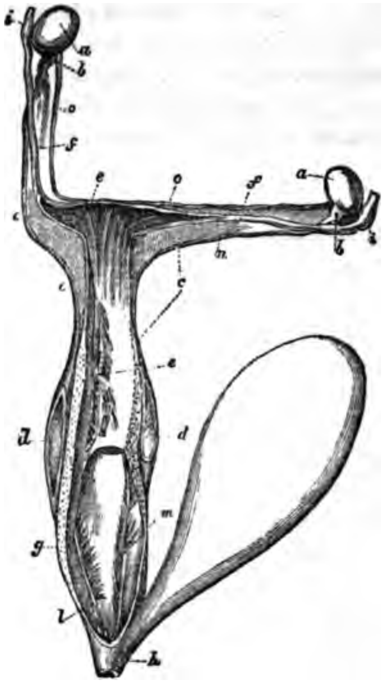
[1] Icones Select. &c., pp. 8–16. See also Walther and Graefe's Journal der Chirurgie und Augenheilkunde, Bd. vii. Hft. 3, and Bd. viii. Hft. 2.

eminalis was observable. The external organs were male;
penis, *h*, was imperforate.

nother fœtus of the sixth month,[1] there existed a perfect
ernal and external male sexual organs, viz., testicles, epi-
vasa deferentia, and vesiculæ seminales, with a prostate
l a normally formed penis and scrotum. But beside these,
also present an imperfect female uterus, the body of which
ıto two cornua, the right longer and incurvated, the left
ıd sacciform. The neck of the uterus was marked inter-
its usual arborescent appearance; and it opened into a
at terminated in the urethra near the exit of the latter
urinary bladder.

third case[2] of hermaphroditic malformation in an infant
of convulsions when six months old, Mayer found the
blending of the organs of the two sexes. Of the internal
tal organs there were present two bodies at the inguinal
t were evidently testicles (Fig. 30, *a a*), as was proved not

Fig. 30.



heir position but by their form, coverings, connections,
ıal structure, "their substance," says Mayer, "being evi-
mposed of yellow canals;" their epididymes, *b b*, were

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[1] p. 8, tab. ii. fig. 5.

[2] Ibid. p. 9, tab. iii. figs. 1 and 2.

also distinctly developed, and each of them sent off a vas deferens, *c c*, which was furnished with a corresponding multilocular vesicula seminalis, *d d*. Of the internal female sexual organs there were found a perfectly developed uterus, *e e*, with its broad, *n n*, and round, *o o*, ligaments naturally formed and placed, and provided with two Fallopian tubes, *f f*, that followed the course of the testicles through the inguinal canals, and vagina, *g*, which opened into the urethra, *h*, near its external orifice. The ejaculatory ducts of the male vesiculæ seminales opened into this vagina at *l* and *m*. The internal surface of the vagina was already beginning to present the appearance of its usual rugæ. The cavity of the uterus was triangular, and exhibited on the internal part of the cervix its characteristic plicated or arborescent structure. The Fallopian tubes were, at their uterine orifices, of a large calibre; their cavity afterwards became suddenly contracted, and then again dilated, and terminated at their ulterior extremities, where they lay in contact with the testicles at the external inguinal rings, in blind sacs, *i i*, without any very distinct appearance of fimbriæ. The external genital parts in this very interesting case were of a doubtful nature, being referable either to those of a hypospadiac male, or of a female with a large clitoris, but without nymphæ, the meatus urinarius being in its normal situation, but leading behind to the cavities of both the urinary bladder and uterus. The circle of the pelvic bones was large.

i. The two other instances described by Mayer occurred in adult subjects, and the malformation in both of them differed from that found in the cases just now cited, in this, that there was only one testicle present along with the imperfect uterus.

The subject of one of these cases[1] was a person who died at the age of eighteen, and whose external sexual organs were those of a hypospadiac male, with a narrow perineal canal or fissure. On dissection, this perineal canal was found to communicate anteriorly with the urethra, and posteriorly with a vagina of two inches and nine lines in length, and five or six lines in calibre. The anterior and posterior columns of rugæ belonging to the vagina were only slightly marked. Its canal led to a large dilated uterus, the superior part of which was unfortunately cut away with some diseased viscera before the genital organs were examined; but, from the portion left, this organ seemed to resemble the uterus of quadrupeds in its oblong form, and in the thinness of its walls, which were composed of a cavernous fibro-vascular texture, and full of lacunæ. The usual arborescent appearance of the internal surface of the os uteri was very perfectly marked. Besides these female rgans, there was a well-formed male prostate gland at the neck of he bladder; and behind the abdominal ring of the right side, a mall roundish body, similar in form and texture to the testicle, and aving the cremaster muscle adhering to its membranous involu-rum. There were no traces of any similar organ on the left side. n both sides some portions of a canal were seen, but whether they ere the remains of the vasa deferentia or Fallopian tubes was not scertained, on account of the previous mutilation of the uterus. n each side of the neck of the uterus there was placed a vesicula eminalis, provided with an ejaculatory duct that opened into the rifice of the vagina. The dimensions of the pelvis approached uch nearer to those of the female than those of the male. In the econdary sexual characters of the individual, the female type was urther recognized in the want of prominence in the larynx, in the lender form of the neck, and (according to Professor Mayer) in the ounded shape also of the heart, the smallness of the lungs, the blong shape of the stomach, the large size of the liver, the narrow-ess of the forehead, and the conformation of the brain; while the ndividual approximated, on the other hand, to the male in the ength and position of the inferior extremities, in the breadth of the thorax, the undeveloped state of the mammæ and the hairy condition of their papillæ, and in the existence of a slender beard upon the chin and cheeks.

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[1] Icones, p. 11, tab. iii. figs. 3 and 4.

j. In the second adult subject, a person eighty years of age, Mayer found,[1] on the left side of the cavity of the abdomen, and near the inguinal ring, a small oval body exhibiting imperfectly in its internal structure the tubular texture of the male testicle, and having an appendix resembling the epididymis attached to it. From this testicle arose a vas deferens, which was joined in its course by a vesicula seminalis, and ended in an ejaculatory duct. On the opposite or right side, a vesicula seminalis, having no continuous cavity, was present; but no vestige of a corresponding testicle, vas deferens, or ejaculatory duct, could be discovered. The prostate gland was present, and regularly formed. In the cavity of the pelvis a uterus was found with parietes of moderate thickness, and of the usual cavernous texture; its cervix was marked internally with the appearance of the natural arborescent rugæ. Inferiorly it opened into a narrow membranous vagina, that received the right ejaculatory duct, then passed through the body of the prostate, and latterly joined the canal of the urethra. The fundus of the uterus could not be examined, as it had been removed in a previous stage of the dissection. The external parts were male and naturally formed, with the exception of the penis, which was shorter than usual, and had the canal of the urethra fissured inferiorly, and the meatus urinarius situated at its root. The individual was during life regarded as a male, but had all along remained in a state of celibacy. The general appearance of the face and body was that of an imperfectly marked male, but the pelvis was broad like that of a female.[1]

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[1] Icones, p. 15, tab. iv. figs. 1 and 2.

3. COEXISTENCE OF FEMALE OVARIES AND MALE TESTICLES.

This third division of complex or double hermaphroditism includes all those cases in which a male testicle and female ovary exist together, either upon one side only, or upon both sides of the body. With this arrangement, other malformations by duplicity of the sexual organs are generally combined; but these are so various in their character as not easily to admit of any useful generalization. In considering this third division of complex hermaphroditism, we shall mention, *first*, the cases in which *two* testicles and *one* ovary are stated to have coexisted; and *secondly*, those in which there have been supposed to be present *two* testicles and *two* ovaries.

Two testicles and one ovary.—The two dissections that we have previously detailed of lateral hermaphroditic insects,[2] show that in these two cases this variety of sexual duplicity existed. It appears to have been observed also in two instances of hermaphroditic malformation in the quadruped, the histories of which have been described by Mascagni and Mayer.

In a bull, nine years of age, and which was provided with the usual external organs of the male, Mascagni found internally, on dissection, a prostate gland and two perfect vesiculæ seminales, vasa deferentia, epididymis, and testicles. The testicles and epididymes were injected with mercury through the vasa deferentia. In addition, there was discovered near the left testicle, and connected to it by peritoneum and bloodvessels, a body having the structure of the female ovary; and, in its normal situation, there existed a distended double uterus, containing from fifteen to sixteen pounds of a clear fluid. This uterus was furnished with two Fallopian tubes at its upper part, and terminated inferiorly in a vagina, which opened by a small orifice into the male urethra.[3]

In a goat dissected by Mayer,[4] he found two testes with their epidymes fully developed, and vasa deferentia and vesiculæ seminales. ne of the testes was placed without, and the other still remained ithin the abdominal cavity. At the same time there were present a large female vagina communicating with the urethra, and double-horned uterus provided with two Fallopian tubes. One these tubes terminated in a blind canal, but the other had aced at its abdominal extremity several vesicles, resembling, acrding to Mayer, Graafian vesicles, or an imperfect ovary. The siculæ seminales and, through regurgitation by the urethra and aculatory ducts, the cavities of the vagina and uterus, were filled ith about four ounces of a whitish fluid, having the color and dor of male semen. This fluid could not be found by the microope to contain any seminal animalcules, but only simple and uble monades (*Monades termones et guttulas*). Bergmann, hower, is alleged to have found it, on analysis, to contain the same emical principle that characterizes human male semen.

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[1] For an account in the human subject of two additional cases of this variety of hermaphroditic malformation, viz., an imperfect male organization, with the addition of a highly developed uterus, see Weber's Zusätze zur Lehre der Geschlechtsorgane, p. 15; or Maret, in the Memoires de l'Acad. de Dijon, 1772, p. 157; and Chevreul, in Journal de Médecine for 1779, p. 441.

[2] See p. 229, under Lateral Hermaphroditism.

[3] Atti dell'Acad. delle Scienze di Siena, tom. viii. p. 201.

[4] Icones, p. 20.

Two testicles and two ovaries.—Various instances have now been ublished in which this sexual duplicity has been supposed to exist mong cattle and other domestic quadrupeds, as well as in the uman subject.

One of the free-martins[1] described by Mr. Hunter comes under is variety. In the case referred to, in the situation of the ovaries were placed," to use Mr. Hunter's words, "both the ovaria and esticles,"—or, as Sir Everard Home, in alluding to this case, more ustly expresses it, "an appearance like both testicles and ovaria as met with close together."[2] The two contiguous bodies were early of the same size, being each about as large as a small nuteg. There were no Fallopian tubes running to the ovaries, but a orn of an imperfect uterus passed on to them on each side along he broad ligament. Pervious vasa deferentia were found; they did ot, however, reach up completely to the testicle on either side, or orm epididymes. The vesiculæ seminales were present, and uch smaller than in the perfect bull. The external parts appear o have been those of the cow, but smaller than natural. The agina passed on, as in the cow, to the opening of the urethra, and, fter having received it and the orifices of the seminal ducts, it egan to contract into a small canal, which ran upwards through he uterus to the place of division of that organ into its two horns.

Velpeau,[3] in his work on Midwifery, mentions that in an embryo alf, he had "found reunited the testicles and ovaries, the vasa deerentia, and uterus."

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[1] An. Econ. pp. 63–64, pl. ix.

[2] Comp. Anat. vol. iii. p. 322.

[3] Traité de l'Art des Accouchemens, tom. i. p. 114.

In an hermaphroditic foal-ass, Mr. Hunter[1] found both what he considered to be two ovaries placed in the natural situation of these bodies, and two testicles lying in the inguinal rings in a process or theca of peritoneum similar to the tunica vaginalis communis in the male ass. No vasa deferentia or Fallopian tubes could be detected, but there was a double-horned uterus present, and from its broad ligaments, to the edges of which the cornua uteri and ovaries were attached, there passed down on either side into the inguinal rings a part similar to the round ligament in the female. The horns and fundus of the uterus were pervious; but its body and cervix and the canal of the vagina from above the opening of the urethra into it, were imperforate. The external parts were similar to those of the female ass; but the clitoris, which was placed within the entrance of the vagina, was much larger than that of a perfectly formed female; it measured about five inches. The animal had two nipples.

Scriba has given an account[2] of an hermaphroditic sheep, in which two large testicles are stated to have been found in the scrotum, at the same time that there existed, in their normal situation, two moderately sized ovaries, and a small uterus furnished with two apparently closed Fallopian tubes. The external sexual parts appear to have been those of a malformed male, the penis being short and impervious, the scrotum divided, and the urethra opening into a contracted perineal fissure resembling the female vulva. This animal had often attempted connection with the female sheep.

Borkhausen[3] has described a very similar case in the same species of animal. Each half of the divided scrotum contained a testicle, which was regularly formed, but greater in size than usual, and furnished with a large spermatic artery. The pelvis contained a normal uterus, which was smaller, however, than natural; it was provided with its usual ligaments. The Fallopian tubes were present, but imperforate, and the two ovaries were full of vesicles and enclosed in a strong membrane. The vagina was natural, and opened as in the female. Behind the divided scrotum, the rudiment of an udder, with four teats, instead of two, was situated. The male penis was also present, but diminutive and short; its erector muscles were small and the prostate gland indistinct. The urethra was single as it left the bladder, but it afterwards divided into two canals, the wider of which opened into the female vagina and vulva, and the narrower ran through the male penis. The urine passed in a full stream through the former canal, and only by drops through the tter. The animal is alleged to have attempted coition in both ays.

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[1] An. Econ. p. 58

[2] Schriften der Gesellschaft Naturforschender Freunde zu Berlin, Bd. x. s. 367.

[3] Rheinisches Mag. zur Erweiterung der Naturkunde, Giessen, 1793, Bd. i. s. 608.

In 1829, an account of an hermaphroditic goat was published at aples, which is said to have been provided with both female varies and male testicles.[1] The two ovaries occupied their usual ituation; no Fallopian tubes were found; but there were present double-horned uterus with blind cornua, and a vagina, which pened externally, as in the female. In the neighborhood of the varies, and more external than them, two small testicles were discovered, having two vasa deferentia arising from them. The vasa deferentia ran downwards to two corresponding vesiculæ seminales, that were placed alongside of the uterus. In the lower angle of the external pudenda, a body resembling in length the male penis more than the female clitoris, was situated: it was as we have already had frequently occasion to mention in regard to the penis in malformed male quadrupeds, of a very tortuous or convoluted form.

I have had an opportunity of examining an excellent preserved specimen of double hermaphroditism in the sow, referable to the present section, which was met with some years ago by Dr. Knox, and I have his permission to state here the following particulars of the case.

Among the internal female organs there is present a natural well-formed double uterus, provided with broad ligaments and two hollow cornua, each about six or seven inches in length. The fimbriated extremities are not distinctly marked, the female tubes appearing to end blind at their upper terminations, as they have often been observed to do in similar cases. The os uteri opens inferiorly into a vagina, which seems normal in its structure. At a short distance from the upper extremity of each horn of the uterus two bodies of considerable magnitude are seen lying in close juxtaposition. The smaller of these two bodies is on either side about the size and shape of a large almond; and though internally of an indeterminate amorphous structure, they are considered by Dr. Knox as answering to the two ovaries. The two larger bodies, which are placed *between* the supposed ovaries and the upper extremities of the cornua uteri, are most distinctly testicles, as shown by their numerous tortuous seminiferous tubes, which have been successfully filled with a mercurial injection. They are of the full size of the organ in the adult male. The seminiferous tubes of each testicle terminate in a vas deferens, which was injected from them; and the two vasa deferentia run downwards through the ligamenta lata of the uterus, and terminate inferiorly in the upper part of the vagina, thus following the course of those natural canals in the female sow that we shall afterwards have occasion to allude to at greater length, under the name of Gaertner's ducts, and which Dr. Knox, from the evidence of the present case, believes to be in reality typical of the male vasa deferentia. There is no trace of vesiculæ seminales. Externally, the vagina opened along with the urethra upon the perineum, at a point lower than natural in the well-formed female. The clitoris, in situation and size, was nearly normal.

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[1] Brevi cenni su di un Neutro Capro; or, Gurlt's Pathologischen Anatomie, Bd. ii. s. 198.

The animal at the time of death, was fourteen months old; it was ferocious in its habits; and it had been in vain tried to be fattened. It had repeatedly shown strong male propensities, and at the season of heat its vagina is said to have presented the usual injected appearance observed in the female sow.

Dr. Harlan, of Philadelphia,[1] has lately described a still more perfect case of double hermaphroditism than any of the preceding, which he met with in the body of a gibbon or orang-outang (*Simia concolor*) from the Island of Borneo. This animal died of tubercular disease in Philadelphia, in 1826, when it was considered to be under two years of age. Dr. Harlan gives the following account of its sexual formation. The penis (Fig. 31, *a*) was about one inch in length, and subject to erections; it terminated in an imperforate glans; and a deep groove on its inferior surface, served as a rudimentary urethra. This groove extended about two-thirds of the length of the penis, the remaining portion being covered with a thin reticular diaphanous membrane, which extended also across the vulva, *b*, and closed the external orifice of the vagina. The vagina was rather large, and displayed transverse striæ. Traces of the nymphæ and labia externa were visible. The meatus urinarius opened beneath the pubis into the vagina, but the urine must have been directed along the groove of the penis by the membrane obstructing the orifice of the vulva. The os tincæ was surrounded by small globular glands. The orifice and neck of the uterus admitted a large probe into the cavity of that organ, which appeared perfect with all its appendages. The round and broad ligaments, together with well-developed ovaries (Fig. 32, *b b*), were all found in situ. The scrotum (Fig. 31, *c*) was divided, and consisted of a sac on each side of the labia externa, at the base of the penis, covered with hair. The testicles (Fig. 31, *d d*) lay beneath the skin of the groin about two inches from the symphysis pubis, obliquely outwards and upwards; they appeared to be perfectly formed with the epididymis, *f f*, &c. The most accurate examination could not discover vesiculæ seminales; but an opening into the vagina, above the meatus urinarius, appeared to be the orifice of the vas deferens. In all other respects the male and female organs of generation were in this animal as completely perfected as could have been anticipated in so young an individual, and resembled those of other individuals of a similar age.

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[1] Med. and Phys. Researches, p. 19.

Fig. 31.

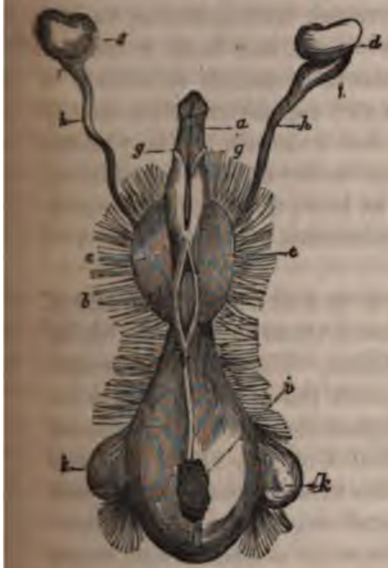
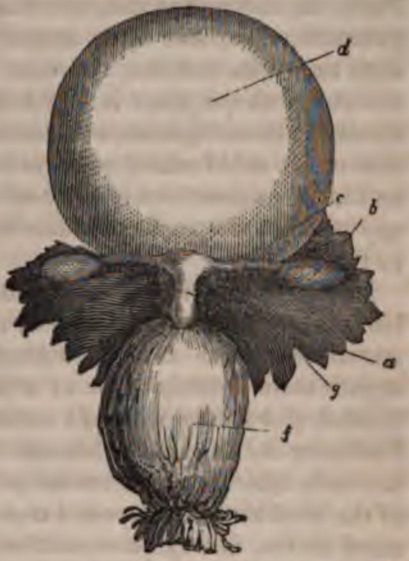


Fig. 32.



Four instances are now on record of the alleged coexistence of male testicles and female ovaries in the human subject. It is greatly to be regretted, that some of the instances have not been so accurately examined in their anatomical characters as their importance demanded.

a. The first of these cases is detailed by Schrell.[1] It occurred in an infant who died when nine months old. The testes and all the other internal and external male organs were present and perfectly formed, with the exception of the prepuce of the penis, which seemed divided in front and rolled up. At the root of the large penis was a small vulva or aperture, capable of admitting a pea, and provided with bodies having an appearance of labia nymphæ. This vulva led into a vagina that penetrated through the symphysis pubis, and terminated in a nipple-like body or imperfect uterus, to which structures having a resemblance to the Fallopian tubes and ovaries were attached.

b. A second and still more doubtful case of the alleged existence of both testicles and ovaries in the human subject, was first published

Fig. 31. External sexual organs and testicles; *g g*, the prepuce; *h h*, the vasa deferentia; *i*, the anus; *k k*, ischiatic protuberances.

Fig. 32. Internal sexual organs seen from behind; *d*, the urinary bladder; *f f*, rectum; *g g*, broad ligaments; *c c*, Fallopian tubes.

[1] Schenck's Medic. Chirurg. Archiv. Bd. i.

by Beclard.[1] The case was met with by M. Laumonier of Rouen, who injected and dissected the sexual parts, and deposited them in a dried state, along with a wax model representing them in their more recent condition, in the Museum of the School of Medicine at Paris. In the wax model, two female ovaries, with a uterus, vagina, external vulva, and large imperforate clitoris, are seen combined with two male testicles, the vasa deferentia of which terminate in the uterus at the place in which the round ligaments are normally situated; these ligaments themselves are wanting. The preparation of the dried sexual parts is far from being equally satisfactory, and in its present imperfect condition, at least, does not bear out by any means the complete double hermaphroditic structure delineated in the model.

c. Dr. Blackman has lately described and delineated[2] a case of hermaphroditic malformation in an adult human subject, which his statements place in the present division. The external genital organs were entirely those of a male, with the penis large and perforated, and the scrotum of a natural appearance, but without testes. Professor Ackley, of Cleveland Medical College, Ohio, dissected the body after death. Internally into the urethral canal, at the neck of the bladder, there opened a contracted vagina, which communicated above, through a normal os tincæ, with a well-formed female uterus. From the upper angles of the uterus there set off two perfect and permeable Fallopian tubes, which had at their fimbriated extremities two ovaries placed in their usual position. In addition there were found, according to Dr. Blackman's drawing and description, the following internal male organs, namely, two testicles, situated near the two ovaries, two vasa deferentia, and a prostate gland of the natural size and appearance. The vasa deferentia, or "excretory ducts of the testes, were perfect," to use Dr. Blackman's own words; but he omits to state how or where they terminated inferiorly. It is greatly to be regretted, also, that there is no account given of the internal structure of the two testes and two alleged ovaries—a defect which it is to be hoped, for the sake of science, that he or Professor Ackley will yet supply. The stature of this malformed being was large; the external configuration, with the exception of the hips, male; the beard moderate; the habits solitary, with a dislike to women; a discharge like menstruation occurred monthly, it is averred, by the urethra, and was attended with much suffering. Death occurred during one of these menstrual periods; and the vagina on dissection was found, according to Dr.

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[1] Bullet. de la Fac. de Méd. 1815, p. 284; or, Dict. des Sc. Méd. xxi. p. 111.

[2] American Journal of the Medical Sciences for July, 1853, p. 66.

Blackman, with "its inner surface reddened; and its cavity contained menstrual blood."

d. Perhaps, however, the most complete and most accurately detailed case of double hermaphroditism hitherto found in the human subject, is one dissected by Professor Vrolik of Amsterdam, in 1846.[1] In this remarkable instance, there were the following male and female organs present, viz., on the left side, and near each other, a testicle and an ovary; on the right side these and other parts were less perfectly developed; besides there existed two rudimentary Fallopian tubes, and two comparatively more perfect vasa deferentia. There was no vestige of a male prostate gland or vesiculæ seminales; but there was found a female uterus and vagina opening inferiorly beneath the arch of the pubis into a narrow uro-genital canal, which ended externally at the root of an impervious but sulcated penis; and the labia externa were united so as to form an apparently perfect male scrotum, the right side of which was enlarged by the presence of an inguinal hernia. The uterus was in its normal situation between the bladder and rectum, but drawn obliquely over to the left or more highly developed side. It was provided and fixed with the usual ligamenta lata, and behind with the ligaments of Douglas. The cavity of the uterus was mucous and smooth on its internal surface, and it ran downwards—without any formation or contraction like the os tincæ and cervix uteri—into the long and narrow vagina. The walls of the upper portion of the vaginal canal were thick, and its mucous surface plicated; inferiorly this canal became thinner and more contracted, especially where it terminated in the urethra or common uro-genital tube. From the angles of the fundus uteri imperfect Fallopian tubes arose, and ran forwards and outwards with the vasa deferentia, &c., through the inguinal canals, and were lost upon the tunicæ vaginales. On the right side the vessels, &c., running to the ovary and testicle, were, as has been already stated, very indistinct; but the left testis and left ovary were more developed, and carefully examined by Professor Vrolik. The testis on this side, though small in size, was provided with a large tunica vaginalis. When divided, the organ did not show the tubuli seminiferi, but its characteristic transverse septa were seen. In a glutinous yellow fluid taken from its divided tissue, the microscope detected small nucleated cells, but no true spermatozoa. The testis was provided with its usual bloodvessels. The vas deferens, after leaving it, became very tortuous for a part of its course, and then ran down by the side of the uterus and vagina, ultimately opening into the cavity of the latter. On the opposite and less developed side, it is worthy of remark that the more rudimentary right vas deferens opened into the angle of the uterus. Returning to the left ovary—this organ was found lying near the left testicle, and of an oblong form, and provided with its appropriate and characteristic *pampiniform* plexus of veins. The internal stroma of the ovary was, at the circumference of the organ, white, contracted, and indurated, as it is in advanced age, and no Graafian vesicles were visible. Further, near it was seen a congeries of tubes, analogous to the sketch given by Kobelt of the pro-ovarium. The subject of this malformation was 58 years old at the time of death. At 17 years of age, the testes began to descend through the inguinal rings, and a copious beard appeared upon the face. He had up to that time been considered a female, and he continued to dress as such till he was 38 years old. At that time, three medical men—of whom Professor Vrolik was one—examined him, and gave him a certificate that he was a hypospadiac or malformed male, in consequence of which he subsequently assumed the male attire—a conclusion which seemed to be further corroborated by the absence of menstruation, and the "magna in sexum sequiorem propensio." The autopsy showed, however, how difficult or indeed impossible it is, during life, to pronounce always decidedly and correctly on the actual sex of such malformed individuals.

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[1] Tabulæ ad illustrandum Embryogenesin Hominis et Mammalium, tab. xciv. xcv.

HERMAPHRODITISM AS MANIFESTED IN THE GENERAL CONFORMATION OF THE BODY, AND IN THE SECONDARY SEXUAL CHARACTERS, INFLUENCE OF OVARIES AND TESTES UPON THEM.

In the preceding observations we have principally confined ourselves to the description of hermaphroditic malformations as seen in the resemblance in appearance and structure of the external genital parts of one sex to those of the other, and in the different degrees and varieties of reunion and coexistence of the reproductive organs of the two sexes upon the body of the same individual. Hermaphroditism, however, may appear not only in what are termed the *primary* sexual parts or characters, or, in other words, in the organs more immediately subservient to copulation and reproduction, but it may present itself also in the *secondary* sexual characters, or in those distinctive peculiarities of the sexes that are found in other individual parts and functions of the economy, as well as in the system at large. We have occasionally an opportunity of observing some tendency to an hermaphroditic type in the general system, without there being any very marked corresponding abnormality in the sexual organs themselves, but it rarely happens that there exists any hermaphroditic malformation of the primary organs of generation, without there being connected with it more or less of an heraphroditic type in the secondary sexual characters; and this cirumstance often offers us, in individual doubtful cases, a new and rplexing source of fallacy in our attempts to determine the true or edominating sex of the malformed individual. Before, however, scribing that variety of hermaphroditism which manifests itself in e general system and in the secondary sexual peculiarities, it will necessary, in order to understand its nature and origin, to preise a few remarks on the dependence and relation of these condary characters upon the normal and abnormal conditions of e primary sexual organs.

That the various secondary sexual peculiarities which become eveloped at the term of puberty are intimately dependent upon the hanges that take place at the same period in the organism of the male ovaries and male testicles, seems proved by various consideations, particularly by the effect produced by original defective evelopment and acquired disease in these parts, and by the total emoval of them from the body by operation. In considering this oint, I shall speak first of the effects of the state of the ovaries upon he female constitution, and shall then consider those of the testicles pon the male.

When the usual development of the ovaries at the term of puberty oes not take place, the secondary sexual characters which are aturally evolved in the female at that period do not present themelves; and this deficiency sometimes occasions an approach in arious points to the male formation. Thus in a case recorded by Dr. Pears,[1] of a female who died of a pectoral affection at the age of wenty-nine, the ovaries on dissection were found rudimentary and ndistinct, and the uterus and Fallopian tubes were present, but as ittle developed as before puberty. This individual had never mentruated or showed any signs, either mental or corporeal, of puberty. The mammæ and nipples were as little developed as those of the ale subject. She had ceased to grow at ten years of age, and had ttained only the height of four feet six inches.

In another analogous instance observed by Renauldin,[2] scarcely ny rudiments of the ovaries existed, and the body of the uterus as absent, but the external genital female organs were well formed. he individual who was the subject of this defective sexual deveopment had never menstruated; the mammæ were not evolved; in tature she did not exceed three and a half French feet; and her tellect was imperfectly developed.

In reference to these and other similar instances that might be

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[1] Phil. Trans. for 1805, p. 225.

[2] Séances de l'Acad. Roy. de Méd. 28th February, 1826, and Medical Repository for 26, p. 78.

quoted,[1] it may be argued that they do not afford any direct evide
of the evolution of the sexual character of the female depend
upon that of the ovaries, as the arrestment in the development
both may be owing to some common cause which gives rise at
same time to the deficiency in the development of the genital org
and to the stoppage of the evolution of the body in general. T
the imperfection, however, in the organism of the ovaries may h
acted in such cases as the more immediate cause or precedent of
imperfection or non-appearance of the secondary characters of
sex, seems to be rendered not improbable, in regard to some, if
to all, of the instances alluded to, by the fact that the removal
these organs before the period of puberty, as is seen in spa
female animals, entails upon the individual so treated, the sa
neutral state of the general organization as was observed in
above instances; or, in other words, we have direct evidence t
the alleged effect is capable of being produced by the alleged cau
and further, when in cases of operation or disease after the perio
puberty, both ovaries have happened to be destroyed, and their
fluence upon the system consequently lost, the distinctive second
characteristics of the female have been also observed to disappea
a greater or less degree.

Thus in the well-known case recorded by Mr. Pott,[2] the cata
nia became suppressed, the mammæ disappeared, and the body
came thinner and more masculine, in a healthy and stout you
woman of twenty-three years of age, whose two ovaries formed h
nial tumors at the inguinal rings, and were, in consequence of th
incapacitating the patient from work, both removed by operatio

Many facts seem to show that the act of menstruation most p
bably depends upon some periodical changes in the ovaries,
rather in the Graafian vesicles of these organs or their contents;
when the function becomes suddenly and permanently stopped i
woman at the middle period of life, without any indications of
catamenial fluid being merely mechanically retained, we may perh
suspect with reasonable probability the existence of a diseased s
which has destroyed either successively or simultaneously the fu
tions of both ovaries. In such a case the distinctive secondary
culiarities of the female sex come to give place to those of the m
Thus Vaulevier mentions an instance in which menstruation s
denly ceased in a young and apparently healthy woman; no gen

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[1] Davis in his Principles and Practice of Obstetric Medicine, p. 513, refers to se
instances in point. We may mention that Dr. Haighton found that after the Fallopian
were divided in rabbits, the ovaries became gradually atrophied, and the sexual fee
were lost.—Phil. Trans. for 1787, p. 173.

[2] Surgical Works, vol. iii. p. 329.

or local disease followed; but soon afterwards a perfect beard began to grow upon her face.[1] Again, in women who have passed the period of their menstrual and child-bearing life, and in whom consequently the functions and often the healthy structure of the ovaries are lost or destroyed, we have frequently an opportunity of observing a similar tendency towards an assumption of some of the peculiarities of the male: an increase of hair often appears upon the face, the mammæ diminish in size, the voice becomes stronger and deeper toned, the elegance of the female form and contour of body is lost, and frequently the mind exhibits a more determined and masculine cast. Women, both young and aged, with this tendency to the male character, are repeatedly alluded to by the Roman authors under the name of *viragines;* and Hippocrates[2] has left us the description of two well-marked instances.

Among the females of the lower animals a similar approach to the male character in the general system not unfrequently shows itself as an effect both of disease and malformation of the sexual organs, and also in consequence of the cessation of the powers of reproduction in the course of advanced age. Female deer are sometimes observed to become provided at puberty with the horns of the stag,[3] and such animals are generally observed to be barren,[4] probably in consequence either of a congenital or acquired morbid condition of their ovaries or other reproductive organs. In old age, also, after the term of their reproductive life has ceased, female deer sometimes acquire the horns of the male in a more or less perfect degree;[5] and Burdach alleges that roes sometimes become provided with short horns when they are kept from the male during the rutting season, and at the same time furnished with abundant nourishment.[1] Mehlis[2] alludes to two cases in which a virilescent type was shown principally in the hair of the female deer. In one of these instances the hair of the head, neck, and abdomen, the shape of the ears and extremities, and the odor of the animal, gave it the closest resemblance to the male, and it followed the other females as if urged by sexual desire.

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[1] Journ. de Méd. tom. lxix. and Meckel in Reil's Arch., Bd. xi. s. 275. Meckel quotes other similar cases from Seger in Ephem. Nat. Cur. Dec. i. ann. ix. and x. obs. 95; Vicat, sur la Plique Polonaise, in Murray's Pr. Bibl. Bd. i. s. 578; and Schurig's Parthenologia, p. 184. Burlin published an express treatise on the subject, De Barbâ mulierum ex menstruorum suppressione, Altorf, 1664. See also Haller's Elem. Phys. tom. v. p. 32; Reuss, Repert. Comment. tom. x. p. 205; Eble, Die Lehre von den Haaren in der organischen Natur. Bd. ii. s. 222, Vienna, 1831; and Mehliss, Ueber Virilescenz und Rejuvenescenz thierischer Körper. Leipzic, 1838, who quotes several cases additional to those of Meckel.

[2] De Morb. Vulg. lib. vi. ss. 55, 56. "Abderis Phætusa, Pythei conjunx antea per juventam fœcunda erat; viro autem ejus exulante, diu menses defecerunt, ex quo postea dolores et rubores ad articulos exorti sunt. Quæ ubi contigerunt, tum corpus virile tum universum hirsutum est redditum, barbaque est enata, et vox aspera reddita. Sed cum omnia quæ ad menses deducendos facerent tentassemus non profluxerunt, verum haud ita multo post vita functa est. Idem quoque in Thaso Namysiæ Gorgippi conjugi contigit."—Hippocr. Op. Ed. Fœsii, p. 1201.

[3] Camden's Angl. Norm. 1603, p. 821. Langelot, Eph. Nat. Cur. Dec. i. ann. ix. and x. obs. 88. Ridinger's Abbild. Seltener Thiere, taf. 79, or Meckel in Reil's Archiv für die Physiol. Bd. xi. p. 273.

[4] Wildungen, Taschenbuch für Forst-und Jagd-freunde, s. 17.

[5] Otto's Path. Anat. by South, p. 166, s. 123, n. 18, for list of cases.

This kind of acquired hermaphroditism in aged females, has, however, been more frequently and carefully attended to as it occurs in Birds than as met with among the Mammalia, the change to virilescence in the former being more marked and striking than in the latter, owing to the great difference which generally exists between the plumage of the male and female.[3] When old female birds live for any considerable period after their ovaries have ceased to produce eggs, they are usually observed to assume gradually more or less of the plumage and voice, and sometimes the habits also of the male of their own species. This curious fact, first pointed out by Aristotle[4] in relation to the domestic fowl, has now been seen to occur in a number of other species of birds, but particularly among the Gallinaceæ. It has been in modern times remarked in the common fowl (*Phasianus gallus*) by Tucker, Butler, and Jameson; in the common pheasant (*P. colchicus*) by Hunter and Isidore St. Hilaire; in the golden pheasant (*P. pictus*) by Blumenbach and St. Hilaire; in the silver pheasant (*P. nyctemerus*) by Bechstein and St. Hilaire; in the turkey (*Meleagris*) by Bechstein; in the pea-hen (*Pavo*) by Hunter and Jameson; and in the partridge (*Tetrao perdrix*) by Montagu and Yarrell. Among the Cursores it is mentioned as having occurred in the bustard (*Otis*) by Tiedemann, and in the American pelican (*Platalea aiaia*) by Catesby. In the order Palmipedæ it has been observed by Tiedemann and Rumball in the domestic and wild duck (*Anas boscha*). Among the Scansores it has been seen in the cuckoo (*Cuculus canorus*) by Payraudeau; and among the Passeres in the cotinga (*Ampelis*) by Dufresne; in the chaffinch (*Fringilla*) and *rouge-queue* (*Motacilla*) by Prevost; and in the bunting (*Emberiza paradisea* and *longicauda*) by Blumenbach.

This change of plumage in old female birds commences, according to M. Isidore St. Hilaire,[5] much sooner in some instances than in

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[1] Phys. vol. i. § 183, p. 318.

[2] Ueber Virilescenz thierisch. Körper; or British and Foreign Medical Review, vol. vi. p. 77.

[3] It occurs also more frequently among birds than among mammalia, from the former possessing only a single ovary.

[4] "Gallini, cum vicerint gallos, concurrunt maresque imitandi subagitare conantur. Attollitur etiam crista ipsis, simul et clunes (uropygium); adeo ut jam non facile diagnoscantur an fœminæ sint. Quibusdam etiam calcaria parva surriguntur." Hist. Animal. lib. ix. cap. 36.

[5] Edinburgh Jour. of Philosoph. Science. 1826. p. 308.

rs; it may only begin to show itself several years after the bird
eased to lay, though depending more or less directly upon this
omenon, and sometimes it commences immediately after it.
change may be effected in a single season, though in general
not complete for some years. When it is perfected, the female
display not only the variety of colors, but also the brilliancy,
he male plumage, which it sometimes resembles even in its
mental appendages, as in the acquisition of spurs, and, in the
estic fowls, of the comb and wattles of the cock. The voice of
bird is also very generally changed. Its female habits and in-
ts are likewise often lost; and in some instances, it has been
to assume in a great degree those of the male, and has even
observed to attempt coition with other females of its own
ies.[1] In most of the female birds that have undergone this
ge, the ovary has been found entirely or partially degenerated,
gh in a few cases the morbid alteration is not very marked, eggs
ng even been present in the organ in one or two instances. In
ral, however, it is greatly diminished in size, or has become
gether atrophied; but the perfection of the change in the plumage
not seem to bear any direct ratio with the degree of morbid
ation and atrophy in the ovary.

hat the change towards the male type, described as occasionally
rring in old female birds, is directly dependent, not upon their
but upon the state of their ovaries, seems still further proved
imilar changes being sometimes observed in these females long
ious to the natural cessation of the powers of reproduction, in
equence of their ovaries having become wasted or destroyed by
ase. Greve,[2] in his Fragments of Comparative Anatomy and
siology, states that hens whose ovaries are scirrhous, crow
etimes like cocks, acquire tail-feathers resembling those of the
e, and become furnished with large spurs. The same author
tions also the case of a duck, which, from being previously
thy suddenly acquired the voice of the male, and on dissection
se ovary was found hard, cartilaginous, and in part ossified.

r. Yarrell, in a paper read before the Royal Society in 1827,[3]
stated that in a number of instances he has observed young
le pheasants with plumage more or less resembling the male,

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mball, in Home's Comparative Anatomy, vol. iii. p. 330, states having observed an
ck, which had assumed the male plumage, attempt sexual connection with another
. This may perhaps enable us to understand the reputed cases of hermaphroditism
men, who, as related by Mollerus (Tract. de Hermaphr. cap. ii.) and Blancard (Collect.
o-Phys. cent. iii. obs. 80), after having themselves borne children became addicted to
urse with other females. Of course we cannot give our credence to the alleged suc-
l issue of such intercourse

[2] Bruchstücke zur vergleich. Anat. und Physiol. s. 45.

il. Trans. for 1827, part ii. p. 268.

and in all of them he found on dissection the ovaries in a very morbid state, and the oviduct diseased throughout its whole length, with its canal obliterated at its upper part. He also shows that a similar effect upon the secondary sexual characters of the female bird is produced by the artificial division and removal of a small portion of their oviduct in the operation of making capons of female poultry; and he states that his investigations have led him to believe that in all animals bearing external characters indicative of the sex, these characters will undergo a change and exhibit an appearance intermediate between the perfect male and female, wherever the system is deprived of the influence of the true sexual organs, whether from original malformation, acquired disease, or artificial obliteration.[1]

From the frequency with which castration is performed, the effects of the testicles in evolving the general sexual peculiarities of the male have been more accurately ascertained than those of the ovaries upon the female constitution. These effects vary according to the age at which the removal of the testicles takes place. When an animal is castrated some time before it has reached the term of puberty, the distinctive characters of the male are in general never developed; and the total absence of these characters, together with the softness and relaxation of their tissues, the contour of their form, the tone of their voice, and their want of masculine energy and vigor, assimilate them more, in appearance and habits, to the female than to the male type. If the testicles are removed nearer the period of puberty, or at any time after that term has occurred, and when the various male sexual peculiarities have been already developed, the effect is seldom so striking; the sexual instincts of the animal, and the energy of character which these instincts impart, are certainly more or less completely destroyed, and the tone of the voice is sometimes changed to that of puberty; but the general male characteristics of form, such as the beard in man, and the horns in the Ruminantia, most commonly continue to grow. In animals, such as the stag, which possess deciduous horns, the removal of the testicles during the rutting season causes the existing horns to be permanent; and if the operation is performed in an adult animal when out of heat, no new horns in general appear.[1] In the ox, the effect of castration upon the growth of the horns, even when performed before the time of puberty, is quite remarkable; for instead of having their development altogether stopped, or their size at least diminished by the operation, as occurs in the ram and stag, the volume of these appendages is even increased by it, the horns of the ox being generally larger but less strong than those of the entire bull. Castration in the boar causes, according to Greve,[2] the tusks to remain small, and prevents altogether the replacement of the teeth. This author also states that the same operation on the horse prevents the full development of the neck, renders the teeth smaller and slower in their growth, increases the growth of the hair, and the size of the horny protuberances on the inside of the legs. The prostate gland, he further alleges, as well as the vesiculæ seminales, become augmented as much as a third in their volume in consequence of the operation.[3]

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[1] On old or diseased female birds assuming the plumage, &c., of the male, see J. Hunter, Observ. on the An. Econ. p. 75; E. Home, Lect. on Comp. Anat. vol. iii. p. 329; Mauduit, in Encycl. Method. Art. *Faisan*, tom. ii. p. 3; Butter, in Wernerian Soc. Mem. vol. iii. p. 183; Schneider's Notes, in his edition of the Emperor Frederick the Second's Treatise "De Arte, Venandi cum Avibus;" Tucker's Ornithologia Damnoniensis; Catesby's Natural History of Carolina, &c., i. t. 1; Bechstein, Naturgeschichte der Deutschlands, Bd. ii. sec. 116; Blumenbach, De Anomalis et vitiosis quibusdam nisûs formativi aberrationibus, p. 8, and Instit. of Physiology, p. 369; Payrandeau, Bull. des Sc. Nat. tom xiii. p. 243; Tiedemann. Zoologie, tom. iii. p. 306; Geoff. St. Hilaire, Phil. Anat. tom. ii. p. 360; Isid. St. Hilaire, Mém. du Mus. d'Hist. Nat. tom. xii. p. 220; Annal. des Sc Nat. tom. vii. p. 336, or Edinburgh New Philosophical Journal, for 1826, p. 302, with additional cases by Professor Jameson, p. 309; Kob, De mutatione sexûs, p. 11, Berlin, 1823; Yarrell, Phil. Trans. for 1827, p. 268, with a drawing of the diseased ovaries, &c.

The removal of the testicles both before and after the period of puberty commonly gives rise to another singular effect—to an increased deposition of fat over the body,[4] and from this circumstance the general form of the body, and in man that of the mammæ, is sometimes modified in a degree that increases the resemblance to the opposite sex. In the sterile of both sexes in the human subject an unusual corpulency is not uncommon, and the same state is often met with in old persons, and particularly in females, after the period of their child-bearing life is past.

The nature of the effects produced by the existence and functional activity of the testicles and ovaries upon the development of the secondary sexual characters of the male and female, may be further illustrated by what occurs in the season of heat to animals such as the deer, sheep, birds, &c., that have periodical returns of the sexual propensity. At these periods all the distinctive general characters of the sexes become much more prominently developed in conjunction with, and apparently in consequence of, the changes which have been ascertained by observation to occur at that time in the relative size and activity of the internal organs of generation. Thus with the return of the season of sexual instinct, the dorsal crests and cutaneous ear-lobes of tritons enlarge; in Batrachian Reptiles the spongy inflations of the thumbs become increased in size; the various species of singing birds reacquire their vocal powers; and some, as the cuckoo and quail, appear capable of exercising their voice only at this period of the year. At the pairing season also the plumage of birds becomes brighter in tint, and in some instances is in other respects considerably changed, as in the male ruff (*Tringa pugnax*), which then reassumes the tuft of feathers upon his head and neck, and the red tubercles upon his face, that had fallen off during the moulting, and thus left him more nearly allied in appearance to the female during the winter. In reference to this subject, it appears to us interesting to remark, that in certain birds, as in the different species of the genus Fringilla, the male presents in winter a plumage very similar to that of the female,[1] and in the present inquiry it is important to connect this fact with the very diminutive size and inactive condition of the testicles of these birds at that season.

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[1] Buffon. Hist. Nat. tom. vi. p. 80.

[2] Bruchstücke zur Vergl. Anat. und Physiol. p. 41.

[3] Ibid. p. 45.

[4] See Cyclop. of Anat and Phys., Art. ADIPOSE TISSUE.

From the remarks that we have now made upon the influence of the ovaries and testicles in developing the general sexual peculiarities of the female and male, it will be easy to conceive that when, in cases of malformation of the external genital organs giving rise to the idea of hermaphroditism, there is at the same time, as sometimes happens, a simultaneous want of development in the internal organs of reproduction, particularly in the ovaries and testicles, the general physical and moral peculiarities distinctive of the sex of the individual may be equally deficient, or have a tendency even to approach in more or fewer of their points to those of the opposite sexual type. In this way we may, it is obvious, have *general* or constitutional hermaphroditic characters, if they may be so termed, added to those already existing in the *special* organs of generation, and rendering more difficult and complicated the determination of the true sex of the malformed individual. Some cases of spurious hermaphroditism in the male, published by Sir E. Home,[2] may serve to illustrate this remark.

A marine soldier, aged twenty-three, was admitted a patient into the Royal Naval Hospital at Plymouth. He had been there only a few days, when a suspicion arose of his being a woman, which induced Sir Everard to examine into the circumstances. He proved to have no beard; his breasts were fully as large as those of a woman at that age; he was inclined to be corpulent; his skin was uncommonly soft for a man; his hands were fat and short, and his thighs and legs very much like those of a woman; the quantity of fat upon the os pubis resembled the mons veneris; and in addition he was weak in his intellect, and deficient in bodily strength. The external genital organs showed him to be a male, but the penis was unusually small, as well as short, and not liable to erection; the testicles were not larger in size than they commonly are in the fœtal state; and he had never felt any passion for the opposite sex.

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[1] Stark's Elements of Nat. Hist. vol. i. p. 243.

[2] Comp. Anat. vol. iii. p. 320.

The following cases by the same author strongly illustrate this subject.[1] In a family of three children residing near Modbury in Devonshire, the second, a daughter, was a well-formed female, the eldest and youngest were both malformed males. The eldest was thirteen years of age. His mons veneris was loaded with fat; no penis could be said to be present, but there was a præputium a sixth of an inch long, and under it the meatus urinarius, but no vagina. There was an imperfect scrotum with a smooth surface, there being no raphé in the middle, but, in its place, an indented line; it contained two testicles, of the size met with in the fœtus. His breasts were as large as those of a fat woman. He was four feet high, and of an uncommon bulk, his body round the waist being equal to that of a fat man, and his thighs and legs in proportion. He was very dull and heavy, and almost an idiot, but could walk and talk; he began to walk when a year and a half old. The younger brother was six years old, and uncommonly fat and large for his age. He was more an idiot than the other, not having sense enough to learn to walk, although his limbs were not defective.

A case in a similar manner confirmatory of the preceding remarks is mentioned by Itard de Riez.[2] A young man, aged twenty-three, had no testes in the scrotum, a very small penis, not capable of erection, and a divided scrotum. He was in stature below the middle size. His skin was soft, smooth, and entirely free from hair, the place of the beard being supplied by a slight down. The voice was hoarse; the muscles were not well marked; the form of the chest resembled that of the female, and the pelvis was extremely broad and large. The intellectual faculties were very dull, and the sexual appetite was entirely wanting.

Renauldin, also, in the same work,[3] has recorded another case in point. In a soldier of twenty-four years of age, whose genital organs were extremely undeveloped, his penis being only of the size of a small tubercle, and his testicles not larger than small nuts, the pelvis was broad; the chest narrow; the face and body in general were not covered with hair, with the exception of a small quantity upon the pubis; the voice was feminine, and the mammary glands were as perfectly developed as in the adult female. The body of this individual was rather lean than otherwise. The mammæ had begun to enlarge when his body attained to its full stature at sixteen years of age. He had all the habits and sexual desires of the male sex.

In quadrupeds, as in man, when the testicles or ovaries are imperfectly formed, the secondary sexual peculiarities are frequently so defectively evolved as to offer a kind of hermaphroditic or neutral type in the general configuration and characters of the animal. Thus, the free-martin does not present an exact analogy in form either with the bull or cow, but exhibits a set of characters intermediate between both, and more nearly resembling those of the ox and of the spayed heifer. In size it resembles the castrated male and spayed female, being considerably larger than either the bull or the cow, and having horns very similar to those of the ox. Its bellow is similar to that of the ox, being more analogous to that of the cow than of the bull. Its flesh, like that of the ox and spayed heifer, is generally much finer in its fibre than the flesh of either the bull or cow, and is supposed to exceed even that of the ox and heifer in delicacy of flavor.[1]

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[1] Comp. Anat. pp. 320–21.

[2] Mémoires de la Société Méd. d'Emulation, tom. iii. pp. 293–95.

[3] Ibid. tom. i. p. 241.

The consideration of the various facts that we have now stated, inclines us to believe that the natural-history characters of any species of animal are certainly not to be sought for solely in the system of the male or in that of the female; but, as Mr. Hunter pointed out, they are to be found in those properties that are common to both sexes, and which we have occasionally seen combined together by nature upon the bodies of an unnatural hermaphrodite, or evolved from the interference of art upon a castrated male or spayed female. In assuming at the age of puberty the distinctive secondary peculiarities of his sex, the male, so far as regards these secondary peculiarities, evidently passes into a higher degree of development than the female, and leaves her more in possession of those characters that are common to the young of both sexes, and which he himself never loses when his testicles are early removed. These and other facts connected with the evolution of both the primary and secondary peculiarities of the sexes further appear to us to show that, physiologically at least, we ought to consider the male type of organization to be the more perfect as respects the individual, and the female the more perfect as respects the species. Hence we find that, when females are malformed in the sexual parts so as to resemble the male, the malformation is almost always one of excessive development, as enlargement of the clitoris, union of the labia, &c.; and, on the other hand, when the male organs are malformed in such a manner as to simulate the female, the abnormal appearance is generally capable of being traced to a defect of development, such as the want of closure of the perineal fissure, and of the inferior part of the urethra, diminutive size of the penis, retention of the testicles in the abdomen, &c. In the same way, when the assumes the secondary characters of the male, it is either, hen by original malformation its own ovaries and sexual are so defective in structure as not to be capable of taking a the function of reproduction, and of exercising that influ- er the general organization which this faculty imparts to or, secondly, when in the course of age the ovaries have to be capable of performing the action allotted to them in roductive process. In both of these cases we observe the of the female organization, now that its capabilities of per- its particular office in the continuation of the species are or lost, expending themselves in perfecting its own indi- ystem; and hence the animal gradually assumes more or f those secondary sexual characters that belong to the male. lo not consider it subversive of the preceding view to qualify the two following admissions—1st, that, owing to the ener- the female system being so strongly and constantly directed the reproductive organs, and the accomplishment of those im- functions which these organs have to perform in the economy pecies, the general characters of the species may be developed ody in a degree *less* than they otherwise would be, or than ac- onstitutes the proper standard of the species; and, 2dly, that, equence of the peculiarities of the sexual functions of the some of the individual organs of her system, as the mammæ, lved in a degree *greater* than is consonant with the standard ers of the species. At the same time we would here remark occasional enlarged condition of the mammæ in herma- es in whom the male sexual type of structure predominates e examples of spurious male hermaphrodites that have been from Sir E. Home, and in those mentioned by Renauldin, Petit, Rullier, and others, in the human subject, as well as rous cases among hermaphrodite quadrupeds), would almost show that the full development of the mammary glands is cter proper to the species in general, rather than one pecu- he female system alone. In males, also, who are perfect in productive organs and functions, the mammæ are sometimes d to be developed in so complete a manner as to be capable eting milk, forming what may be regarded as one of the t approaches towards hermaphroditic malformation in the rganization;[1] and the mammæ of the infants of both sexes requently contain a lactiform fluid at birth.

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[1] Hunter's Obs. on the An. Econ. p. 60.

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ecretion of milk in the mammary glands of the male is occasionally observed ur domestic quadrupeds. See Gurlt's Pathologischen Anatomie der Haus-Saug- . ii. s. 188; Blumenbach in the Hanoverisch Magazin for 1787; and Home in at. iii. p. 328. Among the recorded instances and observations upon it in man efer to Paullini, Cynographia, p. 52; Schacher, De Lacte Virorum et Virginum, 42; Sinnibaldus, Geneanthrop. tom. iv. p. 456; Alex. Benedictus, Anatom. Corp.

LATERAL HERMAPHRODITISM, ETC., IN THE GENERAL OR SECONDARY SEXUAL CHARACTERS OF THE BODY.

In some instances of hermaphroditic malformation, the total form and configuration of the body have been alleged to present not only a general tendency towards the physical secondary characters of the opposite sex, or to exhibit in a permanent state the neutral condition existing before puberty, but different individual parts of it have been occasionally conceived to be developed after a different sexual type. Thus, for instance, we have already mentioned in regard to Hubert Jean Pierre, that the upper half of the body of this individual seemed formed after the female, and the lower half after the male type, the larynx and mammæ being quite feminine, the face showing no appearance of beard, and the arms being delicate and finely rounded, while the pelvis was narrow, and the thighs were marked and angled as in man. In a case described by Schneider,[1] the reverse held true, the bust being male, with a strong beard, large thorax, and the pelvis being large and distinctly female. A more mixed combination of the secondary sexual characters has been already described as existing in the cases detailed by Ricco, Mayer, Arnaud, Bouillaud, &c., &c.

One side of the body has been sometimes observed to be apparently formed in one or more of its parts on a sexual type different from that of the same parts on the opposite side. Girald, in his Topography of Ireland,[2] mentions a reputed female, who had the right side of the face bearded like that of a man, and the left smooth like that of a woman. Mr. King[3] has described an interesting instance of hermaphroditic malformation in an individual whose general character was masculine, but with the pelvis large and wide; the left testicle only had descended into the groin, and the mamma of this side was small comparatively to that of the opposite or right side.

In a hind mentioned by Mr. Hay,[4] and which, he believed, had never produced any young, one of the ovaries on dissection after death was found to be scirrhous. The animal had one horn resembling that of a three-years-old stag on the same side with the diseased ovary; there was no horn on the opposite side. Bomare[5] has given

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Hum. lib. iii. p. 595; Winslow, Anatomy, vol. ii. p. 214; Deusing, De Lacte, p. 327; Kyper, Anthropologia, lib. i. p. 490; Buffon, Hist. Nat. tom. ii. p. 543; Bishop of Cork, Phil. Trans. vol. xli. p. 813; Humboldt, Personal Narrative, vol. iii. p. 57; Franklin, First Expedition to the Polar Seas, London, 1823, p. 157.

[1] Kopp's Jahrbuch der Staatsarzneikunde, Bd. x. s. 134.

[2] Topog. Hiberniæ, in Camden's Angl., &c., 1603, part ii. p. 724.

[3] London Medical Repository for 1820, vol. xiii. p. 87.

[4] Linnæan Transactions, vol. iii. p. 356.

[5] Journ. de Phys. tom. vi. p. 506.

ar case in the same animal, where a single horn was present, d also on the same side with the diseased and degenerated and Russell[1] states, as the result of his experiments on castra- the deer, that when he removed one testicle only from the , the horn on the opposite side was the more completely ped of the two. Azara[2] observed in two birds the right side tail to possess the characters of the male, and the left those of ale.

e hermaphroditic lobster previously alluded to as described holls, the general external configuration of the body was, like the sexual organs, perfectly female on one side, and perfectly n the other.

principally, however, among hermaphroditic insects that a ce of sexual type in the general conformation of the opposite f the body, and of its individual parts, has been observed; s malformation is the more striking and easy of observation class of animals, on account of the great differences in color, d form respectively presented by the antennæ, wings, and parts of the body of the males and females of the same .

ral hermaphroditism of the body in Insects has been most tly observed by entomologists amongst the class Lepidoptera. now been remarked in the following species:—In the *Argyn- hia*, *Lycæna alexis*, *Saturnia pyri*, *Endromis versicolor*, and *vinula* (Ochsenheimer); in the *Gastrophaga medicaginis* and *adonis* (Rudolphi); in the *Liparis dispar* (Shaefer, Ochsen- , and Rudolphi); in the *Saturnia Carpini* (Capieux, Ochsen- , and Rudolphi); in the *Gastrophaga quercifolia* (Hettlinger, dolphi); in the *Gastrophaga pini* (Scopoli); in the *Gastrophaga* (Esper); in the *Sphinx convolvuli* (Ernst); *Sphinx populi* r and Westwood); *Papilio polycaon* (Macleay); *Polyommatus* Entomolog. Mag. vol. iii. p. 304); *Bombyx castrensis* (Duval); *Argynnis paphia* (Allis); in the *Vanessa atalanta* (Schrank and r); and in the *Vanessa antiopha* and *Deilephila euphorbiæ* r). Klug and Germar have recorded two instances of it the Coleoptera, the former in the *Lucanus cervus*, and the n the *Melolontha vulgaris;* and Mr. Westwood mentions a se in the large water-beetle (*Dytiscus marginalis*), as contained Hope's collection, and has seen a fourth in the stag-beetle *us cervus*).

of twenty-nine recorded cases of lateral hermaphroditism in , in which the sexual characters of each side are distinctly

[1] Economy of Nature in Glandular Diseases.

[2] Kob's Dissert. de Mutatione Sexûs, p. 19.

specified, I find that in seventeen instances the right side was male, and in twelve female. Burmeister alleges that in by far the majority of cases the right side is male, and the left female—a statement in which Meckel coincides, while Westwood maintains the reverse. The cases I have myself collected are certainly numerically in favor of the former opinion, but the data are as yet so few, and the difference so trifling, as not to warrant any decided conclusion on this point.

In some instances we find among insects an *imperfect* lateral hermaphroditism consisting of some parts of one side, as of one or more of the wings, palpi, or antennæ, being formed according to a different sexual type from the same parts of the opposite side, and from the general body of the animal. Thus in the *Melitæa* described and dissected by Klug (see page 229), the general form of the insect was male, but the left eye, palpus, antenna, and left sexual fang were smaller than in individuals belonging to this sex; the left antenna was annulated with white and yellow at the apex, while the right was of one color; the general form of the abdomen was male, but somewhat thick, and the wings were all equal and male.

In a *Pontia duplidice* mentioned by Rudolphi, and which in its general external characters was female, the right anterior wing was formed after the male type, and the sexual organs also resembled those of the male.

Ochsenheimer mentions a *Gastrophaga quercus* with the body and the antennæ and wings on the left side female, and the right wings male; and a second with the body and the right side female, and the left side and two antennæ male, the latter being brown and pectinated.

In this imperfect variety of lateral hermaphroditism, the malformed wing, antenna, or palpus is sometimes formed after one sexual type and colored after another. In a male *Melitæa phœbe*, noticed by Germar, the right wings and antenna were female in regard to size, but male in respect to coloring and markings. In a female *Deilephila galii* he found the left antenna and palpus of the small size of the male, but agreeing in coloring and markings with the corresponding female parts on the right side. In a *Pontia cardamines*, which was male in all its other characters, Ochsenheimer observed the right superior wing marked as in the female, and he mentions another individual of the same species which had a female form with some male colors.

In another variety of insect hermaphroditism the sexual difference is sometimes, as we have already noticed in regard to the human subject, expressed not by a lateral, but by a longitudinal sexual antagonism, or, in other words, the anterior and posterior parts of the body are formed after the two opposite sexual types. Thus in a *Saturnia* *rrpini* described by Ochsenheimer, the antennæ were male, the uperior wings male in form, but colored as in the female, and the osterior wings, with the exception of a reddish-brown spot upon he left, were, with the body and other parts, female.

Lastly, in a third variety of external hermaphroditic conformation n Insects, we find the characters of the two sexes mixed up and crossed in different irregular combinations upon the body of the same individual. In a *Gastrophaga castrensis* described by Rudolphi, and where the male type predominated, with a tendency, however, in all parts to the female form, the right antenna and the wings on the opposite or left side were distinctly female, while the left antenna and right wings were entirely male, the latter being only somewhat larger than in male insects, and the colors brighter than in the female. In a *Bombyx castrensis* alluded to by Westwood, the wings on the right side, and the antennæ and abdomen of the left, were those of a male, while the left wing, right antennæ, and right side of the abdomen were those of a female.

GENERAL SUMMARY WITH REGARD TO THE NATURE OF THE VARIETIES OF SPURIOUS HERMAPHRODITISM.

On some of the varieties of spurious hermaphroditism it is unnecessary for us to dwell here. For instance, the first species of spurious male hermaphroditism, or that arising from extroversion of the urinary bladder, is acknowledged by all to be merely one of the many varieties of arrested development or conjunction in the median line of the body, and so need not detain us. Two other forms, namely, the second female species, consisting of prolapsus of the uterus, and the second male, consisting of an adhesion of the penis to the scrotum, seem both referable to the head rather of disease than of original malformation. This latter indeed appears in all probability only an effect or result of adhesive inflammatory action in the affected parts during embryonic or fœtal life. Both of the two remaining forms of spurious hermaphroditism, viz., those consisting of hypospadiac fissure of the urethra, scrotum, and perineum in the male, and of abnormal magnitude of the clitoris in the female—seem readily explicable upon the doctrine of arrestment and abnormality in the development of the malformed parts.

We have already described at sufficient length the process of development of the different copulative organs, and have shown that those various degrees of hypospadiac malformation which constitute the common form of spurious hermaphroditism in the male, may be traced to arrestment of this process at various periods or stages of its progress. And we may here remark that the earlier this arrestment occurs, the distinction of the true sexual type of the malformed organs will always be the less marked, because the younger the embryo—and, on a similar principle, the lower we descend in the scale of animal existence—we find the differences between the organs of the two sexes proportionately the less pronounced, until at last we arrive at that primitive type in which these organs present altogether a common, neutral, or intermediate character.

We have also already shown that at a certain early stage of the development of the female organs, the female clitoris holds the same, or nearly the same, relatively larger size to the whole embryo as the penis of the male, and that so far we may consider the occasional occurrence of spurious hermaphroditism from magnitude of the clitoris, and its resemblance in this respect to the male organ, as a permanent condition of a type of embryonic structure that is normally of a temporary or transitory existence only. But besides this permanence of the embryonic type of the clitoris, we must farther, in all the more complete instances of spurious female hermaphroditism, admit an excess of development in the malformed external sexual parts, and more particularly in the line of the median reunion of the two primitive lateral halves or divisions of these parts. In this way the vulvar orifice of the vagina (a remnant in the female of the primitive perineal cleft or fissure) is often in such cases more or less contracted and closed, so much so indeed in some instances as to leave only, as in the male, a small canal common to the genital and urinary passages. If the median junction is extended still farther, this canal comes also to imitate the male urethra in this respect, that it is united or shut up *below* in such a way as to be carried onward to a greater or less length, and in a more or less perfect condition along the under surface of the enlarged clitoris; and occasionally, as in some of the cases we have previously described, the male type of structure is still more completely repeated in the female organization by the median reunion of the two labia, giving the appearance of the united scrotum and closed perineum of the opposite sex.

If we divide the whole sexual apparatus of the male and female into three corresponding transverse spheres or segments—the first or deep parts including the testicles and ovaries, the second or median comprehending the male seminal canals and prostate gland, and the female oviducts and uterus, and the third or external embracing the copulating organs of the two sexes—we shall find that, relatively speaking, the deep and the external spheres are naturally most developed in the male economy, while the median, comprising the uterus (the principal and most active organ in the female reproductive system), is developed in the greatest degree in that sex. In malformed females presenting a spurious hermaphroic character, this important portion of the female sexual organiza- a is, in general, either itself in some respects malformed, or, m the structure of the other parts of the sexual apparatus being perfect, its specific importance in the economy is cancelled, and refore the energy of development takes the same direction as in male, being expended upon the more complete evolution of the ans of the external and deep spheres. Hence the greater size the clitoris, and the greater development which we have just w pointed out, in the median line of reunion of the external ual parts; and hence also the occasional though rare occurrence, the same cases, of the descent of the ovaries through the ingui- rings into the labia—an anomaly that certainly consists in a true ess of development, and which we cannot but regard as interest- , both in this respect, and as affording a new point of analogy ween these organs themselves and the male testicles.

here is another and equally interesting point of view in which may look upon this subject. Not only are the forms of spurious maphroditism which we have been considering capable of being ced backward to certain transitory types of sexual structure in embryos of those animal species in which the malformations in stion occur, but they may be shown also to present in their normal states repetitions of some of the normal and permanent ditions of the sexual organs in various species of animate beings ced lower in the scale of life. Thus the occasionally imperforate is of the male hermaphrodite has been supposed to have an ana- ue in the naturally solid penis of some of the species of the genera *ridium* and *Hyalæa.*[1] Its more or less grooved or hypospadiac dition is similar to the natural type of the same part in some maphrodite Mollusca, as in the *Planorbis* and *Murex:*[2] in its asional diminutive size it approaches the general smallness of partially fissured penis of most birds and reptiles; and we find in the Rodentia and Marsupiala tied down by a short prepuce a way analogous to what is seen in some cases of severe hypo- dias. In the sloth (*Bradypus tridactylus*) the penis is small and oved in its lower surface, and has the urethra opening at its e;[3] and in several of the male Rodentia the scrotum is also cleft, d has its two opposed surfaces smooth, humid, and free from ir, as in most cases of hypospadiac hermaphroditism in man. In hidian and in most Saurian Reptiles, the male seminal ducts en at once externally, as in some male hermaphrodites, at the t of the fissured penis.

Burdach's Physiologie, Bd. i. § 132, p. 231.

Tiedemann's Zeitschrift für Physiologie, Bd. i. s. 15, or Cuvier, Anat. Comp. tom. v. p. 182.

[3] Meckel, Beiträge zur vergleichenden Anatomie, Bd. ii. cap. i. p. 125.

The fact of the testicle sometimes remaining, in cases of hei phroditic formation in the human subject, within the cavity oi abdomen, presents to us in a permanent state their original changeable position in the early fœtus, and at the same time afi a repetition of their normal situation in almost all the lower ti of animals, and in the Cetacea, Amphibia, Edentata, and s Pachydermata, as the Cape Marmot (*Hyrax*), and Elephant, an the Mammalia.

The malformed clitoris in instances of spurious hermaphrodi assumes also, in its abnormal state, types of structure that we as the normal condition of the organ in various inferior anin Thus in female Cetacea and Rodentia, and in the animals incl in Cuvier's order of Carnassiers, but more particularly among Quadrumana, the clitoris retains as its permanent normal type relatively larger size which we observe in the early fœtus, an female hermaphrodites, in the human subject; and further, s sometimes seen in such malformed individuals, the clitoris becc partially traversed by the urethra, as in the Ostrich, Emu,[1] Anteater;[2] and in the Loris, as we have noticed in a prece page, and Maki, it is completely enclosed, like that of the mal the body of the organ, forming a continuous and perfect c through it.

We may here further observe, though the illustrations sh more properly belong to the next section, that in cases of hermaphroditism also in man and quadrupeds, as well as in above spurious varieties, there may often be traced in some port of the abnormal structures a sexual type bearing a greater or less logy to the corresponding parts of those inferior animals that naturally androgynous. Thus, in instances of true hermaphrodit the orifices of the sexual ducts or passages occasionally open in common cavity, as is normally the case in some species of *Dorid* *Helix*, and other Mollusca; or the female oviducts or Fallo tubes, and the male vasa deferentia, run closely alongside of other without any communication between their canals, as in *Alypsia* and most Gasteropoda. Indeed the occasional coexist even of both testicles and ovaries in individuals among the hi animals would be only a repetition of, or retrogression to, the no sexual type of those genera of animals that we have just named, of the Planaria, Cestoidea, and other natural hermaphrodites.

In this way we see, that, as in many other monstrosities, several varieties of malformation in the sexual organs occurrin spurious human hermaphroditism do not consist of the substitu an entirely new and anomalous type of structure, but are only petitions of certain types of the same organs that are to be met ith, both in the human fœtus and in the inferior orders of animate ings. The investigation of the whole subject shows us in refe-nce to the sexual organs, what is equally true in regard to all the her organs of the body—that their different stages of development the embryos of man and of the higher orders of animals corre-ond to different stages of their development in the series of ani-ate beings taken as a whole; so that here, as elsewhere, the facts f Comparative Anatomy are reproduced in those of Embryology, nd both are repeated to us by nature on a magnified scale in the natomy of the malformations of the part—a circumstance amply stifying to the intimate relations which exist between Compa-ative Anatomy, the anatomy of Embryonic Development, and hat of Monstrosities. Indeed, proportionately as our knowledge f malformations has increased, it has shown us only the more trongly that the laws of formation and malformation—of normal nd abnormal development, are the same, or at least that they differ uch more in degree than in essence, and that the study of each is alculated reciprocally to illustrate and to be illustrated by the tudy of the other.

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[1] Cuvier, Anat. Comp. tom. v. p. 129.

[2] Meckel, Archiv für die Physiologie, Bd. v. s. 66.

REMARKS ON THE NATURE OF TRUE HERMAPHRODITIC MALFORMATIONS, UNITY OF SEXUAL TYPE, MALE UTERUS, ETC.

Of the nature and origin of local malformations by duplicity, we t present possess much less precise knowledge than of those of imple defect or simple excess of development; but there are certain facts ascertained with regard to the formation of the internal exual organs which may enable us to make an approach at least to ccurate ideas of the character and origin of those abnormalities that constitute the several varieties of true hermaphroditism. These facts relate to the interesting subject of the unity of organization, or common plan of structure, which is manifested in the corresponding male and female reproductive organs of the human subject, and of other species of bisexual animals.[1]

By several of the Greek, Roman, and Arabian physiologists[2] the respective organizations of the two sexes were considered as in some degree typical of one another, the female being regarded as an inverted male, with the testicles and penis turned inwards to form the ovaries and uterus. This doctrine of analogy between the male and female sexual organs has, with various modifications, been very generally admitted by modern physiologists, and in some of its bearings it has been made, more particularly of late years, the subject of considerable discussion. The testicles, or, more correctly speaking, the bodies of the testicles, are still regarded as organs which correspond with the ovaries in their original situation, in their vascular and nervous connections, and in their relative sexual functions. The recent progress of the anatomy of the development of the embryo has also shown that the two organs correspond in their primitive origin. It is now well ascertained that the large masses occupying each side of the abdomen of the embryo at an early stage of development, and which Rathke has named the Wolffian bodies, after their illustrious discoverer, form, in Birds and Mammalia at least, the primordial matrices upon which the urinary and genital organs are developed. On the inner side of each of these matrices a small body is early developed, which seems to become afterwards either a testicle or an ovary, according to the particular ulterior sexual type which the embryo assumes. According to the testimony of Valentin, no difference in appearance or structure can be detected at their earlier periods of formation and development, between the testes and ovaries.[1]

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[1] It is right to state that the following observations on the pro-ovarium, prostatic vesicle, &c., and the subsequent deductions from them, have been added during the revisal of this essay for the present work—these being subjects that have only come to be discussed since the essay was originally published in 1839. Three preceding cases—those of Vrolik, ckman, and Banon—have also been added, with references in the notes to some others corded, like them, during the last ten or fifteen years. In other respects the essay stands most entirely as it was originally printed in the Cyclopædia of Anatomy.—(Ed.)

[2] Aristotle, Hist. An. lib. i. 17; Galen, De Semine, lib. ii., and De Usu Partium, c. i.; Rhases, De Re Medicâ, lib. i. cap. 26; Avicenna, De Membris Generat. lib. iii. 21, &c.

If this were a fit opportunity for discussing, in all its details, the unity of type between the male and female reproductive organs, it would be easy to prove further the justness of those greater analogies that we have mentioned between the ovaries and the proper bodies of the testes, by pointing out other numerous minor, but still strong, points of correspondence manifested in the abnormal conditions and localities of these two representative organs in the higher animals and in their conformity of structure in some of the lower. Thus among Insects, in the genus *Libellula*, the long cylindrical testes of the males correspond with the long-shaped ovaries of the females; in the *Locusta* and *Gryllotalpa*, there are ramose bunched testicles with analogous fasciculated ovaries; in the *Lamellicornia*, we find compound radiating and united testes, with similar radiating and united ovaries; and sometimes, as in the genera *Melolontha* and *Trichius*, the number of the single bodies in the testicles corresponds with the number of the oviducts.[2]

Organ of Rosenmuller; Pro-ovarium of Kobelt; Canals of Gaertner, &c.—In further following out the unity of type between the genital organs of the two sexes, the epididymes of the male has usually been compared to the infundibula or fimbriated extremities of the Fallopian tubes of the female, and the seminal ducts, or vasa deferentia, of the Fallopian tubes themselves. This view, however, has been considerably changed in some important respects, by the later investigations of embryologists and anatomists, and particularly by the observations of Kobelt and others, upon the transformations and relations of the two Wolffian bodies and their ducts. We have already alluded, in the preceding paragraph, to the Corpora Wolffiana as two large abdominal bodies in the early embryo, on the inner side of which the glandular organs constituting the future testes or ovaries, are first observed in the study of development. These Corpora Wolffiana are very large in the embryo of some of the lower animals, as in reptiles and birds. In the early Mammalian embryo, the two Wolffian bodies are oblong masses, placed one on each side of the spine, and stretching from the vicinity of the heart to the lower end of the abdomen. They send off two excretory tubes, the Wolffian ducts, which open below into the cloaca, or latterly into that division of the cloaca which forms the uro-genital sinus or common genito-urinary canal. Each Wolffian body consists, when fully developed, of a congeries of transverse cæcal tubes, all of them opening into the common excretory or efferent duct, which passes along the outer side of the organ. Most anatomists have described the Wolffian bodies as temporary embryonic glands, and altogether transitory structures only. "In both sexes," according to the statement of Müller, in 1840, "the Wolffian bodies *entirely* disappear, and are not converted into any other organ."[1] The investigations, however, of Kobelt[2] have latterly proved, that this view was erroneous, and that we can still trace remains of the Wolffian bodies in the adult human female, and often, also, in the adult male.

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[1] Entwickelungsgeschichte, p. 391.

[2] Burmeister's Entomology, § 154, p. 222.

Professor Rosenmuller of Leipsic, published, in 1802, a remarkable essay,[3] in which he pointed out the existence in the human female fœtus and infant, of a peculiar fan-like structure, formed of diverging ducts, and lying in the duplicature of peritoneum connecting the ovary to the outer or fimbriated portion of the Fallopian tube. He did not appear to have searched for this canaliculated "Corpus Conicum," as he terms it, in any subjects older than two years of age.[1] Kobelt has demonstrated, however, that this body or organ of Rosenmuller exists in the same locality in adult females, namely, between the Fallopian tube and ovary, enclosed between the serous coats or folds of the Alæ Vespertilionis ;[2] and he has given to it the name of "Pro-ovarium." He has shown also, by the evidence of the history of this organ or structure, from embryonic to adult life, and by its position and anatomical characters, that it consists in reality of the remains of that Wolffian body which is so very large, and apparently so very important a structure, in early embryonic life. Rosenmuller hints that the "corpus conicum ejusdem ductus,"[3] which he discovered in the female fœtus, may have an analogy to the epididymis and vas deferens of the male ; and Kobelt has tried to prove that in man, the Wolffian body so far remains, that it has gone to constitute, in great part, the epididymis ; the cul-de-sacs or blind tubes of the former being transformed into the coni vasculosi of the latter body, and the inferior cœca of the Corpus Wolffianium being converted into the so-called vasa aberrantia of Haller. In both the adult female and male, some of the higher tubes or cœca of the Wolffian bodies often remain, and become expanded into those cysts which form the sessile and pediculated hydatoid bodies that are so very frequently, or, indeed, constantly, found in the upper part of the broad ligament, and on the fimbriate edges of the Fallopian tubes, and also on and beneath the surface of the male epididymis.[4]

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[1] Müller's Elements of Physiology, by Dr. Baly, p. 1637. The same is expressed by M. Coste in the Comptes Rendus for 1839, p. 331.

[2] Der Neben-Eierstock des Weibes, &c., Heidelberg, 1847.

[3] Quaedam de Ovariis Embryonum et Foetuum Humanorum. An excellent plate illustrates the essay. He thinks Roederer and Trew were probably acquainted with the structure which he describes, p. 8.

According, then, to this modern view of the subject, we ought, in reference to the doctrine of the unity of structure between the two sexes, to regard the epididymis of the male as having its true analogue in the pro-ovarium of the female, and not in the fimbriated extremity or infundibulum of the Fallopian tube.

The female Fallopian tubes and male vasa deferentia were formerly, as has been already stated, very generally considered as typical or analogous structures in the two sexes. Muller,[5] however, and

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[1] At p. 14, loc. cit., he tells us he found this special structure in an infant who died when twelve days old, "admodum magnum, constare e multis canaliculis in basi *corporis conoidi* inter se convolutis et latioribis." In this specimen he reckoned the canals, or canaliculi, as amounting to about twenty in number.

[2] See also the excellent diagram of its structure and position in H. Meckel's Morphologie der Geschlechtszeuge, 1848. Tab. ii. fig. 21, &c.

[3] Quaedam de Ovariis, &c. p. 15.

[4] Rosenmuller delineates (pl. i. figs. 6 and 7), a hydatoid body or "corpusculum sacciforme," attached to the edge of the broad ligament, and connected with the body he has described. Follin and Verneuil have latterly called the special attention of the profession in France to the cysts formed out of the persistent tubes of the Wolffian bodies. See Follin's Recherches sur le Corps de Wolff, published as a Thesis in 1851 ; and Verneuil's Recherches sur les Kystes de l'Organe de Wolff dans les deux Sexes, in the Mémoires de la Société de Chirurgie, tom. iv. p. 58.

[5] Bildungsgeschichte der Genitalien, 1830. Elements of Physiology, vol. ii. p. 1637. Muller still continued to hold, however, that in Mammalia, both the Fallopian tube in the

e[1] first pointed out that in birds and reptiles these two sets of
were developed separately and distinctly from each other in
rly embryo. Bergmann,[2] Leuckhart,[3] and more especially
t,[4] have latterly shown that the same fact is true with regard to
rly history of development of these parts in the mammalian
o and in the human subject. Vrolik[5] and Valentin[6] have
hed the same morphological views; and I know that their
tness is maintained by Professor Allen Thomson, undoubtedly
eatest authority on such a question in this country. From
searches of these and other observers, it seems now fully
l that in the early human and mammalian embryo, in both
two hollow filaments or tubes on each side pass from the re-
f the testes or ovaries above to the uro-genital sinus or canal
These two sets of tubes form at that period the organic link
nection between what latterly become the true reproductive
; namely, the ovaries or testicles and the external organs of
tion. One of these pairs of tubes on each side, consists of the
ry duct of the Wolffian body, and this pair in the course of
pment becomes transformed in the male fœtus into the two
asa deferentia. Of these Wolffian ducts or ultimate vasa de-
a, the upper portion on each side is ultimately united in the
ubject to the secreting structure of the testicles by tubes that
part the remains of the Wolffian body itself, and which form
ididymis. On the other hand, in the female embryos of most
alia, these Wolffian ducts entirely disappear, or, as in female
antia and Pachydermata, they remain as rudimentary or
ect ducts, lying between the layers of the ligamenta lata, and
g from the organ of Rosenmuller, or persistent Wolffian body,
to the sides of the uterus and vagina, and open into the latter,
g, in fact, the canals of Gaertner in the cow, sow, &c. The
pair of tubes—the cords of Müller, seen in the early embryo
h sexes running by the sides of those already mentioned,
go two very different transformations in the male and female.
female, they remain open at their upper extremity and de-
, and come to constitute the canals of the Fallopian tubes and
nfundibula or fimbriated extremities. On the contrary, in
bryo of the male human subject, and of most other mam-

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nd the vas deferens in the male, were on each side formed out of a single distinct
, having originally the appearance of a filament running along the outer border of
ian body.

[1] Entwickelungsgeschichte der Natter, 1839, p. 212.

Wagner's Handworterbuch, &c.

Anatomie und Physiologie des Geschlechtsorgane, 1847, p. 90.

Neben-Eierstock des Weibes, &c. 1847.

lae ad Illustrandum Embryogenesin Hominis, &c., Tab. xcv.

book of Physiology, p. 661.

malia, they early disappear, particularly in their upper portion. In the male beaver, rabbit, goat, &c., they remain as rudimentary extensions of the horns of the organ we shall presently describe, namely, the male utriculus bicornis of these animals. Their lower portions may be considered as forming, or being transformed by their coalescence into, the male utriculus itself, in those species of animals, as in man, where this male utriculus or prostatic vesicle is single in form, and median in position.

Male utriculus ; Uterus Masculinus.—Formerly great diversity of opinion prevailed as to the morphological prototype or analogue of the female uterus in the male genital system. Some anatomists, as Burdach, Steghlener, and Blainville, regarded the uterus and male vesiculæ seminales as corresponding parts ; while others, as Meckel, Carus, Schmidt, Ackermann, and Serres, compared the uterus to the male prostate. Instances of malformation on record appeared to favor both opinions, and there were other cases which almost inclined anatomists to believe that the vesiculæ seminales correspond to the fundus or body of the uterus in the human subject, and to the cornua uteri in quadrupeds ; while the prostate represented in the male structure the lower portion or cervix of the same organ.

M. Geoffroy St. Hilaire divided the uterus of the human subject into the body, and the upper part or fundus, the latter corresponding to what constitutes the cornua uteri in the human embryo, and in adult quadrupeds. Further, believing that in the determination of all analogies in type and structure between different organs, the origin and course of the bloodvessels supplying the part ought to be our principal criterion, he was led by the study of the distribution of the branches of the hypogastric arteries, to consider the body of the uterus and the vesiculæ seminales as repetitions of each other in the two sexes ; and contrary to the opinion of most anatomists, he conceived that the male vasa deferentia strictly correspond with the fundus or cornua uteri, and that the epididymis represents a coiled-up Fallopian tube, or, in other words, that the Fallopian tube is an unrolled epididymis.[1]

The latter investigations, however, of Professor Weber of Leipsic[2] and others, have thrown a new and most important light upon this question in morphological anatomy, by demonstrating that in man and in other males there exists, distinct both from the vesiculæ seminales and prostatic gland, a small rudimentary uterus—a true representative, in the unity of organization between the two sexes, of the more highly developed uterus of the female subject.

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[1] Phil. Anat. 1822, tom. i. p. 471.

[2] Zusätze zur Lehre vom Baue und den Verrichtungen der Geschlechtsorgane, Leipsic, 1846.

Early in the last century, Morgagni described in the region of the caput gallinaginis in the male urethra, a small sac or cell. He named it the Sinus Pocularis,[1] and has given two excellent and illustrative drawings of its situation, orifice, and cavity. He found this cell or vesicle in fourteen out of fifteen human subjects that he dissected. Of late years, since the attention of anatomists has been specially recalled to this part by Weber, Huschke, Leuckhart, and others, it has received a variety of appellations, as that of *prostatic vesicle*, *prostatic utricle*, *uterus masculinus*, &c.

In man this utricular body is a small oblong cul-de-sac or hollow, flask-shaped vesicle, situated in the space or angle between the lower ends of the ejaculatory ducts, and opening below by a narrow neck and orifice upon the posterior wall of the urethra, at the anterior edge of the caput gallinaginis or verumontanum. In man, its fundus is imbedded between the lobes of the prostate gland, but the lower portion of it, projecting as a narrow ridge on the middle and lower surface of the prostatic portion of the urethra, constitutes the bulk of the caput gallinaginis. The normal site of its opening into the urethra is, in the human subject and in most mammalia, at a point intermediate between the two orifices of the seminal or ejaculatory ducts. Sometimes, though rarely, as in two of Morgagni's human subjects, it opens into one of the ejaculatory tubes themselves; and this, indeed, appears to be its normal structure in some animals, as in the hare. In man, the length of the cavity of the prostatic vesicle varies from three to six lines; at its upper extremity its breadth is generally two lines; but it sometimes attains a larger size. The lining membrane of its cavity is covered with small mucous glands, packed closely together. This male utriculus or prostatic vesicle is of much larger size, and in a state of greater development, in the males of some of the lower mammalia; and it was principally from studying its various forms and relations in these animals that Weber discovered the true morphological character of the prostatic vesicle, and its typical analogy with the female uterus.

In the embryos of the sheep and sow, at a certain period of development, the uterus or prostatic vesicle of the male, is so very like the uterus of the female, that, according to Rathke,[2] the two organs cannot be distinguished from each other. In the adults of some male animals this utriculus, or male uterus, is an organ of considerable size, and presents much of the form and relations of the female uterus in the same species. For instance, in the rabbit it is an organ from one to one and a half inches in length, projecting behind, between the rectum and urinary bladder, and with its fundus divided and stretched out laterally into two short cornua, like those of the female uterus. The long uterus masculinus of the male beaver is single at the cervix, and split or divided above into two horns, like the female uterus in the same species; and, like it, too, the male organ is provided with ligamenta lata, &c. In the goat its cornua often stretch to the testicles. In some adult animals the male uterus, however, is even less developed than it is in the human subject; and it seems to vary greatly even in different individuals of the same species.

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[1] Adversaria Anatomica, iv. 1723, p. 6, tab. figs. 1, 2. It was previously figured by Albinus in his Annotat. Academ. iv. tab. iii. fig. 3; and by Cowper, in his work entitled Glandularum nuper detect. Descriptio, i. 3.

[2] Weber's Zusätze, &c., p. 65. See his comparative sketches of the male and female uterus, in these animals, pl. v. fig. 4.

Few, or indeed none, of the eminent anatomists who have in later years studied the subject of the prostatic vesicle or utricle, as Huschke, Leydig, Rathke, Leuckhart, Bischoff, Arnold, Wahlgrew, Kölliker, Duvernoy, Goodsir, and Allen Thomson, have at all doubted that this organ is a representative or analogue in the male organization of the genital canals of the female.[1] But different opinions have been expressed as to whether it morphologically represents the vagina, or the uterus, or both. H. Meckel[2] at one time, and in opposition to almost all other authorities, suggested and maintained that it was the analogue of the vagina rather than of the uterus; Weber considered it as the male prototype of the female uterus; and still more lately, Bimbaum[3] and Leuckhart[4] have shown that this organ may be more truly held as the morphological equivalent of the whole sinus genitalis, both the uterus and the vagina—an opinion now generally shared in by those who formerly took a different view of the subject. Huschke has sometimes found the lower or vaginal portion of the male utriculus separated from the upper and dilated end by a constricted point, as if indicating its division normally into uterus and vagina.[5] Indeed, it is only in accordance with this last doctrine that we can understand the relative positions, and modes of junction, of the genital and urinary canals in some monstrosities, and the fact of the great variety of forms and shapes which the male uterus or prostatic vesicle assumes, when it is found—as so often happens—preternaturally enlarged and disproportionately developed in different kinds of hypospadiac and hermaphroditic malformation.

[1] Some of the various diseased states attributed to enlargement, &c., of the third lobe of the prostate gland will yet be found, I believe, to be morbid states of this prostatic vesicle. To the minds of some, the investigation of "the diseases of the Male Uterus" would appear to be almost a paradox in thought and words.

[2] Zur Morphologie der Geschlechtswerkzeuge, p. 47.

[3] Beschreibung einer Bildungshemming, p. 15.

[4] Cyclopædia of Anatomy and Physiology, vol. iv. p. 1427.

[5] Soemmering's Lehre von den Eingeweiden, p. 409.

The only remaining internal organs of generation requiring consideration under the present head, are the prostate gland and vesiculæ seminales of the male. Formerly, as we have already seen, these male structures were, in different points of view, usually looked upon as prototypes of the female uterus, or the two were held as analogues, one of the neck, and the other of the body and fundus, or cornua, of that important organ in the female economy. The discovery of the existence of a true uterus masculinus, and the investigation of its morphological import and relations, have entirely dispelled these views. At present, the typical analogues in the female, of the prostate gland and vesiculæ seminales in the male, are, in consequence, by no means fixed and established, and some authorities are inclined to look upon these male organs as peculiarly and entirely male structures, that have not any true morphological equivalents representing them in the construction of the female. In their original embryonic origin, as well as in their ultimate adult relations, the vesiculæ seminales seem to be parts and appendages of the male vasa deferentia; and perhaps, morphologically, they ought to be considered merely as highly and peculiarly developed portions of the lower end of the male seminal tubes, and consequently of the analogous ducts of Wolff and Gaertner. The scattered and imperfect form which they present in some hermaphroditic malformations, as in the cases described by Ackermann and Steghlener,[1] seems to accord with this view. The vesiculæ seminales distinctly and essentially belong to the sexual canals and structures; but the prostate gland seems more truly an appendage to the urinary tubes and urinary organization. Generally, its excretory ducts open, and its secretions are discharged, into the urethral canal somewhat before the male seminal tubes enter into, or join with, that canal. And if we are to find a true prototype in the female, of the prostate gland of the male, we shall probably detect it in the follicular glands and structures that exist so abundantly in the course, and at the extremity, of the female urethra—a canal which, throughout its length, is truly an analogue of the posterior prostatic and membranous portion only of the male urethra.

In the observations made under the present head, I have spoken only of the analogous organs traceable in the *internal* sexual organization of the male and female. Already, in preceding sections, the analogous organs and structures observed in the external genital construction of the two sexes has been discussed. If we now attempt to show, in an abridged and tabular form, the unity or common plan of structure between the different parts and organs belonging to the

[1] See these cases at page 257, ante.

whole sexual organization of the male and female—the morphological prototypes or equivalents in each may, with the explanations and reservations previously stated, be briefly represented as follows:

In the Male.		*In the Female.*
Bodies of testes,	=	Ovaries.
Epididymes,	=	Pro-ovaria and bodies of Wolff and Rosenmuller.
Vasa deferentia and their vesicular dilatations,	=	Ducts of Wolffian bodies or canals of Gaertner.
Cornua of male utriculus,	=	Fallopian tubes and their infundibula.
Male or prostatic utriculus,	=	Female uterus and vagina.
Gubernaculum testis,	=	Round ligament.
Tunica vaginalis,	=	Process of Nuck.
Cowper's glands and their ducts opening into bulb of urethra,	=	Glands of Bartholin, and their ducts opening at the roots of nymphæ.
Urethra behind bulb and prostate gland,	=	Whole urethra and its follicular glands.
Bulb of urethra,	=	Vulva or vulvar end of vagina.
Penis, and its corpora cavernosa, ligaments, &c.,	=	Clitoris, and its corpora cavernosa, ligaments, &c.
Prepuce and integumental covering of penis,	=	Præputium clitoridis, and nymphæ or labia minora.
Glans penis and corpus spongiosum,	=	Vascular nodule of clitoris and the erectile bulbi vestibuli.
Sides of scrotum,	=	Labia majora.
Perineal closure of genital sinus,	=	Hymen.

In relation to these equivalent or analogous parts in the two sexes, as given in the preceding table, it is scarcely necessary to observe that some of them—as the cornua of the male utricle on the male side, and the ducts of Wolff or Gaertner, on the female side—are structures pertaining to the *embryonic* condition only, and not normally persistent or traceable in the organization of the extra-uterine being; though their morphological prototypes are fully developed in the adults of the opposite sex. Further, I would here beg to add the following, as a general remark or law which seems to me to explain many of the hermaphroditic anomalies and sexual combinations of structure that we meet with, that whenever we find the development of the sexual organs of an individual, whether male or female, restrained or arrested, so as to be *below* the normal type or standard, some of the analogous or prototypic organs of the opposite sex will in this same individual be observed to be developed in excess, or *above* their normal type or standard.

Some other organs, besides those we have referred to, and that are, so far as regards their functions, peculiar and essential to one sex only, are nevertheless found to be repeated in the opposite sex in the form of an analogous rudimentary type of structure. Thus, in the male, we may observe the unity of sexual structure maintained in the presence of the rudiments of the mammary gland, which is *functionally* an organ of the female system only. In the human subject, and in animals whose females have pectoral mammæ, these organs occupy the same position in the male; while in those species of quadrupeds in which they are placed in the inguinal region, we find them in the corresponding males forming the scrotum or bags for containing the testicles. Hence, as we have already seen, the testicles, in cases of malformation in these animals, are often laid upon or imbedded in the udder. In the same way in the Marsupiata, the bone which the female has for supporting the marsupium is repeated in the organization of the male, although in the latter we cannot conceive it to serve any possible use.[1]

In the female also we observe in some points a similar disposition to the rudimentary repetition of parts that are essential or peculiar only to the male organization, as in the repetition in the clitoris of some female Rodentia, of the penis-bone of the male,[2] and in the formation of rudimentary forms of those processes of peritoneum which constitute the tunicæ vaginales. We are ourselves inclined also to regard, as was seen in the preceding table, the common crescentic form of the hymen of the human female in the same light,[3] and to consider it merely as an abortive attempt at that closure of the sinus genitalis or perineal fissure which we have already described as effected at an early period in the male embryo—an opinion in which we conceive we are borne out both by the history of the development, and the study of the malformations, of the external sexual parts in the female.

M. Isidore St. Hilaire read, in 1833, a memoir to the French Academy,[4] in which, following up the doctrine of his father with regard to the determination and distinction of the type of parts by the particular vessels distributed to them, he endeavored to show

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[1] Home's Lect. on Comp. Anat. vol. ii. pl. v.

[2] We have already, when considering spurious hermaphroditism in the female, mentioned several facts illustrative of the analogical peculiarities in structure between the male penis and female clitoris in some species of animals; and Burmeister, who regards the ovipositors and stings of female insects as corresponding to the clitoris in the female Vertebrata, has pointed out a remarkable conformity of structural type between its valves and those of the penis of the male of the same species.

[3] Burdach (Phys. s. 137), considers the small cutaneous fold situated at the orifices of the vasa deferentia, and Stiebel, the membrane placed at the extremity of the urethra (Meckel's Archiv für Physiol. Bd. viii. s. 207), as the analogue in the male for the female hymen.

[4] Arch. Gén. de. Méd. 1833, tom. i. p. 306.

some new points of analogy between the male and female organs, and to develope new views with regard to the origin and particular varieties of hermaphroditic malformations. With Burdach he divides the whole reproductive apparatus of either sex into three transverse spheres and into six portions or segments in all, or three on each side, viz., 1 and 2, the deep organs, including the male testicles and female ovaries; 3 and 4, the middle organs, or male prostate and vesiculæ seminales, and female uterus; 5 and 6, the external organs, comprehending the penis and scrotum, of the male, and the clitoris and vulva of the female. Each of these portions or segments is, M. St. Hilaire points out, supplied by an arterial trunk peculiar to itself, and the corresponding organs of the male and female by corresponding arterial branches, as the deep organs of both sexes by the two spermatics, the middle by branches of the two hypogastrics, and the external by some other hypogastric branches, and by the external pudics. This circumstance, he conceives, renders all the segments in a certain degree independent of the others, both as regards their development and existence, and allows of the occasional evolution of any one or more of them on a type of sexual structure, different from that upon which the others are formed in the same individual.

Though assuredly we cannot subscribe to the speculations of the elder St. Hilaire, that the development in the embryo, of male testicles or female ovaries, and consequently the whole determination of the sex, is originally regulated by the mere relative angle at which the first two branches of the spermatic arteries come off, and the kind of course which they follow[1] (more particularly as it is admitted by most physiologists that the bloodvessels grow, not from their larger trunks or branches towards their smaller, but from their capillary extremities towards their larger branches), yet we believe that the doctrine of the comparative independence of the different segments of the reproductive organs pointed out by the son is in its general principles correct. At the same time we would here remark that we conceive the doctrine would have been founded more on truth if the influence of the nervous branches supplying the different reproductive organs had been taken into account along with that of their arterial vessels, because, as we shall point out when speaking of the causes of hermaphroditism, there appears to be some connection between the state of the nervous system and the degree or condition of sexual development.

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[1] Anat. Phil. tom. i. p. 359.—"L'ordre de variations des sexes tient à la position d'un artère. . . . . Le plus ou le moins d'ecartement des deux branches spermatiques motive effectivement cette preference. Que les deux branches de l'artère spermatique descendent parallelement et de compagnie, cette circonstance, je le repete, cette circonstance donne le sexe male; qu'elles s'ecartent à leur point de partage, nous avons le sexe femelle."

The consideration of the preceding analogies in structure between the male and female organs is interesting in itself, and, so far as relates to our present subject, important in this respect, that it enables us to understand how it happens, that, without any actual monstrous *duplicity* we should sometimes find, in an organization essentially male, one or more of the genital organs absent, and replaced by an imperfect or neutral organ, or by the corresponding or prototypic organ of the opposite sex, and vice versa. Hence there is no difficulty in conceiving that, in the body of the same individual, the primitive structural elements of these parts should occasionally, in one or more points or segments, take on, in the process of development, a different sexual type from that which they assume at other points. Indeed, some physiologists, as we shall immediately see, deny that the most complete hermaphroditic malformations ever consist of anything except such a want of conformity between the sexual type of different portions of the reproductive apparatus.

If each of the six segments—and we believe that their number might be shown to be really greater than this—is thus an independent centre of development in the formation of the sexual apparatus, and is consequently liable also in abnormal cases to have its own particular malformations, and to assume, either alone or along with some of the other segments, a sexual type different from the remainder, it is evident that we may have as many varieties of true hermaphroditism, without any real duplicity, as it is possible to conceive differences of arrangement among these six segments. Again, however, one or more of these segments may preserve from development its original indeterminate or neutral sexual type, while the others are variously formed either upon one or upon both sexual types; or one or more of the segments may have evolved within them the prototypic or analogous organs of the two sexes, as the vasa deferentia, and cornua uteri; and if we consider the different arrangements of double and single sexual parts that might thus occur in the six separate segments, we may gain some idea of the great diversities of structure in the sexual parts that are liable to be met with in instances of true hermaphroditism.

This doctrine forms, as it appears to us, the most sound and rational solution of the nature and origin of many forms of true hermaphroditism which physiological science is capable of affording, with our present limited knowledge of the laws of development; and its application to the explanation of the different varieties of *Lateral*, *Transverse*, and *Vertical* hermaphroditism is so obvious as only to require to be alluded to. It offers to us, however, no insight into the probable origin of those varieties of double hermaphroditism in which there is an actual coexistence, upon one or upon both sides of the body, or, in other words, in the same segment of the sexual apparatus, of such corresponding or analogous male and female organs as the testes and ovaries. We can only refer all such instances to the laws which regulate the occasional production of *local* duplicities in different other organs of single bodies, and at the same time confess our present ignorance of what these laws are. We know that various individual muscles, nerves, &c., are not unfrequently found double, and that in internal organs of the body examples of duplicity in individual viscera are occasionally, though rarely, observed in the heart, tongue, trachea, œsophagus, intestinal canal, &c. In the several organs composing the reproductive apparatus, instances of similar duplicity would seem to be even more common than among any other of the viscera. Examples of *three* mammæ upon the same person are mentioned by Bartholin,[1] Borelli,[2] Lanzoni,[3] Drejer,[4] Robert,[5] Petrequin,[6] and others;[7] and cases in which the number of these organs was increased to *four* have been recorded by Faber,[8] Gardeux,[9] Cabroli,[10] Lamy,[11] Tiedemann,[12] Champion,[13] Sinclair,[14] R. Lee,[15] and Moore.[16] An instance in which even *five* mammæ existed upon the same woman is reported to have been seen by Gorré.[17] Valentin[18] and Gunther[19] have recorded supposed cases of duplicity in the male penis; and Arnaud has related an example of an analogous malformation in the female clitoris. Weber[21] met with a double vesicula seminalis on each side; and Hunter[22] alludes to the occasional occurrence of an imperfect supernumerary vas deferens. In 1833, a case of a double human uterus, furnished with four Fallopian tubes and four ovaries, was shown by Professor Moureau to the Académie de Médecine.[23] Blasius[24] dissected the body of a man on whom he detected the coexistence of three testicles; the additional testicle was of the natural form and size, and was furnished with a spermatic artery and vein that joined in the usual manner the aorta and vena cava; it lay in the right side of the scrotum. Arnaud found, on dissection, three testicles in a ng; the third was placed in the abdomen, and of the natural consistence, figure, and size; it was furnished with a vas deferens.[1] Other instances of triple and quadruple testicles of a more doubtful character, inasmuch as the observations made during life were not confirmed by dissection after death, are related by Voigtel,[2] Sibern,[3] Brown,[4] Rennes,[5] and others.[6] Scharff[7] even gives an alleged case of a man with five testicles, three of which are stated to have been well formed, while the other two were much smaller than natural. And lastly, Loder[8] is said to have exhibited to the Göttingen Academy, drawings taken from the body of a male infant, on whom *all* the sexual apparatus existed double, there being two penes, a double scrotum and urinary bladder, and, as it was supposed, four testicles.

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[1] Acta Med. Hafn. tom. iii. obs. 93.

[2] Observ. Rar. cent. i. p. 55.

[3] Eph. Nat. Cur. Dec. ii. ann. v. obs. 55.

[4] Arch. Gén. de Méd. tom. xvii. p. 58.

[5] Journ. Gén. de Méd. tom. c. p. 57.

[6] Gazette Medicale for April, 1837. Three distinct mammæ in a father, and in his three sons and two daughters.

[7] Dict. des. Sc. Méd. tom. xxxiv. p. 529.

[8] Eph. Nat. Cur. Dec. i. ann. ii. p. 346.

[9] Journ. de Méd. de Corvisart, tom. ix. p. 378.

[10] Obs. Anat. vii.

[11] Fantoni, Anat. p. 267.

[12] Zeitschrift für Physiologie, Bd. v. s. 110. One case with three, and three with four, nipples. In one case the malformation was hereditary.

[13] Dict. des Sc. Méd. tom. xxx. pp. 377–78.

[14] Statistical Account of Scotland, xix. p. 288.

[15] London Med-Chirurg. Trans. vol. xxi. p. 266.

[16] Lancet for Feb. 24, 1838.

[17] Dict. des Sc. Méd. tom. xxxiv. p. 529.

[18] Eph. Nat. Cur. Dec. iii. ann. iii. obs. 77.

[19] Cohen vom Stein, Halle, 1774, p. 107.

[20] Mém. de Chir. tom. i. p. 374.

[21] Salzburg Medicinische Zeitung, 1811, s. 188.

[22] Bell's Anatomy, vol. iii. p 428.

[23] Journ. Hebdom. tom. x. p. 168.

[24] Obs. Med. pars iv. obs. 20.

In all the preceding instances the local duplicity of the particular reproductive and other organs adverted to, existed independently of any duplicity in the body in general, or in any other individual parts of it. And if we once admit, what the preceding instances will scarcely allow us to deny, that there may occur a duplicity of some of the male sexual organs in a male, or of some of the female sexual organs in a female, it is certainly easy to go one step farther, and admit that the double organ or organs may, however rarely, be formed in other instances upon an opposite sexual type. If, for instance, a division or duplicity, original or acquired, of a Wolffian body, or of the neutral organ primarily formed on its inner side, or of any other cause, be capable of giving rise to the development of two testes or two ovaries upon the same side, it is not difficult to conceive that one of the double organs may become female and the other male. Indeed all our knowledge of the unity of structure and development between the various analogous male and female reproductive organs, as well as the fact of the occasional replacement of an organ of the one sex by that of the other in cases in which the sexual type is entirely single, as seen in instances of Lateral hermaphroditism, would lead us a priori to suppose that, if a local duplicity in any of the sexual organs was liable to occur, this duplicity would sometimes show itself in the double organs assuming opposite sexual characters, and thus constituting some of those varieties of double or vertical hermaphroditism that we have already had occasion to describe.

In the preceding observations we have proceeded upon the opinion commonly received by physiologists, of the fundamental u sex among all individuals belonging to the higher orders of ar or, to express it otherwise, we have assumed that each ind is, when normally formed, originally furnished with element capable of forming one set of sexual organs only. We do n stop to inquire whether this single sexual type is, in all en originally female, as maintained by Rosenmüller, Meckel, ville, Grant, and others; or of a neutral or intermediate ch as supposed by the St. Hilaires, Serres, Ackermann, Home, & as we are certainly ourselves inclined to believe it.[1] On th ject, however, a physiological doctrine of a different kind h brought forward by Dr. Knox, and this doctrine is so inti connected with the question of the nature and origin of true phrodites, that we must here briefly consider it.

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[1] Mém. de Chirurg. s. i. p. 131.
[2] Handbuch der Path. Anat. Bd. iii. s. 393.
[3] Acta Hafn. tom. i. p. 320.
[4] New York Medical Repository, vol. iv. p. 801.
[5] Arch. Gén. de Méd. tom. xxiii. p. 17.
[6] See Haller's El. Phys. tom. v. pp. 411, 412; Arnaud's Chem. de Chirurg. tom. i. p. 128, &c.
[7] Eph. Nat. Cur. Dec. iii. ann. v. vi. obs. 69.
[8] Göttingen Anz. 1802, p. 466.

Dr. Knox,[2] in conformity with some more general views he entertains on transcendental anatomy, is inclined to reg type of the genital organs in man and the higher animals, as embryo, originally hermaphroditic, or as comprising element distinct parts, out of which both sets of sexual organs co formed; and he believes that, owing to particular but un circumstances, either the one or the other only of these elements comes to be evolved in the normal course of develo In those abnormal cases, again, in which, as in instances of hermaphroditism, more or fewer of both sets of genital org present upon the same individual, he maintains that this is be considered as a malformation by duplicity, but is only a nent condition of the original double sexual type, and is attrib to the simultaneous development to a greater or less extent o the male and female sets of sexual elements.

This doctrine of the original but temporary double-sexed ch of all embryos, derives, perhaps, its principal support from a to which Dr. Knox does not advert—we mean the existence the normal and permanent sexual type in most plants and in any of the lower orders of animals. But this argument by analogy rtainly cannot by any means be considered as a sufficient basis r the establishment of so broad and important a generalization in ilosophical anatomy. Dr. Knox himself seems to have been induced to adopt the idea principally because it afforded (when once sumed as a fact) a simple and elegant solution, upon the laws of evelopment, of the occasional occurrence of cases of true hermahroditism; and in doing so, he appears to have proceeded upon the ode in which most such physiological hypotheses have been made, iz., by drawing his premises from his deductions instead of his eductions from his premises. In the present state, however, of natomical and physiological knowledge, Dr. Knox's hypothesis, owever ingenious in itself, is one to which we cannot subscribe; or, first, it is totally opposed to all the facts which have been ascertained, and all the direct observations which have been made by Rathke, Meckel, Müller, Valentin, and other modern anatomists, upon the sexual structure of the embryos of the higher animals in their earliest state; and, secondly, if we were to admit it merely as a probable hypothesis, it is still even in this respect equally as incapable as the old doctrine of sexual unity, of explaining all the cases of malformation by duplicity of the genital organs; for, as we have already shown, there are some apparently well-authenticated instances of the existence of three or four testicles upon the same man, or three or four ovaries upon the same woman; and in reference to all such cases we would, if we proceeded upon the same data and the same line of argument as those adopted by Dr. Knox, be obliged to suppose that the original sexual type is not, as he imagines, double only as respects the two sexes, but double even as respects each sex, and that all embryos had originally not simply the elements of two, but those of three or four testicles and ovaries. In explaining such cases as those to which we allude, Dr. Knox, on his own doctrine, must of necessity admit the existence of a malformation by duplicity of the sexual organs in question; and if we grant this in regard to these instances, it is surely unnecessary to invent a particular and gratuitous hypothesis for the explanation of the analogous anatomical abnormalities observed in hermaphroditism. At present we must, we believe, merely consider the occurrence of anomalous duplicity of the sexual organs, and of various other individual parts of the body, as so many simple empirical facts, of which we cannot in the existing state of our knowledge, give any satisfactory explanation, or, in other words, which we cannot reduce to any more simple or general fact; though from the success which has attended the labors of many modern investigators in this particular department of anatomy, it seems to us not irrational to hope that ere long we may be enabled to gain much new light upon the question of double hermaphroditism and the whole subject of malformation by duplicity.

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[1] Meckel (De Duplicitate Monstrosâ, p. 14), and Andral (Anat. Path. tom. i assume it, after Haller, as a fact, that a much larger proportion of monsters belo female than to the male sex; and while they attribute this circumstance to the genit in these beings retaining, from the general defect of development, their *original fem* character, they at the same time consider this circumstance to be strongly corrob this particular doctrine. Isid. St. Hilaire has shown (Hist. des Anomal. tom. iii that the supposed fact itself does not hold true in respect to some genera of monste even reversed in others; and he doubts if it be of such a degree of generality in r monsters in general as to merit being raised into a teratological law. If the views o were correct, we should certainly expect at least that spurious hermaphroditism, w development of the sexual parts is commonly abnormal from *defect*, should be mu frequent in the female than in the male. The list, however, of recorded cases of latter is, we believe, more than double the number of it in the former.

[2] Brewster's Edinburgh Journal of Science, vol. ii. p. 322.

ANATOMICAL DEGREE OF SEXUAL DUPLICITY IN THE DIFFERENT FORMS OF HERMAPHRODITISM.

Though the cases which we have brought forward do not present any instances of such perfect hermaphrodites in the human subject or in quadrupeds, as those which are represented upon the ancient Greek statues and medals,[1] or that have been described and delineated by Lycosthenes, Paré, Schenkius, and the older authors on monstrosities, they yet present to us a sufficient number of instances in which, in accordance with the definition we have previously given of true hermaphroditism, there actually coexisted upon the body of the same individual more or fewer of the genital organs both of the male and female.

From the relations and size of the bony pelvis, and the fact of the penis and clitoris being only repetitions, in situation and structure and organic connections, of each other in the two sexes, it is useless perhaps to expect that we should ever find in any one case all the parts of both sexes present at the same time. For, since the male penis is only a magnified condition of the female clitoris, and since both of these organs are connected by the same anatomical relations to the same part of the pelvis, it would almost require some duplicity in the pelvic bones themselves to admit of the simultaneous presence of both; and in no authentic case has any approach to their coexistence upon the same individual been observed.

Various authors who have written upon the subject of hermaphroditism have gone so far as to endeavor to refer all instances of it to some one or other of those varieties that we have described under the name of *spurious*. Thus, dogmatizing in a spirit of unphilosophical scepticism, Parsons[2] and Hill[3] have endeavored to show that all reputed hermaphrodites are only malformed females having a preternatural development of the clitoris, and in some instances with the ovaries descended into the labia. Others, on the contrary, as Professors Osiander[1] and Feiler,[2] maintain with equal inaccuracy that every supposed instance of hermaphroditism is referable to a hypospadiac state of the penis and scrotum, in persons that are in other respects essentially male.

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[1] See Winckelman, Hist. de l'Art, tom. i. p. 364; and Caylus, Recueil d'Antiquités, tom. iii.; Heinrich, Commentatio quâ Hermaphroditorum artis antiquæ operibus illustrium, origines et causæ explicantur. Hamburg, 1805. Blumenbach, in his Specimen Hist. Nat. Antiq. artis, Göttingen, 1808, mentions and figures (pl. i. fig. 5, p. 15) a small ancient silver cast or impression of a case of hypospadias of the male genital parts, which he supposes to have formed a votive offering from some individual, malformed in the manner represented.

[2] Inquiry into the nature of Hermaphrodites, p. 145. We would particularly point out the cases quoted by Dr. Parsons at pp. 14, 26, 30, 88, 95, 130, &c. of his able essay, as directly contradictory of his own doctrine, or as instances of hermaphroditic appearances in persons not of the female but of the male sex.

[3] Review of the Philosophical Transactions.

Various physiologists, again, while they admit the occurrence of all the different varieties of spurious hermaphroditism, are inclined to deny that any such combinations of male and female organs upon the same body as those which constitute our several varieties of true hermaphroditism, are ever observed to occur in the human subject, or among the higher classes of animals.[3] In spite of the recent accumulation of new and authentic cases, Professor Müller of Berlin is, in particular, in his excellent treatise on the development of the genital organs, published in 1830,[4] still inclined to coincide in a great degree in this opinion. Few, however, I believe, or indeed, no, physiologists now refuse to admit the occasional occurrence of a combination of male and female organs upon the same individual, when that combination does not (as in lateral and transverse hermaphroditism) imply a true sexual duplicity or repetition of any of the corresponding male and female parts. But some still altogether doubt the probability of any cases of our third division of Double or Complex hermaphroditism. Certainly, in the examination of the cases referable to that section, and especially in reference to the two first varieties of it, a sufficient degree of attention has not been directed to the accurate anatomical distinction of the superadded parts supposed to exist, from others with which it is possible to confound them. We shall here, therefore, shortly recapitulate and inquire into some of the principal sources of fallacy that are apt to mislead the incautious observer in the examination of such instances as those to which we allude, and which have often led to the idea of sexual duplicity, when the sexual type was single only, but malformed; and in doing so we shall consider the various sources of error, in an order conformable with those divisions of double hermaphroditism that we have previously adopted—speaking of the mistakes which may and have been committed in judging of the supposed coexistence, 1st, of a female uterus, and male vesiculæ seminales and vasa deferentia; 2d, of a female uterus and male testicles, &c.; 3d, of both testicles and ovaries.

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[1] Neue Denkwürdigk. für Geburtshülfe, Bd. i. n. 8.

[2] Ueber Angeb. menschliche Misbildung. Landshut, 1820.

[3] Thus Portal, Anat. Méd. tom. v. p. 474; Haller, El. Phys. tom. viii. p. 7, "merito dubitatur;" Voigtel, Handbuch der Path. Anat. Bd. iii. s. 364; Lawrence, Art. Generation, in Rees' Cyclopædia.

[4] Bildungsgeschichte der Genitalien, &c.; Physiology by Baly, vol. ii. p. 640.

1. FALLACIES IN JUDGING OF THE ADDITION OF MALE SEMINAL DU TO A FEMALE TYPE OF SEXUAL ORGANS.[1]

That form of sexual duplicity which consists in the supposed
peraddition of male vesiculæ seminales and vasa deferentia to
organization in other respects female, appears to have been hithe
observed only among the Ruminantia, or in free-martin cows.
judging of the reality of this variety of hermaphroditic malformati
in any given case, in that animal, there is one source of fallacy t
requires to be particularly guarded against, and the considerati
of which suffices fully to explain most if not all the recorded exa
ples of this malformation. In the female sexual parts of some R
minantia and Pachydermata,[2] but particularly in the domestic c
and sow, Dr. Gaertner of Copenhagen pointed out in 1822[3] the ex
tence of two canals or ducts which have since that time been ge
rally described under his name. On each side of the body, one
these ducts arises in the vicinity of the ovary, or toward the fi
briated extremity of the Fallopian tube, runs down first in the d
plicature of the broad ligament, and afterwards in the substance
the parietes of the uterus and vagina, to near the meatus urinari
and there opens into the vaginal cavity. Each duct communica
with several small glands, follicles, or cysts, that are scattered alo
its course, and which perhaps may not be improperly described
diverticula from the ducts themselves, or nascent vesiculæ seminal
Now when we consider the relations of those imperfect ducts a
cysts that are occasionally observed in the free-martin cow, situat
along each side of the defectively developed uterus, and which M
Hunter has described as male vasa deferentia and vesiculæ seminal
it seems to us that these supposed male organs are only in real
the ducts of Gaertner, with their accompanying follicles or cys
existing perhaps in a preternaturally developed and dilated conditi
They correspond in their origin, course, and position with the can
and cysts discovered by Gaertner; and certainly in the present st
of our knowledge, we are fully entitled to refer them to this *norm*
portion of the female structure, rather than to regard them as *abn*
mal and superadded male organs, and as affording, in consequen
an example of true sexual duplicity.

Before leaving the fallacy which we have to guard against in co unding the ducts of Gaertner in free-martin cows with the male minal canals, it is necessary also to observe, that, as was long ago ggested[1] by Dr. Jacobson of Copenhagen, and as we have already en,[2] has been latterly proved by Kobelt, F. Meckel, and others, ese canals are now generally considered by anatomists as merely rmanent remains of the ducts of the Wolffian bodies. If, howver, it should ever happen that further and more accurate observaons prove the two to be different, then the possible permanent ate of the ducts of the Wolffian bodies must be looked upon as fording another source of error, by which we may deceive ourlves in judging of sexual duplicity from the supposed superaddiion of male seminal canals to a female sexual apparatus.

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[1] See ante, p. 251.

[2] M. Delmas seems to have observed a somewhat similar structure in the kanga
(Ephem. Med. de Montpellier, tom. v. p. 115); and Coste, in the sheep. See Comptes Rend
de l'Academie des Sciences for 1839, p. 334.

[3] Anatomisk Beskrivelse over et ved Nogle Dyr-Arters uterus undersögt Glandulöst org
&c., Copenhagen, 1822; Edin. Med. and Surg. Journ. vol. xxi. p. 460.

In the course of the preceding pages we have had occasion to alde to cases in the human subject, and in the dog and sheep, in hich vasa deferentia were stated to have existed in the same indiidual along with Fallopian tubes. In these instances, the supposed ale seminal ducts were, in all probability, canals analogous to those escribed by Gaertner in the cow and sow; and in connection with his inquiry it is interesting to remark that Malpighi,[3] who seems o have been well acquainted with the existence of these ducts in he cow, has suggested that they may also exist in a more obscurely eveloped state in the human female, and may perhaps be identified ith the ramose lacunæ described by De Graaf, Bartholin, Riolan, c. Indeed, A. C. Baudelocque has, in a case published in the évue Médicale for March, 1826, described a human uterus which ontained in its parietes a canal coming from the region of the ight Fallopian tube, and opening upon the internal surface of the ervix uteri; and Moureau and Gardien seem to have me twith a econd similar instance.[4] Were these canals in the human female ot truly the same as the canals of Gaertner in the cow, &c., and onsequently the persistent remains of the ducts of the Wolffian ody? And would not such cases of abnormal persistence of these ucts or canals in woman, be more frequently detected, if the attenion of anatomists were more specially directed to the inquiry?

FALLACIES IN THE SUPPOSED COEXISTENCE OF A FEMALE UTERUS WITH TESTICLES AND OTHER ORGANS OF A MALE SEXUAL TYPE.

We have, in a previous part of this memoir (p. 252 to p. 262) dduced above twenty different instances in the human subject, and the quadruped, in which a large-sized uterus, or both a uterus and Fallopian tubes were described as having been found upon the bodies of individuals that were in other respects essentially males.

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[1] Journal de l'Institut, tom. ii. p. 160; and Die Okenschen Körper, &c. Copenhagen, 1830.

[2] See antecedent section on the Unity of Type, Wolffian bodies, &c., p. 293, &c.

[3] Philosophical Transactions for 1684, p. 634. [4] Medical Repository for 1826, p. 571.

In reference to these instances, it may be doubted whether the sexual organization of the malformed animal was not *entirely* male, the large but imperfect uterus which is present being merely a preternaturally or abnormally developed state of the small prostatic vesicle or utricle normally found in the organization of the male.[1] Thus, in the case detailed by Ackermann, the only male sexual organ that was entirely deficient was the prostate, and the only reputed female organ which was present was an imperfect cystiform uterus, having, as the normal state of the prostatic vesicle, the vasa deferentia penetrating through its substance without opening into its cavity, and ultimately terminating, as the orifice of the male prostatic utricle actually does, in the posterior part of the urethra. In the analogous instance quoted in the preceding page (p. 257) from Steghlener, a similar arrangement of parts was observed; and in that case there was in the enlarged ureters and renal infundibula perhaps sufficient evidence, as we shall afterwards point out when speaking of the probable causes of hermaphroditism, of a distending power having acted upon the whole internal surface of the urinary and genital canals, and with so great a force (we may in the meantime allow) as to be capable of producing a morbid dilatation and enlargement of the cavity and substance of the prostatic vesicle. But even granting that the instances given by Ackermann and Steghlener, and most other cases, are not at all satisfactory in regard to the reputed existence in them of a variety of true sexual duplicity, and allowing, what seems probable or indeed certain, that the misshapen and imperfect uteri in these examples were formed by a simple dilatation and enlargement of the natural male utricle or prostatic vesicle,[2] there are still some other cases pertaining to this division which scarcely fall within the bounds of the explanation, hat the form of hermaphroditic malformation in question always consists of nothing more than a male organization, with the usual normal male utricle developed to an unusual and abnormal size.

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[1] See antecedently its description, p. 294, &c.

[2] In Ackermann's case, the dilated cystoid utricle perhaps represented the vagina more than the uterus, or the lower portion of the uterus more than its fundus; and the same remark applies, with probably great truth, to Steghlener's hermaphrodite, where the vasa deferentia opened into the cavity of the so-called uterus—as the analogous Wolffian ducts normally do into the utricle of the hare and rabbit, and possibly in the very early human embryo. If the upper portion or fundus of the uterine organ had been represented in these cases, that part of the cystoid utricle would in all likelihood have been double or bifurcated as the uterus is in the early female embryo. In the human hermaphrodite, the ordinary site of junction of the Wolffian ducts, or seminal tubes, with the genital sinus, may sometimes in such exceptional cases, be regulated—1. By the very early period at which the arrestment and consequent persistence of embryonic type, takes place; and 2. By the degree of evolution which the malformed parts subsequently undergo. In Vrolik's case of double hermaphrodite—already cited in a previous section, p. 269—the Wolffian duct or vas deferens, on the right or most developed side, opened as usual, into the vagina; while on the left or least developed side, it opened into the angle of the uterus.

In the adult male goat, and in some other animals, the horns of the bilobed prostatic utricle sometimes run and stretch, in their natural state of conformation, alongside the vasa deferentia up even to the testicles themselves. We cannot, therefore, be surprised at finding that in cases of the present division of hermaphroditism, such as the goat described by Gurlt (see p. 253), and represented in our woodcut (Fig. 27), the so-called Fallopian tubes, or perhaps more properly the elongated cornua uteri, passed through the inguinal rings, and were ultimately affixed, as they often are in this division of these malformations, to the epididymes of the testicles. In all this structural arrangement, there is nothing incompatible with the idea that the uterus and uterine cornua which were present, consisted only of a preternaturally enlarged state of the prostatic or male utricle. But in the adult male *human* subject, the male utricle is rounded at its fundus, and gives off no cornua or appearances of Fallopian tubes. Yet, in the division of hermaphroditic malformations of which we are at present treating—viz., those consisting of a male organization, with the addition to it of a large uterus, &c.—we find several cases in our own species in which the uterus that existed was provided with cornua and Fallopian tubes. Thus, in the person dissected by Petit, the imperfect uterus was furnished with two perforate Fallopian tubes of three and a half inches in length, and at the same time it is distinctly stated that not only the prostate gland, but the vesiculæ seminales and vasa deferentia were also present. The vasa deferentia, between their origin from the testicles and their urethral termination, were each above seven inches long, and they entered the urethra by two apertures that were quite distinct and separate from the orifice of the uterus, which opened into the urethral canal at a point between the neck of the bladder and the prostate. In this case the Fallopian tubes of the uterus can scarcely be considered as merely formed at the expense of the normal male or prostatic utricle; and consequently we can only, in one sense, consider the Fallopian tubes or cornua uteri as a *superaddition* to, and not a *transformation* of, the male structures; or, in other words, we may so far look upon the above as an instance of duplicity in a part of the sexual apparatus.

The same reasoning and remarks might be shown, if it were necessary, to apply in a still greater degree to the analogous example in the human subject given by Professor Mayer, and where he found upon an infant six months old, superadded to a complete male organization, a vagina, uterus, and two Fallopian tubes, furnished with dilated sacs as infundibuliform extremities. See the description and delineation in a preceding section (page 259, Fig. 30). In an interesting case of the same description lately detailed by Betz,[1] that anatomist found on the body of a young infant, a male organization (testis, vasa deferentia, &c.), with a uterus in the site of the prostatic vesicle, as large as the female uterus in children of the same age. Further, this uterus had Fallopian tubes running from it on either side, along with the vasa deferentia, to the testicles; and of these Fallopian tubes, the one on the right side, at least, ended at the epididymes of the testis, in an imperfect infundibuliform extremity or dilatation.

In all these latter instances, the form and type of the uterus and uterine tubes were much greater and more perfect than could be produced by simple enlargement and dilatation of the human prostatic vesicle or male utricle, such as we see it in the adult. But still, after all, this does not, we opine, remove such cases of hermaphroditic malformation from the category of those that consist merely of a male organization, with the supperaddition of an enlarged and developed state of the normal male utricle and its elements. In the very early mammalian male embryo, as we have already stated (p. 293), hollow filaments or ducts corresponding to the Fallopian tubes are present, and, in the human and some other embryos they very early also disappear, leaving the male utricle without any appearance of such structural appendages. In the cases of Petit, Mayer, Betz, &c., these transitory uterine appendages were unremoved, in consequence of some cotemporaneous arrest or error in development; they consequently remained beyond the period of intra-uterine life, and at the same time they grew, like most other malformed or persistent embryonic structures, with the general growth of the body, and of the parts more immediately surrounding them. Hence, in these instances, we have, *in addition* to the usual male organs, apparently female uterine structures, incapable of being imitated by any mere enlargement and dilatation of the adult prostatic or male utricle; but essentially consisting of a persistent state of those transitory conditions of the male utricle and its appended tubes, that exist in some of the earliest stages of embryonic developement.

Further, in reference to the general imperfection of the sexual parts observed in these and in other examples of hermaphroditic malformation in the present and neighboring sections, let me here again repeat, as one of the general laws to which these malformations are subject, an important statement already made, viz., that whenever we find the development of the sexual organs of an individual,

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[1] Müller's Archives für Anatomie, &c. 1850, p. 65.

her male or female, restrained or arrested so as to be below the
al type or standard, some of the organs of the opposite sex will,
e same individual, be observed to be developed, as it were, in
s, or *above* their normal type or standard; and, as in the pre-
g instance, this apparent excess will usually, we believe, be
l to arise from the mere permanence in the adult of some early
ransitory form of organization in the embryo.

FALLACIES IN THE SUPPOSED COEXISTENCE OF TESTICLES AND OVARIES.

several of the instances already described, from p. 262 to p.
and in which there was supposed to be a coexistence of both
les and ovaries upon the same side or sides of the body, it seems
y probable that there has been occasionally a fallacy in the
vation, owing to a want of knowledge of some anatomical
mstances that are liable to lead us into error in making an
ination of such a case.

e have repeatedly had occasion to allude to the existence in the
state, and in both sexes, of the Wolffian bodies, which are
d one along each side of the spine, and occupy at an early period
e embryo a great part of the cavity of the trunk. These bodies
k and alter in the natural course of development, but never
ether disappear in man, and in the quadruped leave vestiges of
presence even in the adult animal.[1]

is particular type of structure, which is comparatively so very
in the embryo, may, like every other type of the fœtus, from
pediment or arrest in the natural course of the changes occurring
e development of the body in general, or of the genital organs
rticular, remain occasionally, we have every reason to believe,
anently enlarged in one or all of its parts, and thus by its pre-
in the animal lead us to suppose that a rudimentary testicle
s in an otherwise well-marked female; or, on the other hand,
an ovary exists in an otherwise well-marked male. Both of
mistakes will be the more apt to be committed if the original
tory duct of the Wolffian body remains, for it may give the ap-
nce of the addition of a vas deferens to the supposed testicle,
an imperfect Fallopian tube to the supposed ovary.

e error, also, of confounding a permanently large Wolffian
with the testicle will be more liable to occur, in consequence
e former body being naturally composed of an accumulation
nvoluted diverticula, which might be readily mistaken by an
tious observer for the seminiferous ducts of the latter.

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[1] e the description which I have given of the Wolffian bodies, and their persistent condi-
n the adult female and male, in a preceding section, p. 291, &c.

There is certainly strong cause for doubting whether, in some of the cases that we have cited of the supposed coexistence of testicles and ovaries upon the same side, the permanent embryonic type of the Wolffian bodies and their ducts had not been mistaken either for testicles and vasa deferentia, while the sexual organization was otherwise truly female, or, for ovaries and Fallopian tubes, while the type of structure was in other respects strictly that of the male. This remark may perhaps with confidence be applied, for example, to the case of the free-martin described by Mr. Hunter (see p. 263); and in this and in most other similar instances the supposed testicles and ovaries have not been examined with anything like sufficient anatomical accuracy. At the same time, however, it appears to us quite impossible to explain away all the recorded cases of the supposed coexistence of testicles and ovaries upon this principle. The cases, for example, of testes and ovaries observed on the same side by Mascagni and Vrolik (see pp. 262, 269), cannot be set aside by such an interpretation. And in reference to this point, we would further particularly observe that the consideration of the *relative position* occupied by the reputed testicles and ovaries may perhaps afford us a useful guide in cases of doubt. In some of the instances that have been previously cited, the relative situation of the supposed testicles and ovaries was exactly such as the Wolffian bodies are known to bear to these parts. In other instances, however, as in the ape described by Dr. Harlan, the relative situation in which the testicles and ovaries were found, was that which they occupy in the perfectly formed male and female; and in such a case as this, it would surely be over-sceptical, and at the same time in opposition to all that we yet know of the history of the Wolffian bodies, to suppose that these bodies had imitated the testicles so far as to move out of their original locality and travel downwards through the inguinal rings. At the same time, we must recollect that in this case the distinctive anatomical structure both of the testicles and ovaries seems to have been satisfactorily made out, in so far that the former are described as "perfectly formed," and the latter as having "minute ova visible in them." "The male and female organs of generation," Dr. Harlan adds, "were as completely perfected as could have been anticipated in so young an individual, and resembled those of other individuals of a similar age." Now if we once admit in this, or in any one other particular instance, that the evidence of the coexistence of testicles and ovaries is satisfactory, then certainly we may in any equivocal case be entitled to doubt, until we have some more sufficient criterion for distinction pointed out, whether the dubious double bodies that we may meet with be a rudimentary testicle or ovary conjoined with an imperfect ffian body, or really a true instance of the presence both of tes- s and ovaries upon one, or upon both sides of the body of the individual.

IOLOGICAL DEGREE OF SEXUAL PERFECTION IN HERMAPHRODITES.

mong those lower tribes of animals, such as the Abranchial elida, Pteropoda, &c., that are naturally hermaphrodite, every vidual is in itself a perfect representation of the species to which longs. In the higher orders, however, in which the distinction separation of the sexes come to be marked, each individual, g either solely male or solely female, can, as has often been arked, be regarded only as representing one-half of its entire ies. In most instances of hermaphroditism among these more ect animals, the malformed being does not even attain to this ree of perfection, but is in general so defectively constituted as to have the proper physiological characters and attributes of er sex. In cases of spurious hermaphroditism it would appear sometimes, though the copulative or external sexual parts are atly and variously malformed, the internal or proper reproductive ans are developed with sufficient perfection to enable them to form the functions belonging to them. We have very little of, however, that in any instances of what we have described as hermaphroditism, the apparatus of either sex is ever formed h such anatomical perfection as to empower the malformed being ear a successful part in the reproductive function. Indeed in or in almost all cases belonging to this last order of hermaphro- sm, the individual who is the subject of the malformation may, much more than poetical truth, be described both anatomically physiologically, as, in the words of Ovid—

> Concretus sexu, sed non perfectus utroque,
> Ambiguo venere, neutro potiundus amore.

here is on record one remarkable instance of apparent excep- to this general observation, a notice of which we have reserved this place, on account of the want of such precise knowledge of true anatomical peculiarities of the case as might enable us to r it to the section which it ought to occupy in our classification. case to which we allude was described by Dr. Hendy of New rk, in a letter dated from Lisbon in 1807, and the subject of it a Portuguese, twenty-eight years old, of a tall and slender but sculine figure.[1] "The penis and testicles," to adopt the words Dr. Hendy's own narrative, "with their common covering, the scrotum, are in the usual situation, of the form and appearance, and very nearly of the size of those of an adult. The præputium covers the glans completely, and admits of being partially retracted. On the introduction of a probe, the male urethra appeared to be pervious about a third of its length, beyond which the resistance to its passage was insuperable by any ordinary justifiable force. There is a tendency to the growth of a beard, which is kept short by clipping with scissors. The female parts do not differ from those of the more perfect sex, except in the size of the labia, which are not so prominent, and also that the whole of the external organs appear to be situated nearer the rectum, and are not surrounded with the usual quantity of hair. The thighs do not possess the tapering fulness common to the exquisitely formed female; the ossa ilii are less expanded, and the breasts are very small. In voice and manners the female predominates. She menstruates regularly, was twice pregnant, and miscarried in the third and fifth months of gestation. During copulation the penis becomes erect. There has never existed an inclination for commerce with the female under any circumstances of excitement of the venereal passion." In the preceding case, if we may confidently trust to the account given of it, we have ample proof of the existence of the internal female sexual organs, in the circumstances of menstruation and impregnation taking place; and at the same time there appears considerable evidence for believing that some of the male organs were present. For, even if we were to argue that the bodies present in the scrotum or united labia might be ovaries and not testicles—or, as happened in Vrolik and Sir Astley Cooper's cases, mere masses of fat—and that the supposed semi-perforate penis was only an enlarged clitoris, still the masculine figure of the individual, the imperfect beard, the narrowness of the pelvis, and the form of the lower extremities would tend to indicate the probable existence of the rudiments of some male organs; and if we go so far as to admit this, we must further allow the present to be an instance of hermaphroditism, in which *one* of the sets of sexual organs was capable of assuming its appropriate physiological part in the process of reproduction, though perhaps unable, if we may judge from abortion having twice occurred, of ultimately perfecting that process.

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[1] New York Medical Repository, vol. xii. p. 86.

The preceding remarks upon the functional reproductive powers of reputed true hermaphrodites have been meant to apply only to the supposed perfection of *one* order of their sexual organs. It becomes a still more interesting question whether it ever occurs that in any abnormal hermaphrodite among the more perfect tribes of animals, both kinds of sexual parts may be found in so perfectly eveloped a state as to enable the individual to complete the sexual ct within its own body; or, in other words, to impregnate and be mpregnated by itself. Though we have assuredly no positive roof to furnish,[1] that an hermaphrodite so physiologically perfect as ever yet been observed, and should very strongly doubt its ccurrence from the almost universal imperfection, in an anatomi-al point of view, of the malformed organs; yet we have, on the ther hand, no very rational ground, except that of the experience f all observers up to the present date, for denying entirely and nconditionally the utter possibility of it. And, perhaps, we hould look upon this possibility with a less degree of scepticism, when we consider that a double hermaphroditism exists as the normal sexual condition of some of the lower tribes of animated beings; and, at the same time, take into account the fact of the more or less direct communication which has been generally found to exist between the female uterus and the male passages, in cases of lateral and of complex hermaphroditism, in the human subject and in quadrupeds.

In one of the cases of hermaphroditism in the goat, previously quoted from Mayer, and where there were present two male testicles, epididymes, vasa deferentia, and vasculæ seminales, and a female vagina, uterus, and Fallopian tubes, with a body at the abdominal extremity of one of these tubes, that was supposed by Mayer to resemble a collection of Graafian vesicles; the male vasa deferentia opened into the female vagina, and its cavity, with that f the uterus and of all the male sexual canals, was distended with whitish fluid of the odor and color of male semen, and containng, according to Bergmann, the chemical principle proper to that ecretion. It is not, therefore, altogether without some appearance f foundation in fact, that Mayer has added to the history of this ase the following problematical remark: "Fuit ergo revera hermaphroditus semetipsum fœcundare studens."[2]

In a similar strain, Dr. Harlan has added to the account that he has given of the very complete case of hermaphroditism, already mentioned as met with in the Borneo orang-outang, the following observations and queries. "Admitting," he remarks, "what in reality appeared to be the fact, that all the essential organs of both sexes, were present in this individual, had the subject lived to adult age, most interesting results might have been elicited. Could no the animal have been impregnated by a male individual, by rupturing the membrane closing the vulva? or by masturbation, migh not the animal have impregnated itself? by this means excitin the testicles to discharge their seminal liquor into its own vagina The imperfection of the urethra most probably would have prevented the animal from ejecting the semen into the vagina of another individual."[1]

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[1] We do not certainly feel entitled to place among the category of correct observations either the alleged case given by Linnæus (Mangetus' Bibliotheca Chirurg. lib. iv.) of a sow with perfect male organs on one side, and a womb containing several fœtuses on the opposite; or that mentioned by Faber (Hernandez' Nov. Plant. Anim. Mexic. Histor. p. 547) and quoted by Haller and Rudolphi, of the coexistence in a rat, of ovaries and a uterus with nine fœtuses, along with complete male organs.

[2] Icones, &c., p. 20.

It has been sometimes urged as an argument, conclusively illustrative of the fact of a double hermaphrodite impregnating itself that in the hermaphrodite *Gastrophaga pini* described by Scopoli, the insect is stated to have been seen to advance its penis and copulate with its own female organs; and, afterwards, we are informed, the female side laid eggs from which young caterpillars were produced. Before, however, admitting this case to present an incontrovertible instance of absolute hermaphroditism, with the functions of the two sets of sexual organs existing in a perfect condition upon the same individual, it is necessary to recollect a possible source of fallacy in this circumstance, that female Gastrophaga have been observed to lay fertile eggs, although they had not had previously any connection with the male, as remarked by Professor Baster[3] in one instance in a female *Gastrophaga quercifolia*, and in another in the *Gastrophaga pini* by Suckow.[4] The same fact is further alleged to have been observed in some few instances by Pallas, Treviranus, Bernouilli, and others,[5] in regard to individuals belonging to some other of the higher orders of insects and animals, as in the *Limnæus auricularis*[6] and *Helix vivipara*[7] among Molluscs, thus bringing them in this respect into analogy with the Aphides and Cyprides.

CAUSES OF HERMAPHRODITIC MALFORMATION.

As yet we possess very little accurate knowledge, either in respect to the mode in which the determining causes of hermaphroditic malformation act, or the nature of these causes themselves. Upon the question of the probable or possible origin of actual hermaphroditic or sexual duplicity, we have offered already one or two remarks at pp. 301 and 303.

Most of the varieties of spurious hermaphroditism may, as we have just explained, be traced to an arrest in the development of he sexual organs at one or other period of their evolution, in consequence of which, some of those types of structure in these parts which were intended to be temporary and transitory only, are rendered fixed or permanent in their character. Our knowledge of the more immediate causes of such arrested development in these and in other individual parts and organs of the body, is as yet extremely limited.[1] We may, however, in reference to the particular forms of arrested development observed in hermaphroditism remark, that in consequence of the great influence which, as we have already pointed out, is exercised by morbid states of the ovaries and testicles, in retarding or preventing the evolution of the sexual apparatus and characters after birth, it has been suggested with considerable probability by Meckel[2] and Isidore St. Hilaire,[3] that, in their ultimate analysis, certain cases of hermaphroditic malformation may be traced in the course of their causation to morbid influences exercised in the early embryo, at a period more or less near to conception, upon the ovaries or testicles, or upon those organs of a neuter or yet undetermined sex which afterwards assume the structure of one or other of these bodies. Further, the effects which this supposed morbid influence exercises directly upon the embryonic ovaries and testicles, and indirectly through them, upon the rest of the genital apparatus, and consequently the modifications of sexual structure which it produces, may possibly be much varied according to its extent, duration, and nature, and according to the particular period of development at which it comes into action. It is evident that this explanation of hermaphroditism can only refer to the varieties of the malformation which consist of an imperfection or deficiency in the development, and cannot apply to those instances in which there is a superaddition of sexual organs. If, however, we can once satisfy ourselves that any set of cases whatever are traceable to a morbid action affecting the testicles or ovaries of the early embryo, our investigations into the causes of these cases will necessarily be much simplified, for our inquiries would be reduced from a vague and indefinite search after the production of a number of anomalies of structure affecting several different organs at the same time, to an attempt to trace out the nature of those morbid conditions to which the embryonic testicles and ovaries were subject, and which were capable of so far changing the structure and action of these organs as to give rise to the effects in question. Of the diseased states, however, to which the reproductive and other organs of the system are liable during the progress of their early development, we at present know little or nothing, although in the investigation of this subject, a key, we believe, may possibly be yet found to the explanation of many of those malformations to which different parts of the body are subject.

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[1] Medical and Physical Researches, pp. 23, 24.

[2] Introd. ad. Hist. Nat. p. 416.

[3] Mém. de l'Acad. Roy. de Berlin, 1772.

[4] Heusinger's Zeitschrift für Organ. Phys. Bd. ii. s. 263.

[5] Burmeister's Entomology, s. 204; Burdach's Physiologie, tom. i. § 44, 48.

[6] Isis for 1817, p. 320.

[7] Spallanzani, Mém. sur la Resp. p. 268.

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[1] See Cyclop. of Anat. and Phys. Art MONSTROSITIES.

[2] Anat. Gén. tom. i. p. 609.

[3] Hist. des Anomal. de l'Organiz. tom. ii. 58.

Osiander[1] and Duges[2] have suggested that the variety of spurious hermaphroditism which consists of a division of the perineum in the male, may be produced *mechanically* in the embryo by the preternatural accumulation of fluid in the urinary canal, from an imperforate state of the urethra, and the consequent distension and ultimate rupture of the urethra, &c. From cases published by Sandifort, Howship, Billard, and many others, we are now fully aware of the fact that all the urinary canals of the fœtus in utero are occasionally found morbidly distended with a fluid, which, according to the interesting observations of Dr. Robert Lee,[3] would appear to possess the more characteristic qualities of urine. We have dissected one case in which the dilated fœtal bladder was as large as an orange, and have in the Anatomical Museum of Dr. William Hunter at Glasgow, the preparation of another instance in which the bladder of a full-grown fœtus was dilated to the size of that of the adult subject. In one case mentioned by Dr. Merriman, the distended organ contained half a pint of urine,[4] and in another detailed by Mr. Fearn, it was capable of containing as much as two quarts of fluid.[5]

It is not impossible that the causes in question—namely, the obliteration of the urethra and the consequent distension of all the urinary passages, and probably also of the sexual canals communicating with these passages—may occasionally produce in the male embryo a reopening of the perineal fissure, giving thus to the external parts the appearance of a female vulva, and perhaps at the same time may lead to the retention and imperfect development of the testicles by the distension of their ducts, and the unusual compression to which these organs may be subjected. Indeed, we have satisfactory evidence, in a few instances, that such a cause may have been in operation, by our detecting the other acknowledged effects of the urinary accumulation in question—such as preternaturally dilated ureters, and a cystic form of the infundibula of the kidneys, as in a case of hermaphroditism given by Mayer, in a human fœtus,[6] in the kid described by Haller,[7] and in the child whose case we have already quoted from Steghlener. (See page 258.)

At the same time, the total absence of these collateral proofs in ost other cases of hypospadias, our knowledge of the fact that the erineal aperture is in some cases never shut, and the difficulty of onceiving the possibility of its being reopened when once it is irmly closed, are perhaps sufficient to show that the cause or causes lluded to, produce in but few if any instances, the effect here attriuted to them.

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[1] Neue Denkw. für Aertzte und Geburtsh. Bd. i. pp. 264, 267.

[2] Ephem. Méd. de Montpellier, tom. v. pp. 17, 45, and 52.

[3] London Med.-Chirurg. Trans. vol. xix.

[4] London Med. and Phys. Journ. vol. xxv. p. 279.

[5] Lancet for 1834–35, p. 178.

[6] See p. 8, of Icones, &c.

[7] Comment. Soc. Reg. Sc. Götting. tom. i. p. 2.

We deem it not uninteresting to point out in this place, under the question of the origin of hermaphroditic malformations, a circumstance which has struck us in considering one or two of the cases in which the sexual apparatus of one side of the body was more imperfectly developed than that of the other, viz., that the opposite side of the encephalon was at the same time defectively formed. Thus in the case of Charles Durge, on the right side of whose body there was a well-formed testicle, and on the left an imperfect ovary, the right hemispheres of the cerebrum and cerebellum, but particularly of the latter, were found by Professor Mayer to be smaller and less developed than the left, and the left side of the occiput was externally more prominent than the right. The same author, in the account of his case of hermaphroditism in a person eighteen years of age, which we have previously quoted,[1] and where there was an imperfect testicle, &c., on the right side, but no trace of testicle or ovary on the left, incidently mentions that the right side of the cranium was somewhat prominent—"dextra pars cranii paullulo prominet," in correspondence, there is every reason to believe, with a slight predominance in size in the hemispheres of the encephalon of the same side. In adducing these two cases, we do not wish to draw any inference with regard to the relation of causation between the size and development of the encephalic mass and the determination of the sex, but would merely point out the facts themselves in the meantime, for the purpose of drawing attention to the subject in the observation of any future similar instances that may happen to occur.

In connection with the question of the causes of hermaphroditism, it is interesting to remark that in some instances, malformations of the genital organs giving rise to appearances of hermaphroditism have been observed to be *hereditary* in particular families, both on the maternal and paternal sides; and in other cases to occur among several of the children of the same parents. Thus Heuremann[2] mentions an example of a family, the females of which had for several generations given birth to males who were all affected with hypospadias; and Lecat[3] alleges that a degree of hypospadias is not uncommon among families in Normandy. In Rust's Magazin an instance is related of a degree of hypospadias existing in a father and son.[1] Baum,[2] in his essay on congenital fissures of the urethra, has referred to two instances of the existence of hypospadias in brothers of the same family, the first mentioned by Walrecht, and the second by Gockel.[4] Sir Everard Home[5] found two cases of hypospadias in two children belonging to the same parents. Kauw Boerhaave[6] mentions an example of four hypospadiac brothers, and Lepechin another instance of three.[7] Naegele has reported a case in which two male twins were both hypospadiac;[8] and Katsky[9] and Saviard[10] have mentioned similar instances.

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[1] Icones, p. 12.

[2] Medicin. Beobacht. Bd. ii. s. 234, and Laroche, Sur les Monstrosités de la Face, p. 30.

[3] Arnaud, loc. cit. 312.

When treating of transverse hermaphroditism, we have already alluded to another fact, long and extensively known among our agriculturists, but first prominently brought before the notice of physiologists by Mr. Hunter, that the free martin cow, or cow that is born co-twin with a male, is generally barren and has its sexual organs more or less defectively developed or hermaphroditically formed.[11] In three different instances, Mr. Hunter confirmed the fact of the anomalous sexual development of such animals by dissection; and Scarpa[12] and Gurlt[13] have published some additional observations and cases. We have lately had an opportunity of dissecting the sexual parts of two adult free-martins, and found them as already detailed, formed after an abnormal and imperfect sexual type; and our friend Dr. Allen Thomson made some years ago a similar observation upon a free-martin twin fœtal calf. Cases, however, exceptional to the general fact of the sterility and imperfect sexual conformation of the free-martin twin cow are not unfrequently met with. Mr. Hunter found the sexual organs of a free-martin calf that died when about a month old, apparentally naturally constituted. He speaks also of having heard of some free-martins that were so perfectly formed in their sexual parts as to be capable of breeding; and different instances of their fecundity have been published by Dr. Moulson and others[14] since the time that Mr. Hunter directed attention to this subject. In some pretty extensive inquiries which we have made in regard to this point among the agriculturists of the Lothians, we have d only of two instances in which free-martins proved capable pagating, and such cases seem to be always looked upon as ng exceptions to the general rule.

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[1] Magazin für die Gesammte Heilkunde, Bd. xviii. s. 113.

[2] De fissuris urethræ virilis congenitis, p. 54.

[3] Burdach's Metamorphose des Geschlechter, p. 52.

[4] Eph. Nat. Cur. Dec. ii. ann. 6, 1686, p. 85.

[5] Comp. Anat. iii. p. 320.

[6] Nov. Com. Acad. Sc. Petropolit. tom. i. p. 61, tab. xi.

[7] Ibid., tom. xvi. p. 525.

[8] Meckel's Archiv, Bd. v. s. 136.

[9] Acta M. Berol. Dec. 1. tom. ix. p. 61.

[10] Observ. Chirurg. p. 284.

[11] See also vol. i. of this work, p. 286.

[12] Mem. della Societa Italiana, tom. ii. p. 846.

[13] Lehrbuch der Pathol. Anat. Bd. ii. s. 188.

[14] Loudon's Magazine of Natural History, vol. v. p. 765. See also Youatt on Cattle, p. 539, Farmers' Magazine for Nov. 1806 and Nov. 1807.

are not aware that among other uniparous domestic animals, goat, mare, &c., when a female is born co-twin with a male, male is sterile, and has its sexual organs hermaphroditically d, as in the free-martin cow; and we are sufficiently assured o such law holds with regard to twins of opposite sexes among . Sir Everard Home, in his essay on monstrous formations,[1] ions that in warm countries, nurses and midwives have a pre- that such women as have been born twins with males seldom ; and we have found the same prejudice existing to a con- ble degree among the lower orders in Scotland. Mr. Cribb[2] mbridge published, in 1823, a short paper in order to refute otion so far as regarded the human subject. He refers to the ies of seven women who had been born co-twin with males. f these had children, and the remaining seventh subject alone een married for several years without any issue. We have lves made a series of extensive inquiries of the same nature as published by Mr. Cribb, and have obtained authentic infor- n regarding 42 adult married females who had been born as with males. Of these, 36 were mothers of families, and 6 had ildren, though all of them had been married for a number of . Two of the females who have families were each born as a t with two males.[3] In the Medical Repository for 1827, an mous author has mentioned an instance of quadruplets con- g of three boys and a girl, who were all reared; the female vards became herself the mother of triplets. Limited as the o which we here allude confessedly are, they are still amply ient to show that in by far the majority of cases the females of of opposite sexes are in the human subject actually fertile, as some of the cases we have collected show, they are occa- lly unusually prolific.

to the cause of the malformation and consequent infecundity e organs of generation in the free-martin cow, we will not re to offer any conjecture in explanation of it. It appears to be one of the strangest facts in the whole range of teratolo- science, that the twin existence in utero of a male along with ale should entail upon the latter so great a degree of malfor- n in its sexual organs, *and in its sexual organs only.* The cir-

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mp. Anat. vol. iii. pp. 333–34.

[2] London Med. Repos. vol. xx. p. 213.

tes of the histories of these cases individually were read to a meeting of the Royal l Society of Edinburgh in the beginning of 1837 See additional cases and remarks question in vol. i. p. 321, &c.—(Ed.)

cumstance becomes only the more inexplicable when we consider this physiological law to be confined principally or entirely to the cow, and certainly not to hold with regard to sheep, or perhaps any other uniparous animal.

The curiosity of the fact also becomes heightened and increased when we recollect that when the cow or any other uniparous animal has both twins of the same sex, as two males or two females, these animals are always both perfectly formed in their sexual organization, and both capable of propagating. In the course of making the preceding inquiries for females born co-twin with males in the human subject, we have had a very great number of cases of purely female and purely male twins mentioned to us, who had grown up and become married, and in only two or three instances at most have we heard of an unproductive marriage among such persons.

Further, we may, in conclusion, remark that among the long list of individual cases of hermaphroditism in the human subject that we have had occasion to cite, we find only one instance[1] in which the malformed being is stated to have been a twin. Katsky, however, Naegele, and Saviard have each, as before stated, mentioned a case in which both twins were hermaphroditically formed in their sexual organs.

HERMAPHRODITISM IN DOUBLE MONSTERS.

One of the most curious facts in the history of double monsters is the great rarity of an opposite or hermaphroditic sexual type in their two component bodies, the genital organs of both bodies being almost always either both female or both male.

Physiological science affords us at present no satisfactory clue to the explanation of this singular circumstance. From two cases of double monstrous embryos observed in the egg of the domestic fowl by Wolff[2] and Baer,[3] and from a similar case met with in the egg of the goose by Dr. Allen Thomson, it appears certain that double monsters originate upon a single yolk, probably in consequence of the existence of two cicatriculæ, more or less complete, or of two germinal points, upon a single germinal vesicle, or of two germinal vesicles upon a single yolk.[4] In such a case the two bodies of the double monster are so early and intimately united together as to form, almost from the commencement of development, a single ystem; and therefore the fact of the uniformity of their sexual haracter is the less remarkable.

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[1] Eschricht's case of transverse hermaphroditism, see p. 247.

[2] Nov. Comment. Acad. Petropolit., tom. xiv. p. 456.

[3] Meckel's Archiv für Physiologie, &c. for 1827, p. 576.

[4] We have in our possession a preparation from a duck's egg, in which two full-grown fœtuses are developed on opposite sides of a *single* yolk of the common size.

The fact itself, however we may explain it, of the comparatively xtreme rarity of both male and female sexual organs upon double monsters seems sufficiently established by various careful investiations made into the subject. Thus out of forty-two perfectly double monsters which Haller[1] was able to collect at the time at which he wrote, there were only two that were supposed to be of double sex, or, in other words, that had one body male, and the ther female. Among double-headed monsters with single lower xtremities, he found a hermaphroditic type more common, and adduces three examples of it.

In reinvestigating this matter, the late Professor Meckel[2] could discover among the numerous class of monsters with perfectly double bodies united anteriorly or laterally by the thorax and abdomen, only one very doubtful case of exception to the above general fact. In the class of double monsters united in the region of the pelvis, he mentions two exceptional cases from Valentin[3] and Hasenest;[4] of double-headed monsters with single bodies, he quotes three similar cases from Lemery,[5] Bacher,[6] and Bilsius;[7] and of monsters with a single head and double body, he adduces two cases from Brissæus[8] and Condamine,[9] in which in a like manner one body of the monster was supposed to have female, and the other male sexual organs. Several of these cases, however, certainly rest upon too doubtful authority and insufficient observation.

Isidore St. Hilaire has still further extended the data on which he above general fact is founded, by showing that the same uniformity of sex holds good with respect to double parasitical monsters[10] and even in monstrosities double by inclusion. Thus out of this ast interesting class of double monsters, he alludes[11] to ten distinct cases in which the sex of the included being was ascertained. In six out of these ten cases the including and included body were both male; and in the other four they were both female.

On the whole, therefore, we must consider as founded on a proper nduction from the existing data, the axiom of Meckel,—"Sexuum diversorum indicia in eodem organismo, quantumvis duplicitate peccet, non dari, sed unum tantum observari."[12] But while all the data hitherto collected with regard to this subject would seem to point it thus out as one of the most constant and best ascertained laws in teratology, still we are not altogether disposed to consider it with Zeviani[1] and Lesauvage[2] as subject to no exceptions whatever. In the study of monstrosities, as in the study of other departments of medical science, we find many general, but no universal laws.

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[1] Opusc. Anat. 1751, p. 176.

[2] De Duplicitate Monstrosâ p. 21.

[3] Eph. Nat. Cur. Dec. ii. ann. iii. p. 190.

[4] Comment. Lit. Norimb. 1743, p. 58.

[5] Mém. de l'Acad. des Sc. de Paris, for 1724.

[6] Roux' Journ. de Méd. 1788, p. 483.

[7] Blankaart's Coll. Med. &c. 1680.

[8] Six Observat. de M. Brisseau, Paris, 1734, p. 33.

[9] Mem. de l'Acad. des Sc. 1733, p. 401.

[10] Hist. des Anomal. de l'Organiz. tom. iii. pp. 235 and 386.

[11] Ibid. p. 311.

[12] De Duplic. Monst. p. 21.

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[1] Mem della Soc. Italian. tom. ix. p. 521.

[2] Mem. sur les Monstr. par Inclusion, Caen, 1829; or Archiv. Gen. de Med. tom. xxv. p. 140.

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SPONTANEOUS AMPUTATION OF THE LIMBS OF THE FŒTUS IN UTERO.

(From Dublin Journal of Medical Science, November, 1836, p. 220.)

In the first and second volumes of the Dublin Medical Journal, Dr. Montgomery has offered some interesting observations on the spontaneous amputation of the limbs of the fœtus in utero. He has there alluded to Haller, Murat, Richerand, Desormeaux, and Billard, as having mentioned in their writings the occasional occurrence of supposed separations of portions of the fœtal limbs, but refers to three cases seen by Chaussier,[1] and one described by Mr. Watkinson,[2] as the only accurately detailed cases that he could find on record. To these, Dr. Montgomery has himself added two instances more, that had come under his own observation, and which appeared to him to show, that the phenomenon referred to, is the result of the constriction of the limb at the point of separation, by a ligature of organized lymph. As the subject forms a very curious question in intra-uterine pathology, I feel assured that I shall be excused for bringing forward the following additional cases;—and the more so on this account, that while they seem to add further evidence in favor of Dr. Montgomery's opinion, they at the same time serve to explain one or two circumstances which the instances recorded by him have left unresolved.

Case I.—In a Thesis by John U. T. Schaeffer, published at Erlangen in 1775, and entitled "Fœtûs cum Matre per Nervos Commercium," I find the following example of the spontaneous separation of the fœtal limbs. To the thesis is appended a full-length sketch of the fœtus, from which I have had copied, in the annexed woodcuts, the malformed parts of the extremities. A woman, æt. 32, brought forth, in a natural labor at the eighth month, a female child that survived for four hours. The head and trunk were normally formed, but the extremities exhibited the following peculiarities. The thumb of the right hand (Fig. 33, *a*) was somewhat thicker than usual, and its nail was wanting; the index finger was naturally formed; but the middle, ring, and little fingers were united together by a common membrane or ligament, which covered them down to their last phalanges, and the little finger had no nail upon it. The umb of the left hand, *b*, was also destitute of a nail; and its inner rface was united to the metacarpal part of the index finger. Of e index and middle fingers, only the third phalanges were pre-

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[1] Discours prononcé a l'Hospice de la Maternité, 1812; Dictionnaire des Sc. Médicales, tom. xvi. p. 70, and tom. xxiv. p. 259.

[2] London Medical and Physical Journal, vol. liv. p. 38.

Fig. 33.

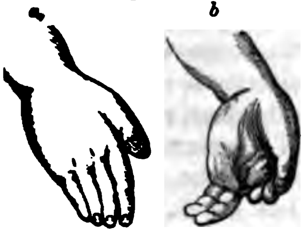
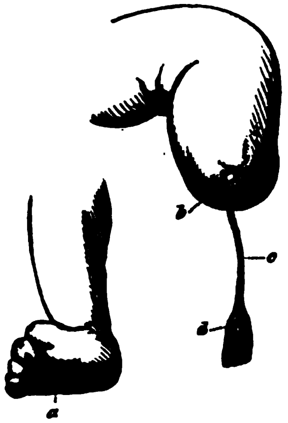


Fig. 34.



ent, and these intimately coalesced with one another. Small fleshy orpuscles supplied the place of the second and third phalanges. he ring and little fingers of this hand were natural. The great oe of the right foot (Fig. 34, *a*) was short and somewhat deformed —one of its phalanges appearing to be wanting, and a small but leep cicatrix occupying the situation in which its nail ought to have een placed. The second toe was also destitute of a nail, and had kind of membranous cord, *a*, attached to its extremity. The re-naining three toes of this foot were natural; the internal malleolus ras reddish, and not covered by cuticle. All the part of the left eg below the gastrocnemii seemed, as it were, amputated, and to ave the whole surface of the stump covered over with newly-formed uticle, a small central part only excepted, which was red and pre-ented the appearance of a recent wound. In this uncicatrized cen-ral part *b*, the ends of the tibia and fibula were visible; and in the pace between them, a dense fibrous cord, *c*, of some length, took ts origin, having attached to its further or loose extremity a small ody, which, on minute examination, proved to be the foot, of the ize of that of a fœtus of the third month, and with divisions on its inferior edge representing the five toes. On the superior part of the foot a minute cartilaginous-like corpuscle was placed, as represented in the figure *d*.

CASE II.—A case in some points resembling that given by Schaeffer, has been published by P. Zagorsky, in the Memoirs of the Imperial Academy of Sciences of St. Petersburgh for 1834.[1] A

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[1] Loc. cit. 6th Series, vol. iii. pp. 3–7.

malformed male fœtus was aborted about the fifth month of u
gestation, by a woman who had previously borne two well-fo
and healthy children. The head and face of the fœtus were gr
disfigured. The forehead and superior part of the head were
pied by two large, soft, reddish, oblong tumors, formed by an ex
sion and protrusion of the meninges, and containing cerebral
stance. The parietal bones, the squamous portions of the temp
the superior parts of the larger wings of the sphenoid, and of
occipital bones, were wanting, as well as the common integum
of the calvarium. The face was much malformed. The eyes
separated to an unnatural distance from each other, and the left
situated on a higher line than the right. The external parts of
nose were absent, but two foramina leading to the nasal pass
were situated high up on the face. The left cheek was larger
more prominent than the right, owing to its being swelled out
duplicature of skin arising inferiorly from the neck. The m
was large, and of a very irregular shape; but the tongue, fa
and larynx were naturally formed. The helix of the right ear
turned forwards, and united to the skin lying anterior to the me
auditorius externus. The external parts and internal organs of
chest and abdomen were natural. The prepuce was large, but
orifice small.

Fig. 35.

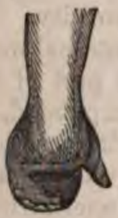
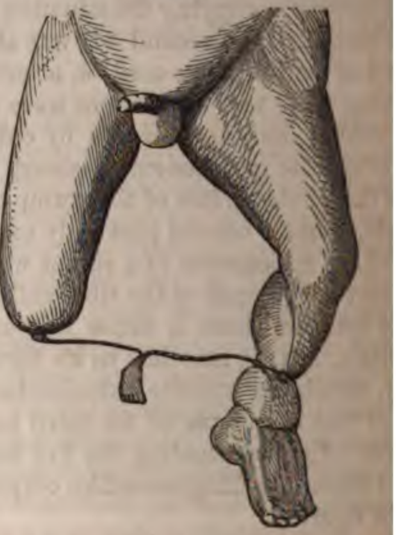


Fig. 36.



Both the upper extremities were normally formed as far dow
the hands, and the right hand was natural in all respects, excep
this,—that the internal edge of the first or proximal phalanx of

umb, was intimately united to the corresponding surface of the etacarpal part of the index finger, so that the palm appeared roader, while the thumb seemed shorter than natural, looking as composed of one phalanx only. The left hand (Fig. 35) was reatly mutilated—the thumb being present and natural, but the ngers altogether wanting. The inferior margin of the hand was ccupied by two little, contiguous, oblong-roundish bodies, representing imperfect fingers, but formed by productions of the skin lone. In the middle almost of the palm, two other transverse, nequally sized bodies were situated, united together at their internal margins, and connected also to another smaller body which arose at the ulnar side of the palm, and had the form of a jointed finger. These three last bodies were, like the two first, formed of skin only.

The leg of the right lower extremity (Fig. 36) was wanting, the thigh nding in a kind of rounded and cicatrized stump, and presenting in the centre of this stump a small, acuminated, projecting point. From this apex of the stump was prolonged a slender, thread-like membrane, strong in proportion to its size, which ran nearly transversely across, to below the middle of the left leg, which it encircled like a tightened ligature, constricting it so that a circular depression of considerable depth was formed in the leg at that part, while the portion of the extremity situated below the ligature was, with the appended foot, rather tumefied. From about the middle of this transverse, thread-like membrane an oblong-shaped body was suspended, which on more minute examination, proved to be the right foot, perfectly formed, as its general outline and five toes demonstrated, but not larger in size than the foot of a fœtus of the tenth or twelfth week. Besides, it was removed and dislocated laterally, as it were, from its natural position, being suspended from about the middle of the transverse cord by the heel. The other or left foot had the two large toes wanting, but was less remarkable for its defects of development, than for the abnormal and inverted position of its surface and margins, its sole being turned upwards, its dorsum or instep downwards, its external margin inwards, and its internal margin or edge being consequently placed on the outer side. Hence the three last toes which were present, appeared to be situated on the internal instead of the external side of the foot, and their lower surfaces were inverted upwards. At that margin of the foot which was situated internally, but was naturally the external, a slender membrane was perceived, prolonged from the heel to the little toe, and continuous above, on the back part of the leg, with the transverse cord which passed to the right thigh.[1]

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[1] This membrane and the different parts of the left hand are very imperfectly shown in the accompanying drawing, but they are equally defectively presented in the original full length view of the fœtus given by Zagorsky.

CASE III.—Beclard, in his essay on Acephali, contained in the Bulletin de la Faculté for 1817,[1] incidentally mentions the case of "a very deformed hydrocephalic fœtus, born in Paris, that had the middle and ring fingers of the right hand multilated; the middle one, however, still being kept attached to the stump by means of a filament. The legs were covered with reddish phlyctenæ. The left presented at its superior part a transverse cut that penetrated down to the bone, and resembled that which would have been produced by a tight ligature. The two opposite sides or surfaces of this indentation were both cicatrized and almost touching each other. It is evident," Beclard adds, "that if this fœtus had remained in utero for some time longer, it would have been born with an amputated and cicatrized leg, the remains of which might have been found in the liquor amnii."

CASE IV.—In a dissertation "Ueber mangelhafte Bildung der Extremitaten," by Albert F. Veiel, printed at Tubingen in 1829, among the list of cases given of defective development of the extremities, reference is casually made to an instance recorded in Froriep's Notizen,[2] of a fœtus "whose left foot was separated during pregnancy from the leg, and the forefoot was born by itself quite healed;" (*der linke Fuss während der Schwangerschaft sich von dem Beine ablöste, und der Vorderfuss für sich, bereits geheilt, geboren wurde.*) I regret that I have not an opportunity of consulting the volume referred to, in order to ascertain the more particular details of this case.

CASE V.—In his "Adversaria Medico-Practica,"[3] Ludwig has described and delineated a child that was born wanting a great part of the left upper extremity, but otherwise well formed. The arm appeared amputated about the middle of the humerus. To the rounded stump two soft papillæ were affixed, consisting only of prolongations of the skin, and a little fat. One of these papillæ is shown in the drawing given by Ludwig, and is represented as having a slight indentation or small cicatrix on its apex. On dissecting the multilated arm, its bloodvessels and nerves were found to become contracted as they approached its lower extremity, and at last were changed into a kind of fibrous tissue. The muscles of the arm appeared to become degenerated, like the bloodvessels and nerves, into a kind of fibrous or cellular structure, as they approached the surface of the stump. The os humeri was natural at its upper part, but below, it became somewhat broader and flatter than atural, and terminated in a rounded extremity with two slight late-il projections or tubera, which represented imperfectly, and on a ery diminished scale, the condyles of this bone.

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[1] Loc. cit. tom. v. pp. 513–14.

[2] Loc. cit. Bd. xii. p. 26.

[3] Loc. cit. tom. ii. p. 79.

I have myself had an opportunity of examining, during the pre-ent year, three instances of amputation of the forearm in utero, omewhat similar to that of Ludwig, but the mutilated extremity t the point of division presented, in each of them, in a much more arked degree, the exact appearance of an artificial stump, having he usual depressed and puckered cicatrices upon their surface, in-tead of the prominent and projecting ones met with in this case. In ll the three instances to which I refer, no deformity whatever ex-sted in any part of the body except in the mutilated arm; and the ubjects of the cases belonged to families, all the other members of rhich were perfectly formed in every respect. The following are ome of the more interesting particulars relative to each of these hree cases.

CASE VI.—Mary K——, at Dauffington near Queensferry, a fat nd healthy child, now fourteen months old, was born wanting the ight forearm and hand. The forearm is deficient from about half n inch below the elbow, the insertion of the biceps being left, and he elbow joint perfect, as shown by the free motions of the part. The remaining upper portion of the forearm presents exactly the ppearance of a very full and neatly-formed stump, the soft parts ffording a large cushion which covers and even overlaps the bones or half an inch. The situation of the two bones, however, is still well marked by two deep, puckered, converging cicatrices, seen on he lower surface of the stump, and leading down at their apices to the divided ends of the ulna and radius. The mother states that the arm presented the same appearance of perfect cicatrization at birth.

CASE VII.—Mary P——, a healthy child of between two and three years of age, living at Kinnaird in the neighborhood of Falkirk, was born wanting the right forearm from nearly the same point as in Case VI. The stump is less full and fleshy than in that case, and the divided ends of the bones of the forearm are consequently not so deeply buried in the soft parts. The truncated extremity of the ulna is felt somewhat enlarged, and the enlargement seems more on the anterior than on the posterior surface of the bone, so as to afford a useful fixed point for the child in her attempts to hold or move any object with the mutilated arm, which she is reported to use already with great activity. The portions of the upper part of the ulna and radius that are present seem not to be united together by bony matter, but can be made to move upon one another. On the surface of the stump there is an appearance of puckered cicatrices over the ends of the two bones, one of them being deep and depressed, like the usual sunk cicatrix of the umbilicus, but less in size, and another much shallower, and containing in it a very small flattish mass, of the consistence of cartilage. From one of these cicatrices a slight furrowed line runs forward for a short distance, to what almost appears to be a third cicatrix on the anterior part of the surface of the stump. The mother states that at the time of birth, a part over the ulnar or inner side of the stump was for a small extent raw or uncicatrized, but it skinned over in the course of a few days afterwards.

CASE VIII.—Margaret F——, Grangemouth, a married woman about forty-two years of age, and the mother of three well-formed children, two of whom are at present alive, wants the right forearm and hand, from about an inch and a half or two inches below the elbow, a deformity which has existed since birth. The stump is not very fleshy, but she uses it with extraordinary adroitness, being able to sew, wash, and perform every other kind of ordinary household duty with it. The portions of the upper parts of the radius and ulna which it contains, appear to be firmly united together at their corresponding surfaces by bony matter. The surface of the stump in respect to cicatrices nearly resembles, in almost every particular, that of Mary P.'s arm, only the cicatrices are not so very deep, and have more the appearance of perfectly natural skin, owing probably to the more advanced age of the person. Indeed the cicatrix over the end of the radius is more distinguished by the natural sulci of the skin at that part all radiating from it, as from a central point, than by any perceptible difference there in the structure of the cutaneous texture itself. In the cicatrix over the site of the ulna, there is a small circumscribed cartilaginous thickening. Mrs. F. is not herself informed as to whether any difference of appearance was observed in the cicatrices or stump at the time of birth.

In none of the three cases which I have just described, were any remains of the absent forearm or hand observed, and most probably they were not looked for or ever suspected to exist among the secundines, by the attendants—a subject which I shall again have occasion to revert to in the course of the subsequent remarks.

CASE IX.—I have had repeatedly an opportunity of examining another case of considerable deformity of the hand, which is not improbably referable to the kind of congenital mutilations now under consideration. Helen C——, a native of Bathgate, and now a child of between two and three years of age, was born wanting entirely

ex and middle fingers of the left hand. The skin at that
the hand from which these fingers should have projected
th, and the extremities of their two metacarpal bones are felt
eneath it, the second being shorter than the first. The ring
tle fingers of this hand are very intimately coherent with each
the thumb is small, and has, opposite apparently to the situa-
the joint between its first and second phalanges, a remarkable
r depression or indentation in it, of about one-third of the
erse diameter of the thumb in depth. The parts are alleged
presented exactly the same appearance in all respects at birth;
remains of the absent fingers were observed. The left arm
t kept pace with the right in growth, and is now nearly an inch
r than it. The different parts of all the other extremities are
lly formed.

E X.—In the Acta Academiæ Cæsareæ,[1] an example of mal-
tion of the hands is recorded by Aculoth, which is interesting
he similarity that it bears to the preceding case. There is no
ular description of the parts given by the reporter; but from
awing appended to the brief notice of the case, it appears
he thumb, index, and little fingers of the left hand, and the
and little finger of the right, were normally and perfectly
d. The middle fingers of both hands are altogether wanting.
eft ring finger seems as if deprived of its last phalanx and
nd, about the situation of the joint between its first and
l phalanges, it is almost completely divided by a very deep
r depression; so that the body of the second phalanx, which
he natural size and thickness, appears to be fixed to the extre-
f the first by a slender neck or pedicle of skin only. The ring
of the right hand presents at the same point an equally deep
r furrow; and, in addition, has what seems to be a diminu-
hird phalanx, unprovided with a nail, attached to the extre-
of the second by a similar slender pedicle. In the situa-
f the right index finger there is seen only a small roundish
about the size of this third phalanx of the ring finger,
connected to the metacarpus by a similar narrow neck or
us of integument.

judging in any doubtful case of deficient or deformed extre-
whether the defect which we observe be the result of a want
e original development of the absent parts, or the effect of a
ation or amputation in utero of these parts after they had been
actually formed, there are some circumstances which will in
instances, I believe, be found a sufficient guide to enable us

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[1] Loc. cit. tom. v. p. 181.

to come to a satisfactory decision of the question. When the malformation of the limb is the result of an arrest or deficiency of its original development, it would seem to be very generally observed, that one or more parts of the pertaining hand or foot, forearm or leg—sometimes, it is true, a very small part only, as portions of a single finger or toe, or some bones of a carpus or tarsus—are affixed to the extremity of the defective limbs; these more peripheral parts appearing very early in the natural process of the development of the extremities, and not unfrequently being seen to exist alone, in those cases of their arrested or deficient development, in which no other part whatever of the limb is present, in consonance with the law of the development of organs from their circumference to their centre, pointed out at such length by M. Serras, but probably not by any means so universally applicable as he himself seems to believe it. In the cases, again, in which the deformity of the extremity has been produced by the process of spontaneous amputation, the end of the mutilated limb will be found to present the appearance of a common artificial stump, in so far that the soft parts and bone are perfect up to the point of separation, and have no parts of the limb, that are naturally situated anterior or distal to this point, attached to them. It is evident that when the deformity is confined to the fingers or toes alone (unless it happens that the third phalanx, as characterized by its nail, form the part of the finger or toe that is present, and thus authorize us to refer it to an arrest of development), the above criteria will not be sufficient to enable us to distinguish between malformations originating in that cause, and mutilations produced by spontaneous amputation. In most such cases, we must, I conceive, be chiefly guided by the appearances of the surface of the stump at the time of birth, and the fact of the adjoining fingers or toes presenting one or more deep circular depressions, as indicative of the process of separation going on in them. It is principally, at least, from this latter circumstance, that I have ventured to include the ninth and tenth cases among those of spontaneous amputation; at the same time it must be allowed, that the absolute diminutive bulk of the whole left arm, and, I may add, of the left side of the chest also, in the girl C——, cannot but be considered as a strong argument in favor of the deformities of her hand depending in some degree on a defective development.

In the three first cases which I have quoted, and the same remark partly applies also to the ninth and tenth, the division of the limbs and fingers at the strictured parts, was not so complete as to have produced their entire separation. The same was the fact in regard to both the instances seen by Dr. Montgomery. In Mr. Watkinson's case, in one of Chaussier's, and also, I presume, in the instance in

Froriep's Notizen, referred to by Veiel, the division was complete at the time of birth, and the amputated portions of limb were discharged along with the fœtus from the uterus. In other examples, as in that given by Ludwig, and in the cases that I have brought forward (VI., VII., VIII.), the evidence of the amputation has been so far incomplete, that the separated portions of limb were not observed by the attendants;—a point in the history of all the cases of mutilated fœtal extremities that Haller could find on record at the time at which he wrote, and which he has collected in his Elementa Physiologiæ,[1] that led him entirely to reject the idea of such deformities ever depending on spontaneous amputation in utero. "No author," he observes,[2] "has cited an instance in which the amputated hand, or other limb, was found in the membranes, separated from the body of the fœtus; and yet in the more advanced terms of pregnancy the fœtus is of no small size, and no powers of nature could annihilate the absent or separated portion of limb." With reference to this remark, the two first cases that I have brought forward appear to be very important, because the circumstance of the separated part of the limb being sometimes stopped in its development at a comparatively early stage of intra-uterine life, as seen in the left leg and foot in Schaeffer's, and in the right in Zagorsky's case, may serve to explain how the oversight might very easily be made. For if the cord or membrane uniting this undeveloped part of the limb to its corresponding extremity, should happen to become completely divided, either during the course of pregnancy—in the act of parturition—or immediately after birth, it is evident that on account of the arrested formation and minute size of the part itself, it might be difficult to detect it among the secundines and discharges, even though it were carefully looked for.

The cases described by Schaeffer and Zagorsky appear to support strongly the idea entertained by Dr. Montgomery, that the amputation of the limbs of the fœtus is, in some if not in all instances, the result of constriction by a cord or ligature thrown around the limb at the point of disjunction; and the language which Beclard employs in the description of his case (see Case III.) would almost seem to show, that he held a somewhat similar opinion with regard to the agency employed in the process of separation. And here it may not be unworthy of remark, how similar in some respects the ligatures or cords were in the two cases referred to; for if we suppose, what was very probably the case, that in Schaeffer's child the portion of cord attached to the second toe of the right foot (*a*, Fig. 14) was originally adhering by its loose extremity to the small eminence, *d*, seen on the superior part of the left and undeveloped foot, this left foot would, before the attachment was lacerated, have exhibited an appearance of connection and suspension, analogous to that presented by the right foot in the monstrous fœtus described by Zagorsky.

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[1] Loc. cit. tom. viii. p. 139.

[2] Ibid. p. 140.

As to the nature of the bands or cords which constitute the constricting ligatures, there seems no reason to doubt that they are, as Dr. Montgomery has suggested, formed of organized lymph; and that this lymph has been effused by inflammatory action is, I am inclined to think, in the highest degree probable. No one would now attribute such morbid bands or cords, if they were found in other parts of the body, as in the cavities of the chest or abdomen, to any other morbid action than that of inflammation; and that the cutaneous texture of the fœtus in utero is liable to inflammation attended with the effusion of organizable lymph is known from the circumstance of different parts of the surface of the fœtus being occasionally found adhering, either directly, or by the medium of false membranes, to the amnion covering the placenta, as seen in a number of cases which I have elsewhere collected,[1] to some part of the umbilical cord, as described by Polo, Meckel, Velpeau, and others, or to other parts of its own body, as observed in the case mentioned by Morlanne.[2] Further, that organizable lymph, when effused by inflammatory action between two contiguous parts of the surface of the fœtus, is capable of being changed into pseudo-membranous bands or cords, similar to those observed in Dr. Montgomery's first case, and in the cases described by Schaeffer and Zagorsky, appears from the circumstance of lymph occasionally assuming this form, when effused between the contiguous points of such surfaces as have a free motion on each other, as is sometimes seen in the bands of false membrane found stretched from one point to another of the cavities of the pleura, pericardium, and peritoneum. Besides, it has been particularly noticed, that in some of those instances in which one or more points of the surface of the fœtus has been morbidly united to the membranes or umbilical cord, the effused organizable lymph forming the band of connection between the adherent parts has, probably from the free motions of these parts upon each other, and the consequent stretching to which the pseudo-membrane was constantly subjected, taken more or less completely that form which the cords in the above cases presented. Thus Meckel[3] mentions a preparation in his possession, showing, in a fœtus of the sixth month, the dura mater protruded through an enlarged occipital foramen, and morbidly united to the umbilical cord through the medium of a strong thread (*starker Faden*) of membrane, six inches in length. Gurlt[1] has described and represented a variously malformed fœtus of the goat, which had been adherent at different points of its surface to the amnios. One of the bands of adhesion, about an inch and a half in length, is represented as arising from the upper lip, and has exactly the appearance of a cord or thread in its middle portion, and a more dilated and membranous form at either extremity.

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[1] Essay on Diseases of the Placenta, Edinburgh Med. and Surg. Journ. April, 1836, p. 307. See also a future page.

[2] Gardien, Traité des Accouchemens, tom. ii. p. 173.

[3] Handbuch der Pathologischen Anatomie, Bd. ii. s. 138.

In the present state of our knowledge with regard to the diseased conditions to which the fœtus is subject, it would be difficult, or indeed altogether impossible, to determine what may constitute the active or exciting cause of the apparently local inflammation which gives rise to the exudation of that organizable lymph that constitutes the constricting pseudo-membranous cords, in such cases as those under consideration; but there are some facts connected with the *seat* of the adhesions, or origin of the cords, that may ultimately lead to some knowledge on this point, and which seem, at least in the mean time, not unworthy of being particularized. In the upper extremities, the root of the fingers, or middle of the hand, appears to form a very frequent locality for the seat and effects, if not for the origin, of the inflammatory action which gives rise to the constricting bands of false membrane. Thus in Dr. Montgomery's first case, the threads of lymph, were placed as a complete ligature round the middle of each hand, causing a distinct depression in the constricted part; in Zagorsky's case, all the fingers of the left hand were amputated at their roots, and the inside of the thumb of the right hand was partially adhering to the side of the palm; in Schaeffer's child, the thumb of the left hand was united in the same manner to the metacarpal part of the index finger, and the two last phalanges of the index and middle fingers were wanting, while the middle, ring, and little fingers of the right hand were held together by a common membrane; in the girl C—— (Case IX.), the ring and little fingers of the left hand are coherent, while the index and middle fingers are separated from their roots, and the thumb as its second joint has a circular indentation in it, which existed at birth, and was not improbably formed by the constriction of the ligature that had separated the two last mentioned fingers. In Beclard's hydrocephalic fœtus, the right ring finger was wanting, and the middle one nearly separated by a deep circular depression; and lastly, in Aculoth's case, the ring fingers of both hands were very deeply divided with similar circular depressions or furrows, the two middle fingers were entirely wanting, and the right index finger nearly so.

The upper third of the forearm has now formed the seat of the amputation in five cases, viz., in two of Chaussier's, and in three of the instances (Cases VI., VII., and VIII.) which I have met with. While the lower third of the leg seems to have been the most frequent point for its occurrence in the lower extremities, it having now been observed in this situation in Dr. Montgomery's two cases, in Mr. Watkinson's, in Schaeffer's, and in the left leg of the monster described by Zagorsky. In the instance mentioned by Béclard, the seat of stricture was at the upper part of the leg; in the right lower extremity in Zagorsky's case, it was above the knee; and in Ludwig's and one of Chaussier's cases, the amputation had occurred in the course of the humerus.

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[1] Lehrbuch der Pathol. Anat. der Haus-Säugethiere, Bd. ii. s. 133, and Tab. vii. Fig. 1.

Allowing what there seems every reason to believe, that the pseudo-membranous threads or bands found at the seat of separation or amputation of the fœtal limbs, form the active physical agents employed by nature in the production of this effect; it may still be allowed difficult to conceive how they should produce such a strong constriction, and consequent atrophy, of the member at the point of ligature, as leads not only to the partial but even to the entire disjunction of the limb at that point. With reference to this subject, it is necessary to recollect, how readily the atrophy or interstitial absorption of any living texture is produced by the application to it of a continuous and strong pressure. Besides, in the earlier months in particular, of intra-uterine life, the limbs of the fœtus are so slender, and their component tissues so soft, as to afford great facilities for the disjunctive action of the constricting pseudo-membrane. We must further remember that, supposing the organizable lymph, forming one of these constricting false membranes or bands, to be once effused, so as to have its two extremities attached to two parts of the body, more or less distant from each other, as to the parts of two limbs, it is evident that the texture of the pseudo-membrane must itself soon become stretched, and at the same time compress more or less forcibly those parts or surfaces of the fœtus over or around which it may happen to pass, in proportion as the two points of the body forming its origin and insertion become gradually more and more separated from each other, in the regular progress of development. The sudden movements also of the fœtus, in the latter months of pregnancy, may contribute in some degree to the same effect.[1] This stretching of the cord of pseudo-membrane is not only capable of producing a constriction; but also in some circumstances an elongation of the limb; as seen in Schaeffer's case (Fig. 34, *c*). Upon the right foot of the fœtus described by Zagorsky, an interesting effect has been produced apparently by this same cause. The pseudo-membrane attached to it seems to have been originally affixed along the inner side of the heel, and is stated to have stretched from thence as far forward as the little toe. In its course to the opposite extremity it had passed round the outer and across the fore part of the leg; and when stretched, had, in consequence of its connections and course, produced the effect of turning the included portion of leg and foot half round upon its axis, so as to bring the inner ankle to the situation of the outer, and to invert the surfaces of the foot in the manner before mentioned. (See Case II. and Fig. 36.) Both in the child described by Mr. Watkinson, and in Dr. Montgomery's second case, the foot which was not the seat of the amputating process was turned partially inwards.

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[1] It is curious, and, at the same time, so far as regards the mechanism of the process, not unimportant, to connect our knowledge of the method employed by nature for the separation of the limbs of the fœtus, with the mode of amputation without bloodshed, practised in the fourteenth century by Guy de Chauliac, by applying to the joint situated more immediately above the seat of disease, a ligature so tight as ultimately to cause all the portion of limb below the point of constriction to drop off. The same method of amputation was again brought under the notice of German surgeons, towards the end of the last century, by Wrabetz and

The sketch given in Fig. 36 of the left leg in Zagorsky's case, may serve also to illustrate a remark make by Dr. Montgomery, as to how it happens that in cases of amputation of the fœtal limbs, the surface of the stump, and even of the amputated portion of the extremity, seem nearly or entirely cicatrized; for, it is evident, that by the constricting ligature gradually carrying in before it, at the point where it encircles or compresses the limb, a duplicature or process of the cutaneous texture, and from this texture constituting (as it certainly appears to do) one of the last that is removed by the disjunctive atrophy, both sides of the deep indentation or furrow, formed at the seat of constriction, must be found regularly covered over by the elongated and depressed integuments as far almost, in most cases, as the bone itself; and even the small portion of the stump which is left raw by the division of the last parts, may be observed in some instances invested with a true cicatrix at the time of birth; provided the separation of the limb has taken place at such a period before the expulsion of the child from the uterus, as to allow of this cicatrization being completed.

The view that has been taken of the disjunctive action which we have attributed in the preceding remarks to the forcible and gradually increasing constriction of the bands of pseudo-membrane upon the fœtal limbs, seems confirmed, in no inconsiderable degree, by what is occasionally observed to occur when the same kind of con-

Plouquet, and recommended by the latter as more particularly adapted to the case of emaciated and timid subjects.—(See Sprengel's Histoire de la Médecine, tom. vii. pp. 313 and 333; Cooper's Dictionary of Surgery.) During last year, in the experimental rooms of M. Amussat at Paris, I had an opportunity of seeing M. Mayor, the distinguished surgeon of Lausanne, attempt to cut off the limbs of dogs by forcibly tightening around them a circular ligature of strong wire.

striction is exercised upon any part of the fœtus by the *Umbilical Cord.* From the facility with which the smooth surface of the cord moves over that of the fœtus, it is of extremely rare occurrence that the convolutions of the former around the neck, body, and extremities of the latter, ever come to act as a constricting or compressing agent upon the parts over which they pass, although such an effect has been occasionally observed, while the surface of the cord still remained healthy, when its coils have accidentally become so very firmly entangled and knotted with one another, or with the fœtal limbs, as to prevent its own motions, as seen in a remarkable case represented and described by Van de Laar,[1] in which the arms of an aborted fœtus were curiously displaced and distorted, from having been thrown transversely across the back and twisted in that position, by some duplicatures of the navel-string. When the surface of the cord, however, becomes, as occasionally occurs, morbidly adherent at some point to the surface of the fœtus, the state of matters is necessarily much changed; the portion of cord between the umbilicus and point of adhesion is, as it were, fixed, and the parts which it happens to be stretched over and encircle in its course are liable to become compressed and constricted by it, during the progress of the natural enlargement of the body, in the same manner as we have already seen them to be by the ligatures or bands of pseudo-membrane. Thus Meckel[2] mentions the case of an aborted fœtus of the third month, whose umbilical cord, after being imbedded to the depth of half an inch in the parts of the right scapular region, stretched from thence downwards to the right thigh and leg, to which it was intimately united by morbid adhesions. Wrisberg[3] has delineated, and very minutely described, a case which is still more in point, in reference to the present subject. In a deformed fœtus of the fourth month, the umbilical cord, on leaving the abdomen, ran first over the left shoulder, and around the back of the neck, and then came to encircle completely the right upper extremity immediately below the shoulder. To all these different parts it was morbidly adherent, and at the point where it encircled the right arm, it imbedded itself deeply in the subjacent soft parts (*carnes subjacentes valde stringit*). In its subsequent course the cord, after running again over the left shoulder, returned a second time to the right arm, and crossed over it above the elbow, impressing another furrow or indentation upon it at the point of contact. In this case we have an example of the process which produces the spontaneous amputation of the limbs of the fœtus, going on at two different points in the same arm, and it differs from the instances previously detailed in this respect only, that the constricting and dividing agent was not, as in them, a band of false membrane, but a portion of the umbilical cord.

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[1] Observationes Obstetrico-Medicæ, p. 41, and Tab. ii.

[2] Pathologischen Anatomie, Bd. ii. s. 137.

[3] Sandifort's Thesaurus, tom. iii. p. 235, Tab. ii. fig. 5.

At one time I was inclined to believe that spontaneous amputation of the limbs of the fœtus might be occasionally produced in another mode, namely, by previous *Fracture* of the bone; and this opinion was suggested by the following case. In 1834, a boy, at that time seven years of age, was exhibited in Edinburgh, wanting both arms and hands. From the right shoulder, one finger, containing four phalanges, and from the left two other shorter fingers protruded. The two fingers attached to the left shoulder were situated vertically over one another, and the inferior presented at one point in the course of its first or proximal phalanx, a great and sudden narrowing or contraction. On making some further inquiries, it appeared that this last finger had been fractured some years before by a fall, and had gradually from that time become more and more atrophied at the fractured part, so much so, that if the same process continued much longer, it seemed not improbable that the pedicle or neck of integuments attaching the finger, might ultimately come to be completely divided, and consequently all the portion of the member anterior to it be allowed to drop off. The circumstances of this case readily suggested the idea that the similar constricted appearance, and occasional complete amputation, of the limbs of the fœtus, might possibly in some instances originate in the same cause. We know from various accurately recorded instances, that fractures do occur in the bones of the fœtus in utero, as an effect of physical injury, and also spontaneously, if we may trust to the cases observed by Chaussier and Baudelocque, and it seems possible, at least, that under some circumstances this fracture may be attended with such lesions of the surrounding soft tissues, as may give rise to a kind of disjunctive atrophy or absorption similar in its nature and effects to that which had occurred in the finger of the boy, and, perhaps, in the textures of the fœtal limb, still more active and speedy in its operation. At the same time it must be admitted, that in none of the instances hitherto recorded of fractures of the bones of the fœtal limbs, does such an atrophy of the surrounding soft parts appear to have been observed, if we may not be allowed to except the case given by Amand, who found in a fœtus of the fourth or fifth month the bones of the forearm, thigh, and leg separated and mobile, as if they had been fractured by design, and the parts seemingly held attached by the medium of the integuments only.[1]

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[1] Nouvelles Observations sur la Pratique des Accouchemens, p. 92, obs. viii.

In his first communication,[1] Dr. Montgomery has referred to a passage in Haller's Elementa Physiologiæ,[2] in which that author casually alludes to some expressions of Roederer, in such a way as led Dr. Montgomery to suppose, that this last writer might have described the compression of the limbs of the fœtus by cords of organized lymph. Having access to the work of Roederer referred to by Haller, I beg to subjoin the passage alluded to, only remarking that the language of the author is not so definite as to leave the matter entirely without doubt. It is contained in an essay, "*De vi Imaginationis in Fœtum negatâ.*"[3] After speaking of the supposed resemblance of certain deformed human fœtuses to apes, he observes, that the comparison is not so far fetched, for "*femora abdomini applicantur, reflectuntur crura, ad faciem eriguntur manus, retractis cubitis. Inquirens in structuram corpusculi ligamenta strictiora notavi, artusque compressos.*"

Since the first publication of the preceding essay, in 1836, there have been many additions to the literature of spontaneous amputation, and various instances of the *three* preceding modes of its production adduced. 1. Cases in which the umbilical cord acted, as suggested, as the amputating agent, have been recorded by Drs. Montgomery, Schwabe, Buchanan, &c. 2. Instances in which bands of coagulable lymph formed the constricting agents, have been published by Smith, Nettekoven, Dubois, and others; some years ago, I showed a recent example of this kind to the Obstetric Society.[4] 3. Martin has lately described and figured a case, proving the truth of the suggestion offered above, that spontaneous amputation may sometimes originate in fracture and injury of the extremity of the fœtus. In his case, the mother had fallen from a ladder eight weeks before delivery, and the child was born with the left arm amputated near the shoulder, and the wound still not entirely closed. The separated, but nearly full-sized arm was expelled with the placenta.

ON THE RUDIMENTARY REPRODUCTION OF EXTREMITIES

AFTER THEIR SPONTANEOUS AMPUTATION.

On the stumps of limbs that have seemingly undergone an early spontaneous amputation in utero, there is often seen a species of anormal structure, which has not yet, so far as I am aware, been described in any existing work on the subject of monstrosities. I lude to the appearance on the end of many such stumps of a procting mass or nodule, varying in size from a small cutaneous dge to the bulk of a walnut, and having protruding from its surce one, two, or more, still smaller fleshy divisions or projections, hich are provided at their extreme points with nails.

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[1] Dublin Journ. of Medical Science, vol. i. p. 143.

[2] Loc. cit. tom. viii. p. 135.

[3] Opuscula Medica, p. 120.

[4] See details regarding this case in Monthly Journal for 1848, p. 891.

[5] See Monthly Journal for July, 1841, p. 537, and for June, 1848, p. 891, &c.

This variety of anormal structure is by no means rare. Several ears ago, when searching for instances of it, I found five or six ving examples in Edinburgh and its neighborhood; and I have seen some, and heard of many more, living in different parts of Scotland and England. It is interesting, however, not so much from the frequency with which it is met with, as from the nature of the anormal structure itself, consisting, as I believe it does, of a tendency in the human subject to the reproduction of a lost extremity.

As a general law, the power of repairing and reproducing lost parts decreases as we ascend from the lower to the higher parts of the animal scale. In the lowest and simplest forms of animal life, as in polyps, not only are separated parts or segments rapidly restored but the separated segments themselves sometimes become developed into whole and perfect individuals. A hydra was cut at different times into various portions by Trembley, and fifty separate individuals of the species were developed from the segments of one. Johnstone and Duges have shown that animals with much higher organization—namely, the Planariæ—could in the same way be multiplied by artificial subdivision; and Lyonnet and Bonnet found the same true of the Nais. As we ascend upwards in the scale of life, all power of self-development in separated parts or segments disappears, but the power of regenerating these lost parts or segments is retained to a greater or less degree by the general body of the animal. When the arms or rays of a Star-fish are broken off artificially, or when they are thrown off, as they sometimes are, in the Lingthorn or Luidia, &c., by a true "spontaneous amputation" on the part of the animal, the lost arms are betimes entirely restored. In the Crustacea a separated or amputated limb is also rapidly renovated. The head or anterior rings of the earth-worm and other Annelida are generally regenerated after their decapitation; and the power of reproduction is still so great in the Mollusca, that the snail, according to Schweigger, has sometimes its head and antennæ restored after they are removed by amputation, provided the cephalic ganglion lying above the œsophagus be left uninjured. In the lower divisions of the Vertebrata we have the salamander still capable of reproducing an entire leg or tail, or even of forming a new under jaw; and the triton can regenerate, as in Blumenbach's experiment, a complete and perfect eye. But in the higher and warm-blooded vertebrata this power of repairing and restoring lost compound parts and organs seems totally or almost entirely wanting. In man not only are complex individual parts, however small, generally held incapable of restoration, but portions of the higher individual tissues even, as mucous membrane, muscle, &c., when cut, removed, or destroyed, are not usually regenerated in their entire organization. To this general law, however, there are the following exceptions in the human subject.

1st. When the part removed is primitively of a lower type of organization than that of the general body, restoration sometimes occurs. Thus, in a case of a child born with an additional thumb, or with a thumb double from the first joint, the outer or smaller one was amputated by Mr. White of Manchester. It grew again, and along with it the nail. Subsequently, Mr. Bromfield of London a second time carefully removed this superadded portion of thumb, and turned the ball of it fairly out of the socket. "Notwithstanding this," adds Mr. White, "it grew again, and a fresh nail was formed."[1]

2d. In those animals that possess in the most marked degree, the power of readily regenerating lost compound parts, this power resides specially in the extreme points of the body, as the tail and limbs. In the human subject we sometimes find instances of an appearance of permanence of the same power in the extreme parts, as the fingers and toes. I have seen a distinct but imperfect nail grow on the end of the second phalanx of the finger, after the complete amputation of the first phalanx. Similar instances of nails, and consequently of the matrices of these nails, becoming regenerated on the tips of fingers amputated through their first joint, have been recorded by Corvisart, Ansiaux, Blumenbach, and others.

3dly. When, in the human subject, the removal of a compound part—such as a portion of an extremity—is effected in early fœtal life, and consequently at a time when the physiological powers of the young human being are more assimilated to the reparative and other powers of animals of a lower type in the animal scale—the lost part seems capable of at least a partial and rudimentary restoration. In the animal kingdom generally, we find the power of regeneration greater in the inverse ratio of the degree of development or age of the individual. The more perfect hexapod insects never reproduce a lost limb; but in the larvæ of these same insects, limbs and antennæ are restored after their removal. The experiments of Heineken show that the Arachnida, in the same way, lose the property of regenerating their legs after they have ceased to change their skin, and have reached their full or adult development. It is only in the young frog that reproduction of a limb occurs; and Spallanzani found that the rapidity with which the tail of the tadpole and the limbs of the salamander are regenerated, was always in an inverse ratio to the age of the animal. So, while in the human subject after birth we never see any trace of the reproduction of a limb after amputation, we have the contrary, as I believe, evidence of the possibility of their rudimentary regeneration in the appearances sometimes seen on the ends of stumps resulting from spontaneous amputation in early fœtal or embryonic life.

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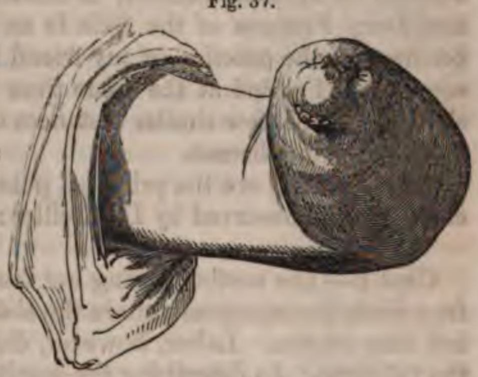
[1] Regeneration of Animal and Vegetable Substances, p. 16.

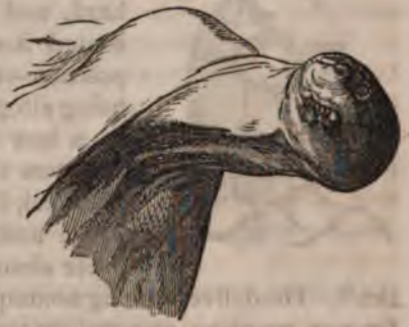
In most of the cases in which I have observed this appearance of a rudimentary reparation of an extremity, the spontaneous amputation had occurred in the upper half of the forearm; and the general resemblance of these cases to each other is very remarkable. Usually the rounded end of the limb has exactly the appearance of a stump after amputation, and is well covered with soft parts. Two points of the skin, or rather of the subcutaneous tissue, are found adherent to the ends of the ulna and radius, and present a depressed or umbilicated form, particularly when the forearm is flexed and moved, and the fissures of the skin run in converging lines to these two points as centres. Midway and a little in front of these two points, the rudiment of the regenerated extremity is situated in the form of a raised cutaneous fold or fleshy mass or tubercle, and having on its surface one, two, or more, smaller projections, or nodules, furnished with minute nails. In the instance given for illustration in the accompanying woodcut (Fig. 37) from a young woman of eighteen years of age, four such imperfect fingers are seen, two of them tipped with nails. In this, as in most other cases, the left arm is the seat of the mutilation, but I have seen the right similarly affected.

Fig. 37.



The woodcut (Fig. 38) represents the stump of the left forearm of a fœtus of the seventh month, preserved in the Obstetric Museum of the University of Edinburgh. There are five small rudimentary fingers tipped with minute nails in the usual position on the end of the stump. But the case is principally remarkable for the circumstance, that the cicatrization over the ends of the ulna and radius is not complete. There is an aperture at the end of

Fig. 38.



the radius through which the end of the bone can be felt when the point of a pin is passed through it. The ulna projects to the cutaneous surface of the stump, and has a small wound or circle of uncovered granulations still around it; or, in other words, the cicatrix of the stump is as yet incomplete, exactly as in Schaeffer's case already figured, p. 328.

INTRA-UTERINE CUTANEOUS DISEASE.

ICHTHYOSIS.

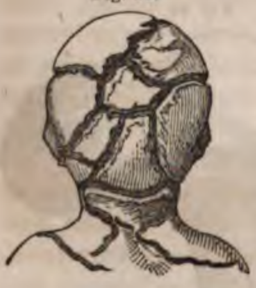
(From London and Edinburgh Monthly Journal of Medical Science, Aug. 1843, p. 697.)

In 1843 I had the pleasure of communicating to the Monthly Journal of Medical Science, a remarkable "Case of Thickening and Deep Fissures of the Skin in an Infant at birth," which had occurred in the practice of my friend Dr. Keiller. At Dr. Keiller's suggestion I added at the same time to his communication, some short notes of a few similar instances of this curious and rare form of intra-uterine disease.

The following are the principal points regarding the first of these cases, or that observed by Dr. Keiller:

Case I.—The mother during her fourth pregnancy suffered much from acute paroxysms of pain in the abdomen, and particularly in the left iliac region. Labor, however, did not appear to come on till the full time. In describing the results I shall borrow Dr. Keiller's own words: "On rupturing," he observes, "the protruding membranes, and bringing my fingers into immediate contact with the presenting structure, in order to ascertain its relative position, I was not a little astonished to find that instead of the smooth, soft, yielding scalp, a rough, hard, and irregularly fissured surface presented itself (Fig. 39), the usual guides to position—the fontanelles, sutures, and ears—being altogether indistinguishable. Although at a loss to determine what the advancing structure consisted of, its unyielding serrated-like feel, led me to anticipate the existence of an anencephalous development or some other abnormal condition of the bones of the skull. On delivery being accomplished, however, a sufficient excuse for any previous uncertainty immediately presented itself in the

Fig. 39.



hard, preternaturally thickened and deeply rent dermoid covering, not only of the head, but of the whole body, which seemed as if completely enveloped in an irregular coating of cartilaginous structure. So deep were the numerous fissures in the hard and much-thickened scalp, that in appearance, as well as to the touch, they very much resembled the sutures, or rather the serrated edges of the bones of a disarticulated adult skull.

"The skin of the face, in like manner, consisted of ossified-looking portions, also irregularly fissured; the nose and ears were represented by mere tubercular knobs without apertures; the eyelids were altogether undeveloped; red fleshy-like cushions occupied the *eyeless orbits;* the mouth seemed large and gaping; the tongue was voluminous and protruding, and their ill-starred owner, as if to render his hideousness still more complete, was yelling most vociferously.

"The skin of the trunk and extremities was equally hard and cartilaginous, though not so thick as that of the head and face; the rents, or cracks, were so extensive on the abdomen, that it appeared, in several parts, altogether void of dermoid covering.

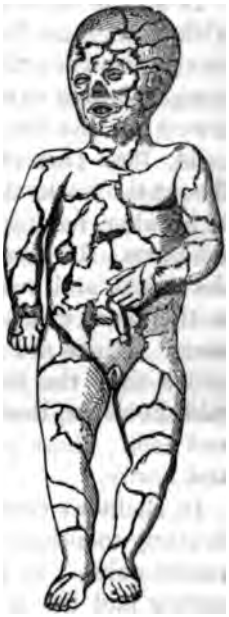
"The carpus and tarsus of the right extremities were apparently somewhat diseased.

"The genitals, like the nose and ears, seemed a mere tubercular knob.

"The child was otherwise well formed, and, judging from its size and apparent strength, more especially of its vocal organs, appeared perfectly '*viable.*'

"It only, however, survived twelve hours."

Fig. 40.



CASE II.—I saw three years ago, a fœtus presenting the same disease, in the Museum of Guy's Hospital, London. The only note of it to which I can refer, is a pencil jotting made at the time, to the following effect: "Fœtus born with skin-bound disease, and integuments fissured in all directions."

CASE III.—In 1792, Richter gave a very imperfect description of an instance of the disease, in an appendix to an essay, entitled, *Dissertatio de Infanticidio in Artis Obstetriciæ exercitio non semper evitabili.*

CASE IV.—Another case, much more fully detailed, was published in 1802 by Hinze in his *Kleinere Schriften.*

CASE V.—An infant, showing a well-marked instance of this form of disease, is preserved in the Berlin Museum. It was described in 1826 by Steinhausen in a thesis, entitled, *Dissertatio de Singular Epidermidis Deformitate.*

Instead of giving a separate account of each of these cases individually, it may be more instructive, and will avoid repetition, if we merely state some of the principal peculiarities connected with their pathological history.

In all the cases the skin showed the same diseased appearance, with numerous fissures extending over it in every direction, and leaving between them irregular-shaped compartments of thickened integumentary substance. In Steinhausen's case, the fissures were fewest on the back and extremities, and more numerous on the head, fore part of the trunk, and about the pudenda and anus. About the neck, the intervening compartments or scales were few in number, having apparently been rubbed off by the motions of the parts. In Hinze's case the fissures were longest and deepest at the wrists, and over the malleoli, where they extended down even to the bone, which was seen lying bare on raising the edges of the rents. Hinze did not see the child till two days after its birth, at which time the fissures extended over the whole body. He was told, however, that at birth they existed only over the face, head, and neck; those on the head had, when he saw it, become dry and horny.

In Richter's case, the remarkable hardness of the skin is not particularly mentioned; it is simply stated to have been of a reddish-yellow color. In Hinze's and Steinhausen's cases it is described as having had the same horny, cartilaginous feel, as in Dr. Keiller's patient. Steinhausen states that the whole body appeared as i covered with scales, overlapping each other, and varying in size in different situations. These scales, he adds, were composed of two layers, which could be easily separated from each other. The external layer was the hardest and thickest, and appeared in some part to be divisible into several lamellæ. On removing this layer, the exposed surface showed numerous depressions and elevations, and was beset with innumerable projections like small hairs. Beneath both the two layers, the skin is stated to have shown no abnormal appearance. In Hinze's case, on some parts, the segments of the skin had a smooth surface; in other situations the surface was rough, and here and there had a tuberculated appearance.

In Richter's case, as in Dr. Keiller's, the eyes are said to have been wanting, and in their place fleshy masses, very tender to the touch, were seen projecting from the orbits. In Steinhausen's, the yelids presented the same appearance as the rest of the skin; the yes themselves were probably sound, as the contrary is not stated. n Hinze's case there was ectropion of the upper eyelids, so that the yes, which were healthy, appeared as if covered with a bloody kin. This would probably have presented precisely the same appearance as is described in Richter's case, and naturally leads to he question whether the true state of the organs may not have been overlooked by him, especially as in all other situations the disease seems to have been confined almost entirely to the cutaneous texture.

In all, the ears were almost or altogether wanting; in Richter's case, there were small foramina in their place, and in Steinhausen's, merely small tubercles without any opening. The nose in Richter's case was also absent; the opening of the nostrils was present. In Steinhausen's, the nose was very small, and the lips were much disfigured, though internally they had their natural appearance. Richter mentions that the tongue in his case was unusually large and cleft, and that the anus was wanting. In Steinhausen's this last mentioned opening was very small.

The extremities in Hinze's case were much deformed and swollen, and the fingers and toes, with the fore part of the foot, are described to have been so disfigured as to resemble bird's claws. Otherwise, the child appears to have been well formed. In the case described by Steinhausen the hands were much swollen, and each had five fingers; the same was the case with the feet. In Richter's case both hands and feet were malformed; the extremities terminating in rounded knobs. Those of the upper extremities had warty projections instead of fingers. The head was also malformed, and, like all the others, was very scantily provided with hairs.

In Richter's case the infant lived for three days, though it could take no nourishment without being threatened with suffocation. The child described by Hinze lived four days. During the first twenty-four hours it was remarkably lively, and took eagerly the milk and water on which it was fed, the mother not being allowed to see it. The meconium was of the natural color. The urine is said to have been clear and inodorous. In Steinhausen's case the child lived three days and a half. In one case, Richter's, it was born about the eighth month; in the others not till the full period. In one of these cases, Hinze's, the mother, who was 34 years of age, had enjoyed excellent health during her pregnancy, and had previously given birth to three healthy children. In another, Steinhausen's, both parents are said to have been healthy, as in Dr. Keiller's case.

NATURE OF THE DISEASE.

The pathological nature of the disease before us is, under our present deficiencies of accurate anatomical details, a difficult and unsatisfactory problem. One thing, however, seems manifest, viz., that the cutaneous fissures are in all probability merely a secondary and mechanical result. The integumentary parts, having by previous disease lost their expansibility, and no longer increasing with the increasing dimensions of the growing body of the fœtus, at last split and fissure under the stretching from within to which they are subjected. It is a much more difficult matter to determine the character of the morbid action which has produced the physical inexpansibility of the integumentary coverings of the body. Hinze considered the disease to be of the nature of the lepra of the Greeks, or elephantiasis leprodes, and supposed that it depended on a syphilitic taint on the part of the parents, though of this he seems to have had no proof whatever, and the healthy state of the parents and of their children directly contradicts any such idea. It would appear to be much more analogous to Ichthyosis than to any other skin disease that can be referred to. We have no evidence of the disease affecting, primarily at least, more than the epidermic layer, or rather the surface of the true skin which secretes it. Steinhausen expressly states, that in the case he has described, the skin under the two layers of thickened epidermis appeared to be quite healthy. Hinze's case is particularly instructive, as showing the disease in its progress, before it had reached the complete form in which it is seen in the others. The whole body appears to have first become incrusted with the inelastic morbid epidermic covering. As the growth of the fœtus proceeded, this covering was split up in various directions, and the process of fissuring was probably hastened after birth, by the exposure of the surface to the air, causing it to harden and to contract. The fissures extending to the surface of the true skin, reparative or ulcerative inflammation necessarily followed, and from the great extent of surface affected, the irritation and exhaustion thus induced, speedily proved fatal to the infant. Had it been able to survive this irritation and exhaustion for some time, till the other fissures, like those on the head, were become dry and horny, it would have afforded a condition presenting a perfect specimen of ichthyosis in its most extreme form, and affecting the whole surface of the body.

If the disease be thus considered as a species of intra-uterine ichthyosis, we may further find some explanation of the differences presented by it, and those presented by the various forms of ichthyosis hat sometimes supervene in early infancy, or in more advanced life, y recollecting the very opposite conditions to which the surface of he diseased skin is subjected before and after birth. During extra terine life, the surface of the diseased and hypertrophied epidermis s generally kept dry, and densely fissured, by constant exposure to he air, and by the flexions and movements of every part on which t may be situated, whilst by continued friction and abrasion it is in most situations prevented, except in extreme cases, from acquiring any very great thickness. During intra-uterine life, on the other hand, the thickening crust of epidermis is, from the absence of these last causes, allowed to accumulate, and the exposure of the diseased surface to the liquor amnii, instead of to the atmospheric air, may account for the other points of distinction between this new species of ichthyosis (*Ichthyosis Intra-uterina*) and those previously known to nosologists.

As our knowledge of the pathological anatomy and exact pathological nature of this curious disease is still so imperfect, it is greatly to be desired that some of the excellent and zealous pathologists connected with Guy's Hospital would favor the profession with the result of a minute examination of the case contained in the splendid museum attached to that institution.

I may add, that if the affection prove to be connected in any degree, as suggested in my original note of the Guy's museum case, with the "skin-bound disease" (an opinion which, as will be seen from the preceding context, I am not now, on a fuller examination of the recorded cases, inclined to hold), there will probably be found, in addition to anasarcous effusion, appearances showing the existence of Bright's disease of the kidney.[1] Physicians are well aware, that the most vague and contradictory opinions have been promulgated in regard to the true pathological nature of the skin-bound disease. In the Edinburgh Lying in Hospital, I have seen only two cases of this affection, and the examination of these before and after death, with the coexistence in them of coagulability of the urine, and of slight yellow striated deposits in the structure of the kidney, seemed strongly to support, if not to prove, the idea that this affection was merely a form of anasarca connected with the occurrence of Bright's disease in the earliest period of infancy—an opinion which had at once occurred to the acute mind of my colleague, Dr. Ziegler, on his examining, during the lifetime of the little patient, he first of the two cases to which I refer. If this opinion be corroborated by additional observations, it will be valuable in two espects:

First. As so far resolving a pathological problem in regard to the nature of skin-bound disease, that has hitherto, in a great measure, set all explanation at defiance;[1] and—

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[1] See vol. i. p. 732.

Secondly. As showing that albuminous nephirtis—a form of malady which is so frequent and important in many respects in the pathology of adult life—is an affection that may occur, and under some conditions very frequently does occur,[2] at the earliest period of infantile existence, and even during intra-uterine life.

CASE VI.—Since writing the above remarks, I find that a long and detailed account of a case by M. Souty, has been published in the *Bulletin de L'Académie de Médecine* for October 30, 1842. In all its essential features, this case corresponds closely with the others, and it confirms in a great measure the view I have taken of its pathology. It was the third child of healthy parents, who had previously had two healthy children. About the fourth month of pregnancy, the mother had met with a severe fright, after which she was troubled with great disgust for food, intense pain in the lower part of the belly, with constant general uneasiness, and a frequent feeling of suffocation. Six weeks before her confinement, she had frequent copious discharges from the vagina of a thick yellowish matter with a fœtid smell.

The whole surface of the infant at birth was traversed by reddish bands, of different shades, from a violet tint to purple, and varying in length and breadth. These enclosed irregular scaly patches of skin, which at birth were white, but soon became yellow. The epidermis on these patches was somewhat rough, its appearance resembling that of morocco leather. In some parts, it was more than a millimetre thick, and approached in hardness to the skin of the heel of the adult.

The surface of the fissures was covered with a thin pellicle like serous membrane, or like the cicatrix of a recent burn or blister. In some situations, as in the neck and inner surface of the articulations, this cuticle had, from the movements of the infant, given way, exposing a raw surface, that showed very distinctly the texture of

[1] In illustration of the variety of opinion that has been held in regard to the pathological nature of the skin-bound disease of infants, we may mention that Andry and Auvity thought that this affection was brought on by the action of cold on the surface of the body, stopping the transpiration, and leading to condensation of the fluids in the cellular tissue; Dennis and others supposed the cellular tissue to be in a state of inflammation; Baron, that it was simply œdematous, from some obstruction to the circulation; while Underwood and Stutz speak of its being in a state of tonic contraction or spasm. Others have imagined it to depend on a disordered condition of some internal organ. Thus Breschet, finding in some cases he examined, that the foramen ovale was still open, attributed the disease to this circumstance, as one of its causes. From similar evidence, Hulme ascribed it to inflammation of the lungs; Paletta, to congestion of the liver; M. Leger to unusual shortness of the intestinal canal, &c.

[2] Of 644 infants received into the Infirmary of the Hospice des Enfans Trouvés at Paris during the year 1834, 289, or about 45 per cent. of the whole, were affected with this disease

the true skin, which appeared to have preserved its natural structure. Over the thorax and abdomen, the red bands were arranged symmetrically; a broad band ran along the middle line in the course of the linea alba, from which transverse lines passed off on each side. On the limbs, they were also somewhat symmetrical, and they were broadest in the neighborhood of the joints, where a considerable space intervened between the different segments of epidermis. They were far most numerous on the anterior surface of the body and on the limbs.

A few brown hairs were present on the head. The nose scarcely projected from the face, the cartilages being apparently bound down by the thick and almost horny integument which covered them. The auricles were also in a rudimentary state, and the foramina obscured and narrowed by the thickened epidermis. The eyes had the appearance of red fleshy projections. This arose, however, from the upper eyelids being completely everted, so that the palpebral conjunctiva projected between the two tarsal cartilages, which had been stopped in their growth by the thick and horny epidermis; on raising the red tumor, the ball of the eye itself was seen to be healthy. The mouth was kept widely open by the contraction of the epidermis. The external genital organs had also the appearance of having been arrested in their development.

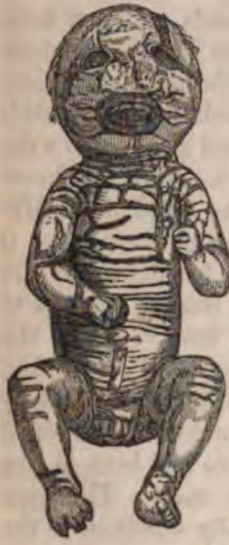
The extremities were well formed, but the hands and feet were much disfigured by a collection of matter under the skin, which was so much thinned and distended that the whole had the appearance of a scrofulous tumor. This matter, which was a reddish sanies, extended up to the points of the fingers, and gave them a deformed curved appearance, so as to resemble claws. Nothing abnormal was found in the interior of the thorax. The child lived fifty hours; it cried feebly, but deglutition and respiration were easy; it passed some meconium, but no urine. M. Souty has deposited it in the Musée Dupuytren at Paris. In this case the disease appears to have been originally in a somewhat milder form than in any of the others, and also to have advanced farther in the process of reparation. This is seen from the degree of cicatrization of the fissures, and by the greater distance between the different segments of thickened epidermis. In all probability, however, the surface had been at one time in much the same condition as in the other cases. This case also clearly shows that the disease was originally confined to the epidermis, and that the deeper parts, as the cartilages of the ears and nose, the external genital organs, &c., were contracted and disfigured simply from being confined by the thick and unexpansible epidermic covering. The degree of development of these parts thus gives us pretty accurate information as to the period of intra-uterine life at which the disease supervened, or at least the period at which it acquired such a degree of intensity, that the skin had become so thickened as to be able to repress these subjacent parts in their growth.

Additional Cases of Ichthyosis Intra-Uterina.[1]—Two additional instances of this curious and rare disease have been communicated to me since the publication of the previous notice.

Case VII.—Professor Vrolik of Amsterdam informs me, that a preparation of the same fœtal disease exists in the museum attached to the medical school of that city. An account of the case was drawn up and published by his distinguished father, in a Dutch Medical Journal in 1841. Through the great kindness of Professor Vrolik, I have been furnished with drawings and manuscript notes of the case; and I venture to publish them, not only as an addition to, and confirmation of, the observations which I have offered on the subject, in the preceding pages, but as containing also some new and valuable remarks.

The child described by Dr. Vrolik was a male, and born in the eighth month. It was the offspring of parents who were in the enjoyment of perfect health.

Fig. 41.



"The external appearance of the infant was so peculiar," observes Dr. Vrolik,[2] "that it would be very difficult to form a correct idea of it without the drawings. Its skin (Fig. 41) fissured in different parts, was hard and thickened generally; it was of a yellow color, and covered with the epidermis; in the fissures it was softer, and somewhat of a bloody appearance.

"The head, which was almost round, presented very open sutures, and a very large anterior fontanelle. It was covered with thin, short, silky hair, and the scalp was intersected by numerous fissures, one of them running from the left superior eyelid over the os frontis, being twelve millimetres in breadth. Another was confined to the right superior eyelid, and here also there was a very large fissure at the external angle of the eyelids, which ran in a direction backwards.

"The forehead was inclined backwards; the ears were quite concealed by the skin, and only to be seen in their outline very imperfectly; the eyebrows were entirely absent; instead of closed eyelids, there were two bloody tumors formed by the interior lining membrane of the eyelids, or by the ectropia of the conjunctiva, behind which the eyeballs were deeply concealed in the orbits; there were but few cilia on the eyelids, and the extroversion of the conjunctiva was less in the under than in the upper eyelids.

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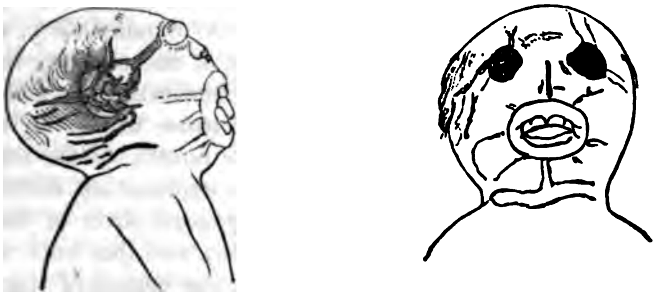
[1] From Edin. Monthly Journal of Med. Science, July, 1844, p. 545.

[2] The case has since been published by Vrolik, in his Tabulæ ad illustrandum Embryogenesin Hominis, &c., Amsterdam, 1849, Tab. xcii.—(Ed.)

Fig. 42.



"Between the two very distant eyes there was some indication of a nose, which was very large and flattened (Fig. 42), and indeed only shown to be such by the two nostrils.

"The mouth was largely opened, forming an ellipsis with the returned interior surface of the lips, and surrounding the alveolar parts of the two jaws and the tongue, which was much thickened; in the upper jaw the rudiments of the incisor teeth were very apparent; at each angle of the lips there were three cutaneous fissures; and in the same manner there was one on each side of the upper lip.

"The neck was short and broad, and consequently the head was close to the thorax; the breast and abdomen were almost of the same size, and very different in their appearance from those of well-formed subjects of the same age; the upper and lower extremities were so far natural, but the hands and feet were swollen, and covered with a fissured skin, and the fingers and toes were so disfigured that they resembled claws; the natural thickening of the epidermis on the sole of the foot was confined to a very small part, and distinctly circumscribed.

"The scrotum, in which there were no testes, and the penis, were of a red color, and appeared as if the corium and epidermis were retracted from them; no perineum was to be seen; the stretched skin did not allow of a depression behind the scrotum; the opening of the anus was placed on the same level with the nates; here, as well as on the thighs, there were a great many fissures, which, by some difference in their appearance, showed that they had not all been formed at the same time.

"By a superficial examination of all these fissures, it was very clear that some of them were recent and unhealed; others were in the way of being cured; some were covered by a red cicatrix; and others were quite like the unfissured parts of the skin."

After the above description, "Dr. Vrolik enters," as his son writes me—and I quote his own words, "into some considerations regarding the etiology of this deformity, and arrives at a conclusion similar to that of Dr. Simpson, viz., that the cutaneous fissures are merely a secondary and mechanical result, and that the integumentary parts, not possessing a proper degree of expansibility, and not increasing with the increasing dimensions of the growing fœtus, at last split and fissure under the stretching from within to which they are subjected. To the same mechanical cause are to be referred the ectropia of the eyelids, the form of the mouth, of the nose, and of the ears. The only part in which his explanation differs from that of Professor Simpson is, that the general form of the child must depend upon an arrest of development, and that both this and the degeneration of the integuments are the causes of this deformity."

Case VIII.—After the former cases were published, it occurred to me that my friend Dr. Lewins once casually informed me, that he had seen a child born at Haddington, with a diseased state of the skin greatly resembling, if not identical with, the other instances I had collected. On lately applying to Dr. Lewins on the subject, and transmitting to him at Tiverton, drawings of Dr. Keiller's case, he immediately furnished me with the following reply:

"In the drawing that you had the goodness to send to me, I distinctly recognize an exact similitude to the case I saw at Haddington sixteen years ago, except that in the instance which I witnessed, the disease was confined to the head and neck. The fissures in the Haddington child were deeper, perhaps I should say wider, than those in Dr. Keiller's case, if I can judge from the drawing. The back view seems to me more like that which came under my observation. In consequence of the fissures in the face and forehead being wider, and the interstices broader, and the child larger—gigantic, I may say—its aspect was more horrible than the Dundee case seems to have been. Indeed, the object of the parents in sending for me was to sanction the destruction of the monstrous production; and I had difficulty in convincing them that the *Senatus Academicus* of the University of Edinburgh had not invested me with such despotic power when they made me a Doctor of Medicine.

"The child had been born at the full time. I saw it about 48 hours after the woman was delivered. The infant appeared viable enough. It had not been applied to the mother's breast, but swallowed milk and water greedily, and cried lustily. It was dead and buried at my second visit, a few days afterwards.

"The labor had been dreadfully severe, from the great size of the child's head.

"I have no recollection of anything particular in regard to the eyes; but the eyelids were fissured and swollen, as was the nose, particularly the alæ, and the lips. Altogether, the face was most horrible to behold.

"The parents were healthy persons, with rather fine skins, and both well formed. They had a large family of fine children."

INTRA-UTERINE SMALL-POX.[1]

(From Edinburgh Monthly Journal of Medical Science, April, 1849, p. 694.)

During the last month I had an opportunity of seeing two cases of variola in the fœtus. Both children were born dead.

Case I.—The first case occurred in the practice of Dr. Purdie. The child was near the full time. The variolous eruption was scattered freely over the whole cutaneous surface in the form of pustules. Several of the pustules were umbilicated. The mother at the time of the birth of the child was laboring under an advanced stage of an attack of modified small-pox.

Case II.—In the second case, occurring in the practice of Dr. Gordon, the fœtus was expelled about the fifth month. The pustules were principally confined to the trunk of the fœtus, and consisted chiefly of flattened collections of matter. The mother was recovering from modified small-pox.

Intra-uterine small-pox has now been observed by Mead, Jenner, Hosack, and other pathologists, under the following circumstances, viz.:

I. In some cases the fœtus was affected with natural small-pox, while the mother was suffering under the modified form of the disease, as in the two above cases.

II. Both mother and fœtus sometimes suffered contemporaneously under the natural small-pox. In a case which occurred to Dr. Paterson of Leith, the disease proved fatal to the mother; and on a post-mortem inspection, the child in utero was found covered with them.

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[1] Extracted from proceedings of Edinburgh Medico-Chir. Society, Feb. 21, 1849.

III. Occasionally the child when thus affected, passes safely while in utero through a full course of small-pox, and is at length born with the pits of the disease alone remaining. I vaccinated repeatedly a person born under these circumstances, and always unsuccessfully.

IV. In some instances in which the mother has been exposed to the contagion of small-pox, but herself escapes an attack of the disease, in consequence of previous variolation or vaccination, the fœtus is nevertheless born affected with it. In these last instances the fœtus in utero being attacked by a disease, to the contagion of which the mother was exposed and yet herself escaped, it is evident,

1st. That we have in this a proof that a morbid contagious matter may be inhaled and introduced into the system without the disease following in the system of that individual, the predisposition to it being destroyed by previous disease.

2d. That this morbid matter may nevertheless pass into and produce the specific disease in the fœtus in whom the predisposition to it is present.

3d. That the morbid matter can only thus pass from the mother to the fœtus through the medium of the circulation, for there is no communication by the nervous system between the economy of the mother and that of the fœtus.

And 4th. That this affords sure presumptive proof that variolous, and perhaps other contagious febrile matters, affect the body by first entering the vascular system.

Other contagious diseases have affected the fœtus in utero, as Dothinenterite, according to Roederer and Wageler; Plague, according to Russell; and numerous instances of congenital Measles and Scarlatina have been recorded. Morton, Russell, Paulini, &c., have described instances of malarious poison thus also reaching the fœtus and producing intra-uterine ague.

ON INTRA-UTERINE GOITRE, OR BRONCHOCELE.

Various kinds of tumor in the cervical region of the fœtus have been found at the time of birth.

1. The cervical portion of the vertebral column is sometimes, though not so often as the loins or back, the seat of spina bifida; and the resulting tumor has been seen to vary in size from the bulk of a nut to that of the infant's head.

2. Meckel, Otto, and other pathologists, have described a variety of congenital cystic tumor of the cellular tissue, situated on the posterior part of the neck, and remarkable for a central pillared division, into two lateral and symmetrical lobes, by the ligamentum nuchæ. I have seen it mistaken for a spina bifida.

3. Numerous instances of intra-uterine tumors in the anterior and lateral portions of the cervical region have been recorded of late years by Berndt, Cæsar Hawkins, Beatty, Mütter, and others, under the name of cysts, or "congenital hydrocele" of the neck. These tumors, which sometimes grow much after birth, usually consist of one, two, or more large serous cysts, capable of being emptied and obliterated by puncture and the injection of iodine; by setons, &c. Sometimes an agglomeration of small cysts enters also into their composition.

4. A kind of large cystic tumor, the mass of which consists of a numerous collection of small cells filled with thickish glairy fluid, is occasionally observed at birth in the upper part of the neck, and projects more or less into the mouth. It seems to be a true Ranula, originating in and consisting of hypertrophy and enlargement of the follicles of the salivary glands. I have seen two instances of it where the children died a few days after birth, the puncturing of one or two cells being of no use in diminishing the mass. There is a specimen of it in the obstetric museum of the University, forming a large tumor at birth. This form of apparent cystiform tumor has, I believe, been frequently confounded with the third variety alluded to above.

5. Among the tumors on the neck at birth, I have seen one remarkable instance of a large flattish swelling on the posterior cervical region, covered with skin of the usual color and appearance, and formed of a deep-seated erectile vascular tissue, which in a great measure disappeared under pressure, and enlarged when the child cried or strained. I treated it by various means, none of which produced complete obliteration. Some years afterwards I heard that it was cut into by a surgeon, and that the resulting hemorrhage was most excessive.

6. Few cases of congenital enlargement of the thyroid gland, or of true intra-uterine goitre or bronchocele, have been hitherto placed upon record. The following cases, however, will show that goitre constitutes one form of cervical tumor, which may be occasionally met with at birth. Bronchocele is sometimes hereditary, but very few instances of it have been seen at birth in infants thus

predisposed. Usually there is no trace of it till some years subsequently. The following is the only exception to this general remark which I have been able to find:

Case I.—In an essay on goitre, published in 1824, M. Ferrus refers to a congenital instance of the disease, which had occurred in the practice of M. Godelle, physician to the Hospital of Soissons, and where the mother was affected with the same disease.[1] The child only survived for a few hours after birth.[2]

Lately I met with a marked instance of intra-uterine goitre in my own practice, and had an opportunity of ascertaining its true nature by dissection.

Case II.—The mother of the child never suffered from any symptoms of goitre, or lived in any place where the disease was endemic. She has now borne ten children, The first seven of these infants were still-born. They all died, I believe from the reports given to me, of disease of the placenta, and not from any malady or malformation in their own bodies. During her last four pregnancies she has been under my professional care, and has taken always in the latter periods of utero-gestation, large and continuous doses of chlorate of potass. The four last children were born alive, and have continued to live, with the exception of the last, namely, the one born with goitre. It survived only for about eight hours after birth, and would have died much earlier from asphyxia, if a catheter had not been retained in the trachea to obviate the compression of the mass of bronchocele. The child was born two or three weeks before the full term, labor having been induced in consequence of the child's heart beginning to beat with morbid slowness. The goitrous enlargement of the thyroid gland was nearly of the size of a hen's egg. It rendered the labor tedious, by preventing—as the hands or arms placed in the hollow of the neck sometimes do—the proper flexion of the head, and the approach of the chin to the sternum ; the presentation in consequence being one of the forehead, and not of the parietal bone. The goitre, or bronchocele, as seen after birth, appeared to fill up entirely the space or hollow between the chin and sternum. On examination after death, it was found to surround almost entirely and compress the trachea. All parts of the thyroid gland were equally affected. The goitrous tumor was comparatively smooth on its surface, but had a small irregular nodule attached anteriorly to its upper border, close to the body of the hyoid bone. In the accompanying woodcut (Fig. 43), *a* represents the hyoid bone, *b* the epiglottis, *c* the trachea; and *d d* the right and left sides of the goitre respectively.

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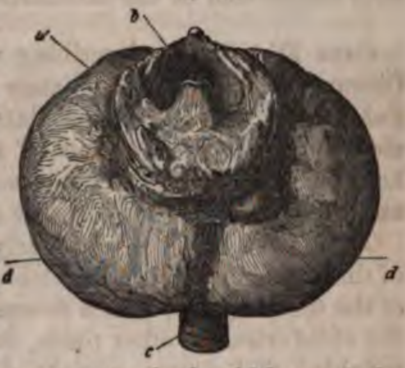
[1] Dictionnaire de Médecine, tom. x. p. 283.

[2] In the Archives Générales de Médecine, tom. xiii. p. 76, Dr. Cassan speaks of a remarkable case of hereditary goitre, where a young infant in the family died of it; but whether it was congenital or not in this child is not precisely stated. "A woman, aged 23 years, married, affected with pulmonary consumption in the second stage, presented to us an example of the obstinate hereditary predisposition of pulmonary phthisis and of goitre; her young infant (*jeune enfant*), her father, and seven brothers of her father, had died of the former disease; one of her paternal aunts, who showed no disposition to phthisis, carried a very large goitre; herself (the patient) was affected with goitre, which had considerably diminished since the first symptoms of phthisis had been developed. All her brothers and sisters had been victims to that cruel affection; only one sister, who had a goitre commencing, enjoyed good health at that period. One could say in that family, that the two affections were in such relation, that the one appeared reciprocally to supplant the other."

Fig. 43.



Internally, it presented a firm glanduloid structure; and under the microscope, it appeared to consist of the usual thyroid tissues, greatly hypertrophied. The vesicular cavities of the gland seemed not only increased in number, but enlarged in size also, and the septa between them were thickened. They were distended with epithelial contents. The external surface of the brain of the child was surrounded with a large quantity of serum, and the brain itself was considerably below the usual size. The opening of the eyelids was also small. The thymus gland, supra-renal capsules, &c., were normal in size and structure ; and there was no other unusual appearance detected.

In his essay on the pathological anatomy of new-born infants,[1] Dr. F. Weber describes an example of congenital goitre, similar in several respects to the preceding instances.

Case III.—A child was born some weeks before the ninth month, and it survived only a few minutes. The goitrous thyroid gland projected forward in the cervical region, was about half an inch thick, and extended not only laterally, but also backwards, some distance over the upper part of the trachea, though not to such a degree that a union of both lateral lobes had occurred posteriorly. On being cut into, the parenchyma of the bronchocele appeared dark red, and the microscope showed within it a quantity of effused blood-globules, which were not evident to the naked eye. In other respects, the parenchyma of the tumor presented internally the normal structure of the thyroid gland. The thymus gland appeared also larger than usual, and particularly on one side, but without any change of structure. There was a considerable degree of hydrocephalus present, with contraction of the extremities.

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[1] Beiträge zur Path. Anatomie der Neugebornen, p. 84.

CASE IV.—When describing the case No. II. to the Medico-Chirurgical Society, immediately after the time of its occurrence, in February, 1855,[1] Dr. Keiller stated that he had, a few months previously, met with an instance of the same disease, where the child's head at birth offered the same unusual presentation. I have lately examined the child, who is now about a year old, with Dr. Keiller.

There is still a large irregularly lobulated swelling in the region of the thyroid gland, and stretching somewhat upwards on each side of the trachea. It projects forwards, and appears to swell out when the child cries. At other times, the skin of the neck looks flaccid, wrinkled, and empty, over the site of the tumor in consequence of the tumor itself having diminished and shrunk considerably since the time of birth. The lobulated masses of the tumor feel firm and hard to touch; and probably the intervening and connecting tissue, in which the absorption has been specially marked, was originally more cystic in its character.

The tumor does not seem to affect in any way the general health and growth of the child. The mother was born and brought up in the county of Cumberland, where goitre is not uncommon; but neither she nor any of her relatives were ever in the least degree affected by it. The present goitrous infant is the first child which she has borne. Before pregnancy occurred, she was under my care for chronic metritis; but her general health is good.

An instance of congenital cervical tumor, under the title "Scrofula in Fœtu," was long ago described by Francus,[2] with characters and a site which have made Graetzer and Montgomery refer it to the head of goitre. In this, as in Dr. Keiller's case, the certainty of the tumor consisting of enlargement of the thyroid gland was not made out by dissection.

CASE V.—The child—a boy—presented at birth a tumor on both sides of the neck, but it was largest on the left. When the infant cried, or moved his neck too freely, that on the left side swelled excessively, and appeared to interfere with the power of suction and deglutition. Francus adds, that he unsuccessfully tried to effect the removal of the swelling by various remedies, local and general, and that, notwithstanding, it increased daily in size.

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[1] See report of Society's proceedings, Ed. Monthly Journal of Med. Science, April, 1855, p. 350.—(ED.)

[2] *Ephemer.* Nat. Cur. Dec. ii. An. v. Obs. 223.

CONGESTION AND INFLAMMATION OF THE PLACENTA.[1]

(From Edinburgh Medical and Surgical Journal, April, 1836, p. 265.)

The Diseases of the Placenta form a subject of inquiry which appears to have hitherto attracted, especially from British pathologists and accoucheurs, a very incompetent share of attention. In the works of various systematic medical and obstetrical authors of the last and of the preceding century, as in those of Bonet, Ruysch, Mauriceau, Morgagni, and Wrisberg, notices of some individual cases and forms of placental disease are occasionally to be met with —and more lately, several of the morbid affections of this organ have been investigated with greater or less attention, by different continental pathologists—as by Murat,[2] Desormeaux,[3] Brachet,[4] and Cruveilhier,[5] in France, and by Professors Stein,[6] D'Outrepont,[7] and Wilde,[8] in Germany. I am not aware, however, that the medical literature of England contains any account of the various morbid states to which the placenta is liable, besides what is to be found in the casual observations and in the details of isolated and incidental cases recorded in the obstetrical writings of Smellie, Denman, Hamilton, Burns, Ramsbotham, Ingleby, Montgomery, Granville, and others.[9] It is not, therefore, without hesitation, that I venture to offer the following general remarks upon some of the principal forms of placental disease, as derived, partly from the data furnished by others, and partly from the observations which I have myself had an opportunity of making upon a few recent, and a very considerable number of preserved specimens of morbid placentæ, contained in the different pathological museums of this country. At the same time, I may, I hope, be allowed to plead in excuse for the many imperfections of the following essay, the comparative novelty of the subject of which it treats, the paucity of materials still existing with regard to it, and the difficulty of collecting these materials from the numerous different sources over which they are scattered.

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[1] Read before the Royal Medical Society, November 20, 1835.

[2] Dictionnaire des Sciences Médicales, tom. xlii. 1820, p. 543.

[3] Dictionnaire de Médecine, tom. xv. 1826, p. 379.

[4] Journal Générale de Médecine, tom. cii. 1828, p. 10.

[5] Anatomie Pathologique, Livrais. i., vi., and xvi., 1829–34.

[6] Gemeinsame Deutsche Zeitschrift für Geburtskunde, Bd. v. Heft. 2, and Kleinert's Allgemeines Repertorium, v. Jahrgang. 1, Heft. s. 35, 36, 1830.

[7] Gemeins. Deutsche Zeitschrift für Geburt. Bd. v. Heft. 4, and Kleinert's Allgem. Repertor. iv. and v. Jahrg. Suppl. i. Abtheil. s. 77, 1830.

[8] Medicinische Zeitung, Jahr. ii. Abtheil. 11, Berlin, 1833.

[9] I ought perhaps to mention, as exceptions to this general remark, a brief analysis of the symptoms of different diseases of the placenta from Professor Wilde's essay, inserted in p. 573 of the 6th volume of Dr. Ryan's London Medical and Surgical Journal, as well as a short notice of the subject in Dr. Ryan's own lectures on the Physical Education and Diseases of Infants, published in the same Journal, vol. vi. pp. 140–142.

The Human Placenta is an organ which serves as a mere temporary medium of communication between the mother and fœtus, and passes through all its different stages of formation, growth, and ultimate expulsion, in the short space of seven or eight months, yet during this comparatively brief existence, it appears to be liable to a considerable number and diversity of morbid conditions, more frequent, I believe, in their occurrence, and more important in their effects, than seems to be generally suspected. Nor are the morbid conditions and lesions of this organ matters of pathological curiosity and interest only; a more accurate knowledge of their various phenomena appears to promise results of no inconsiderable consequence to practical medicine. The peculiarities of the placental economy of the human female, and the nature of the relations which exist between it and the uterus, constitute, particularly when these relations happen to be deranged by placental or uterine disease, one of the greatest sources of danger and fatality in human parturition. Numerous facts also seem fully to prove, that different morbid actions and lesions of the placenta exert a marked and powerful influence upon the health and well-being of the mother during pregnancy—and, above all, the study of these lesions promises to throw an almost entirely new light upon the abnormities of development, the disease and death, of the fœtus in utero. Most, indeed, of the latest inquiries instituted upon these points, appear to me to afford satisfactory evidence, that certain particular varieties of malformation of the fœtus are attributable, in some instances at least, to morbid conditions of the placenta, or of the membranes covering the internal surface of the placenta, as their immediate exciting cause;[1] that the not unfrequent occurrence of the death of the fœtus in the earlier as well as in the latter stages of pregnancy is almost lways the result of a diseased state or states of this organ, and that he act of abortion, occasionally the indirect cause of that death, ut much more commonly its indirect effect, is in very many intances capable of being traced to the same source. It does not eem, therefore, irrational to hope, that by a more careful investiation into the nature of the different diseased states to which the lacenta is liable, and by a more diligent and rigorous observation f the symptoms that precede and accompany these states, and of he causes by which they are produced, we may be gradually led to greater degree of facility and accuracy in their detection, and to he adoption of better established principles for their prevention nd treatment—and that in this way may be promoted in no inconiderable degree, those great and ultimate objects of all medical tudy,—the preservation of human life, and the alleviation of human uffering.

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[1] The varieties of malformation to which I here more especially allude, consist, as will appear in the sequel, of certain forms resulting from a morbid adhesion between the embryo and the membranes covering the internal surface of the placenta. It is not improbable, I conceive, that other kinds of malformation may yet be traced to a diseased state of the appendages of the ovum, as their first or more immediate exciting cause, but the present state of our knowledge upon this subject is certainly not by any means such as can entitle us to assent to the following comprehensive proposition, deduced by M. Serres as the result of his able investigations into the origin of monstrosities with a reference to the theory of the primitive duality of all the organs of the body, and laid down by him in his late interesting work, "Recherches d'Anatomie Transcendante," &c. "En dernière analyse, la cause la plus generale des irregularités des developpement et de la monstruosité reside en premier lieu dans la dualité primitive des placenta, et en second lieu dans la separation, ou la coalescence, de ces deux organes."—Loc. cit. p. 178.

The two most frequent and important diseased states to which he placenta is liable, are certainly sanguineous *Congestion* and *nflammation*, with their various consequences. The placentary arenchyma, and the membranes investing the organ, are liable, s I shall afterwards endeavor to point out, to other morbid states —to hypertrophy and atrophy, to softening and induration, to artilaginous and calcareous degeneration, and the secretion or foration of other morbid products and tissues, to an abnormal cystoid r hydatidiform structure, and to various forms of malformation nd displacement,—but, in a practical point of view, most of these esions are comparatively less important than congestion and nflammation, and the effects which these diseased conditions produce. It is to congestion and inflammation of the placenta, therefore, that I would wish first and principally to direct attention.

I.—Congestion of the Placenta.

By the term *Congestion of the Placenta,* I mean to express the existence of a greater than usual quantity of blood in the vessels of that organ, taking place rapidly or more slowly, in some cases rapidly or more slowly terminating in resolution, and in others leading to sanguineous infiltration or extravasation, in greater or less quantity or quantities, into the substance of the placenta, upon its uterine, or fœtal surfaces, or between the membranes of the ovum. The excess of blood in the vessels of the placenta, constituting congestion of that organ, may, there is every reason to believe, have its seat primitively and separately, either in the placental ramifications of the fœtal umbilical arteries and veins, or perhaps more frequently in the vascular prolongations, which seem to be extended from the uterine vessels of the mother into the substance of this viscus;[1] but a state of congestion in one of these systems of vessels, the fœtal or the maternal, scarcely perhaps ever continues for any considerable period of time, without giving rise to a similar state in the other.

Characters of Placental Congestion and Hemorrhage.—The anatonical characters of simple congestion of the placenta may, I believe, be sufficiently illustrated by a reference to the engorged and dark-olored condition which this organ presents, in cases in which the head of the child has happened to be long and dangerously impacted n the passages of the pelvis. The appearances which the placenta xhibits on its being expelled after such cases, and more particularly f the impaction has been so great as to prove fatal to the child, are well known to every practical accoucheur. The external surface of he organ is of a more or less deep violet, and sometimes almost ivid color, its internal structure when torn or divided by the scalpel resents a deep purple hue, its vessels are everywhere loaded and distended with dark-colored blood, the organ appears enlarged, and ts substance feels heavier and more solid than natural.

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[1] In this and in other parts of the present essay, the language that I have employed, and some of the pathological explanations that I have offered, will be found to accord with the belief, that the description of the utero-placental vessels and circulation originally given by the Hunters is fundamentally correct, and that not only small tortuous bloodvessels (arterial branches) pass across from the inner surface of the uterus into the placenta, but that venous branches also, or hollow canals at least, continuous with the venous sinuses of the uterus, run through the deciduous membrane covering the uterine surface of the placenta. From what I have myself observed in recent placentæ, and in a personal examination of Dr. William Hunter's preparations in the Museum at Glasgow, I am inclined to believe that Dr. Lee, and with him M. Velpeau and others, have deceived themselves in considering the placental decidua as an imperforated membrane, by not attending sufficiently to the circumstance, that the utero-placental veins pass through the deciduous covering of the placenta in a very oblique direction, and that the coats of these venous canals are extremely thin and slender, and composed of a tissue that is endowed with very little elasticity, and resembles in color and appearance the proper tissue of the decidua. Hence it happens, that when we look upon the outer or convex surface of the placenta after it has been separated from the uterus, the deciduous covering of the organ presents the appearance of a perfectly continuous membrane, having no visible foramina or hollow canals in it, owing to the inelastic parietes of its venous canals having then completely collapsed together, and to the torn orifices of these canals, being thus concealed by their sides, more or less fully overlapping one another in consequence of the oblique course of the vessels themselves. These sources of fallacy in ascertaining the proper vascular structure of the placental decidua are always rendered greater, if, instead of being kept of its natural convex form externally, the placenta be placed for our examination upon a flat surface, as by this means all the parts on the uterine side of the organ are necessarily more closely pressed together. Drs. Sharpey and Allen Thomson, in examining during last summer a gravid uterus taken from the body of a woman who had poisoned herself between the fourth and fifth month of pregnancy, were able to trace in the most satisfactory manner both utero-placental arteries and veins passing through the deciduous membrane of the placenta. They convinced themselves of the free communication of the venous canals which they found in the placental decidua, with the proper uterine veins or sinuses, by tracing a continuity of tube between these two sets of vessels, in carefully dissecting the venous canals or cavities of the decidua both from the external or maternal portion of the substance of the placenta towards the uterus, and from the sinuses of the uterus towards the placenta, as well as by blowing air through them in these different directions while the preparation was immersed in water. These gentlemen discovered also small tortuous spiral arteries, less in diameter than the utero-placental, but similar in form, passing from the inner surface of the uterus into the decidua vera covering those parts of the uterus to which the placenta was not attached. They further found a network of considerably sized veins ramifying through the substance of this uterine decidua. The anastomosis of these deciduous venous canals with one another, as well as with the cavities of the veins running on the edge at least of the placental decidua, and with the mouths of small venous orifices opening on the internal surface of the uterus, fully appeared on inflating them in different parts of their course. It is to be hoped that the results of this interesting observation, with all its necessary details, will soon be laid before the medical public. In addition, I may state, that Mr. Owen of London has been lately successful in verifying, by the careful dissection of several gravid uteri, the accuracy of the anatomical description of the utero-placental circulation left us by the Hunters.

In cases of abortion occurring as a consequence of physical injuries f the uterine region, or of any of those causes which give rise to it y producing a strong determination of blood to the vessels of the terus, the same congested and engorged state of the placenta, as hat to which I have just now alluded, is generally met with, and n these instances also the blood is not unfrequently found effused n more or fewer points, from rupture of the lacerated or distended lacental vessels, constituting the Apoplexy of the Placenta of M. Cruveilhier, who, with Laennec, Andral, and other Parisian patho-ogists, has employed of late years, this term to designate the exist-nce of an effusion of blood into the tissue of the lungs, and other rgans of the body, while the word literally and originally signifies ne of the more obvious external phenomena or symptoms only, which such an effusion of blood generally produces when it happens o occur within the encephalon.[1]

The blood effused in placental congestion and hemorrhage occur-ing during pregnancy may occupy different anatomical situations. n numerous instances it is found on the external surface of the placenta, or between it and the uterus, occasionally in very large quantity; in other instances the extravasated blood is situated owards the fœtal surface of the organ, or insinuated between the membranes, or, as very often happens, between the uterus and decidua; and in others again, it is seen effused into the substance or proper structure of the placenta, as remarked by Deneux[1] and Baudelocque,[2] and as I have myself had an opportunity of observing in a considerable number of specimens. In cases in which abortion occurs in the earlier periods of pregnancy, the extravasated blood is frequently found diffused over the whole or nearly the whole surface of the chorion, and between it and the amnion, in the form either of a continuous layer or layers of coagulum, or of more or less isolated nodules; and in two instances I have seen it occupying these situations, at the same time that it was both effused into the cavity of the amnios itself, and extravasated in the form of roundish masses among the rudimentary structures of the placenta. The immediate anatomical seat of sanguineous effusions situated in the substance of the placenta itself, is probably, in general, in cavities formed in a greater or less degree by mechanical rupture of the tissues of the organ; but the occasional similarity in form of the coagula, to that presented by portions of wax injection thrown into the placenta by the utero-placental arteries or veins, would seem to show that in some cases the coagula are more or less completely circumscribed within the maternal cells of the placenta, supposing the description of these cells given by the Hunters, Meckel, and Hildenbrand, to be correct. M. Deneux states that, in the case already referred to, he found the effused blood partly situated in the interstices between the lobules or cotyledons, a circumstance which I have not had occasion to observe in any of the specimens of the disease that I have had an opportunity of examining.

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[1] Professor Thomson, in his Lectures, has long been in the habit of employing the old erm *ecchymosis* instead of *apoplexy*, to designate all morbid extravasations of blood into the issues or structures of the internal organs of the body. This use of the term *ecchymosis* in edical pathology seems to be preferable to that of apoplexy, inasmuch as it is consonant oth with the original derivation of the word, and with the signification in which it has ong been employed by surgical writers. Dr. Rush was, I believe, the first who in modern imes set the example of applying the term apoplexy in the somewhat perverted significa-ion to which I have above alluded. In his Essay on Pulmonary Consumption, after stating ifferent points of resemblance between the effusion of blood in the brain, and the effusion f blood in the lungs, he remarks—"From this great analogy between the remote and roximate causes of the two diseases, I have taken the liberty to call them both by the ame of *apoplexy*."—Medical Inquiries and Observations, vol. ii. p. 53.

The *particular vessel or set of vessels* which furnish the blood effused in placental congestion terminating in sanguineous extravasation, is liable to differ in different cases; but from the difficulty of accurately tracing the source of the hemorrhage in individual cases of the disease, I have little or no precise information to offer upon this point. When the hemorrhage takes place in the earlier weeks of pregnancy between the chorion and decidua, the blood probably escapes either from the new and tender decidual or umbilical vessels of the rudimentary placenta, or from both of these sets of vessels at the same time. When the blood is effused into the substance of the placenta, it is generally altogether impossible to trace the vessel or vessels from which it proceeds, and indeed the extravasation seems in many cases to occur at several different points at the same time. In a case detailed by MM. Grisolle and Caseaux,

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[1] Journal Générale de Médecine, tom. lxviii. p. 339.

[2] Recueil Periodique de la Soc. de Médecine, tom. iii. p. 23.

the Revue Médicale for 1833,[1] and in which coagula of blood
lf an inch in thickness were situated towards the fœtal surface
the placenta, some of the secondary divisions of the umbilical
ssels would seem to have been ruptured. Occasionally the
morrhage appears to proceed from a rupture, more or less ex-
nsive, of the vascular parenchymatous substance of the placenta.
ius Mr. Wildsmith[2] mentions a case where, on inspecting the
dy of a woman who died during pregnancy, there was disco-
red a large coagulum at the anterior portion of the fundus uteri,
ighing eighteen ounces, with a rupture of the placenta, two and a
lf inches in length. The blood in some instances, though origi-
lly poured out by vessels traversing the substance of the placenta,
terwards bursts through the tissues of that organ and reaches its
cidual or uterine surface, but in very many also of those cases in
hich the effused fluid occupies this latter situation, it may be traced
a direct rupture of the utero-placental vessels themselves.

The *quantity of blood* effused internally in placental hemorrhage
curring during pregnancy is generally small, in most cases not
ceeding a few drachms; but it is occasionally also, particularly
hen the blood is extravasated from the utero-placental vessels, be-
reen the placenta and uterus in the latter stages of gestation, so
eat as to endanger or destroy the life of the mother. In some
ses where the hemorrhage has thus proved fatal, the extrava-
ted blood has been found to be wholly collected between the
terus and central portion of the placenta, and to be completely
nfined within this circumscribed situation, by the edges of the pla-
nta preserving, around the whole circumference of the organ, their
atural attachment to the internal uterine surface. Albinus has
corded a fatal hemorrhage of this description in his Annotationes
cademicæ;[3] and Delaforterie[4] and Saumarez[5] have detailed similar
ses. The quantity of blood effused between the placenta and
terus in the kind of circumscribed cavity above alluded to, is stated
M. Delaforterie's case to have measured three French chopins, and
Mr. Saumarez's case it amounted to a pint and a half.[6] In
hese, and in similar instances of fatal placental or utero-placental

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[1] See also the Dublin Journal of Med. and Chem. Science, vol. v. p. 154.

[2] North of England Medical and Surgical Journal, vol. i. p. 446.

[3] "Parturienti placenta abscesserat, multo sanguine coagulato interjecto inteream et uterum.
ota autem marginis circumferentia nihil quicquam abscesserat, eoque ne sanguis proflueret,
rohibebatur. Quod quum non intelligeretur, mulierem interemit."—Annotat. Academ. lib.
cap. x. p. 36. This placenta, with the whole membranes of the ovum, is represented in
his author's Uteri Mulieris Gravidæ Tabulæ, tab. vi.

[4] Journal Générale de Médecine, tom. xxix. p. 384.

[5] New Medical and Physical Journal, vol. vi. p. 535.

[6] See also a case of miscarriage, by Dr. Thomas of Baltimore, in the American Archives
f Med. and Surg. Science. Ryan's Journal, 1836, vol. i. p. 268. A clot of blood of a pound
eight was effused between the uterus and placenta, which retained its attachment for an

hemorrhage occurring during gestation, death can scarcely be considered as an effect merely of the loss of the absolute quantity of blood effused. The attendant strong feeling of sickness and syncope, the paleness and coldness of the surface of the body, the state of the pulse, &c., would seem rather to indicate that the extravasated blood, by the sudden uterine distension to which it gives rise, or from other causes, produces a fatal depression of the action of the heart, similar to that which results from blows upon the epigastrium and severe injuries of the abdomen, and which we see supervening more slowly in cases of extensive peritonitis running on to a fatal termination.

The size or volume of the individual coagula or effusions of blood found in the substance of the placenta is liable to great variations. In some instances they are extremely small, not equalling a vetch seed in size; and again I find in my notes two cases mentioned, in one of which a single coagulum as large as a walnut is described; and in the other case, another coagulum is alluded to as equal in volume to a pigeon's egg. Wrisberg appears to have met with a placental sanguineous coagulum of much larger size than either of these two.[1]

When, in placental hemorrhage or apoplexy, the blood is extravasated into the substance of the placenta itself, it most generally appears, so far as I have myself observed, under the form of roundish coagula, often accurately circumscribed; but in other cases, the form and outline of these coagula are altogether irregular, and the transition between the sound and diseased portions of placenta, not very accurately marked. In some instances, one or two coagula only are observed, or the extravasation is confined to one or two lobes or cotyledons only; but generally, a considerable number of coagula are seen scattered throughout different parts of the same placenta, the sanguineous extravasation having occurred at several

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inch in extent; its remainder being greatly compressed, and its uterine surface presenting a concavity where the blood had been lodged. Blood had also been hemmed in by the adhesion of the membranes, so that not one drop was lost till the child was expelled. The abortion occurred at the sixth month.

Another case of suddenly fatal hemorrhage between the membranes, is reported by Ingleby in Ryan's Journal, iv. p. 752.

Mr. Coley, in the Lancet, 1829–30, states the proper treatment in these cases, viz., *immediate turning.*

[1] In describing the placenta of an aborted ovum, Wrisberg remarks, "Media ejus pars exhibet magnum ovatæ figuræ corpus, ultra septem lineas supra reliquam eminens superficiem, coloris fusci nigri, instar coagulati sanguinis, omnem placentæ latitudinem 3½ poll. æquantem occupat, cum tota placentæ longitudo 5¾ poll. sit. Maxima prominentis hujus corporis latitudo 2 poll. 6 lin. invenitur. In ipso eminentiæ centro funis inseritur umbilicalis, cujus sanguifera vasa in magnos dividuntur ramos," &c.—Descriptio Anatomica Embryonis.—Sandifort's Thesaurus Dissertionum, tom. iii. p. 227.

s, either simultaneously or successively. In one extreme case s kind which I have met with, the section of a placenta of about urth month, presented throughout its whole structure only gglomerated mass of distinct and separate roundish coagula, he organ was increased so greatly in thickness by these inter- effusions as to infringe very considerably upon the cavity of nnion.

en hemorrhagic effusions take place toward the fœtal surface placenta, or, in the earlier stages of pregnancy, between the a and chorion, the coagula of extravasated blood very fre- ly protrude these membranes before them, towards the cavity amnion, and appear in the form of projecting eminences or ions on the amniotic surface. These eminences vary greatly mber and extent in different cases. In some instances, almost part of the amniotic cavity is crowded with them, in others re seen only in that part which corresponds to the placenta. vary from the size of a very small pea to that of a hazel-nut, or . Occasionally, they form very slight projections only above neral level of the surface of the amnion; generally, they ap- s rounded, irregularly hemispherical eminences; and again I seen them standing out from the walls of the amniotic cavity, elongated nipple-like shape, and nearly as large as the first of the thumb. In several cases, I have seen the umbilical cord insertion into the placenta, at the same time greatly distended oagulated blood, for a distance of six or eight lines, giving an rance similar to that represented in two aborted ova figured binus[1] and Sandifort.[2]

substance of the placenta more immediately surrounding re- hemorrhagic extravasations, is occasionally darker red than al, from imbibition or infiltration of the effused blood into the boring portions of tissue, and sometimes the surrounding pla- structure is studded with a number of dark spots or points, bly originating in partial sanguineous extravasations, or in stagnated and coagulated in the cavities of some obstructed s.

nges in the Blood effused.—In internal placental hemorrhage, hymosis, the blood, after its effusion, undergoes a variety of es, interesting in themselves, and important in this respect, misconception of their nature has, as it appears to me, led logists into error in regard to the nature of some of the lesions onally observed in this organ. When the blood is poured out its containing vessels into the substance or cells of the pla- or between the membranes, it gradually coagulates, and assumes a very dark purple, and sometimes, as I have seen it in two preserved specimens,[1] almost a melanotic black color. After a time, however, it begins to lose this tint, the coloring matter gradually becomes removed, and the coagulum successively assumes a chocolate brown, a reddish or brownish-yellow hue, and latterly, if time sufficient is allowed, it presents a pale yellowish-white or straw-colored appearance, the fibrinous portion of the coagulum being then alone left. When these fibrinous and generally firm and indurated masses are divided, they for the most part exhibit internally a dense uniform or homogeneous tissue; but in some cases where the individual mass or coagulum has been formed by several successive effusions of blood occurring at the same point, and probably from the same vessel, its section shows a more or less perfect concentric laminated structure. M. Cruveilhier[2] has represented an ecchymosed placenta, in which the structure of the coagulum appears to be concentrically laminated in this manner, and the more external layers are seen to be losing their coloring matter, while the nucleus of the effusion is composed of darker and more recently effused blood. I have not had an opportunity of observing these changes in specimens of laminated placental coagula, but have had repeated occasion to remark the discoloration of the more common single or homogeneous coagula, proceeding from their circumference to their centre.

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[1] Annotat. Academ. tom. i. Tab. v. fig. 5.

[2] Observationes Anatomico-Pathologicæ, lib. iii. Tab. viii. fig. 4.

In the blood effused in placental hemorrhage, I have been able to trace a change still more advanced than that of its discoloration and conversion into a yellowish straw-colored fibrinous mass. In four instances, I have seen these fibrinous masses in different parts of the same placenta, contracted to a size considerably less than the space which they originally occupied, and consequently appearing, as it were, to be contained in cavities which were only but partially filled by them. This appearance is particularly well seen when the blood has been extravasated towards the fœtal surface of the placenta, or behind the chorion and amnion, and when it has protruded these membranes forwards in the form of the eminences or nipple-like projections already alluded to. In such cases, after the coloring matter of the effused blood has been removed, and time is given for the fibrinous masses which are left, to contract, the folds of the chorion and amnion, which they had carried before them, being inelastic membranes, remain in the position in which they were placed, and present themselves, when the ovum is afterwards examined, in the form of loose half-empty bags or sacs, projecting towards the cavity of the amnion. In one of the four placentæ in which I have observed this contraction, or diminution at least, of the fibrinous coagula, the cavities containing these contracted coagula were filled up with a limpid serum; but in the other three instances, I do not find in my notes any mention of such an effusion.

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[1] One of these specimens was in the rich and valuable pathological collection of Mr. Langstaff of London, the other is contained in the Anatomical Museum of the University of Edinburgh, and is thus described in the printed catalogue (p. 236): "Placenta about the fourth month; fœtal surface studded with numerous irregular dark-colored tubercles resembling melanotic depositions." The dark appearance of the sanguineous coagula in both of these cases, is such as might render them at first sight very liable to be confounded with actual melanotic deposits; and in the Edinburgh case this mistake in regard to their true pathological nature might have been the more readily committed, from the small masses of effused blood appearing of a beautiful bluish-black color, as seen through the membranes covering the fœtal surface of the placenta, owing to these membranes, as semitransparent whitish media, reflecting the blue rays of light transmitted through them from the dark surfaces of the coagulated masses of blood lying immediately beneath them.

[2] Anatomie Pathol. Liv. xvi. pl. i. fig. 1.

In no case have I seen the complete removal of the fibrinous coagula from their containing cavities, or any appearance whatever indicative of the organization of these coagula; and I have to regret not having directed my attention more particularly to the state of the placental tissue immediately surrounding the effusions, and observed the changes which take place in it upon the occurrence of these sanguineous extravasations. In some instances, as in those represented by Cruveilhier,[1] the surrounding placental structure appears to become atrophied and anæmic, after the effusion has occurred; it seems probable that, in other cases, this state and other diseased states of the placental tissue and vessels, may precede and indirectly give rise to effusions; but whether a cyst is ever formed around the sanguineous coagula, and under what circumstances it is formed, I have no data to determine, either from observations which I have myself made, or that I can find in the works of others.

I regret, further, not having in my possession any precise facts to determine the period of time which blood effused into the placenta, or between the membranes, requires, to undergo the several changes that I have mentioned; but I have seen them all effected in diseased ova which I had every reason to believe were expelled before the fifth and sixth month. The placenta I have formerly alluded to as presenting cavities containing at the same time contracted fibrinous coagula and an effusion of serum, was expelled at the eighth month, but was less in breadth, though of much greater thickness, than placentæ at that period generally are.

A number of the cases of tumors and circumscribed indurations of the placenta described by Morgagni[2] in his 48th Letter, of the scirrhous tubercles mentioned by Albrecht[3] and admitted by Murat[4] and Moreau,[1] and of those eminences on the surface of the amnion figured by Ruysch,[2] Sandifort,[3] Breschet,[4] Velpeau,[5] and Dr. Granville,[6] and spoken of by this last author under the name of tuberculated ova and placentæ, appear to consist in reality of coagula of blood, which have undergone more or fewer of the changes that I have attempted to describe. The white masses in the placenta, composed of a substance resembling both fat and cartilage, recently mentioned by Professor Stein,[7] not improbably consisted also of the fibrinous matter of effused coagula of blood. In the notes which I took of the first specimen of these yellowish fibrinous masses I had occasion to examine,[8] I find that I have described them as internally, "not unlike tuberculous or dense encephaloid matter;" but since that period I have had ample opportunities of correcting this conjecture, by an examination of above twenty additional specimens of this particular lesion; and I have now not only been able to trace in different placentæ the gradual transformations already described as occurring in the effused blood, from its appearance as a recent dark-colored coagulum, to its condition of a yellowish-white fibrinous mass; but I have seen more than one instance in which these two states, with all their intermediate gradations of change and coloration, could be observed in different coagula which had been effused at successive periods in the same individual placenta.

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[1] Anatom. Patholog. Livr. xvi. pl. i. figs. 1 and 2, and Livr. vi. pl. vi. fig. 2.

[2] De Sedibus et Causis Morb. Ep. 48, Art. 23, 24, &c.

[3] Acta Nat. Cur. tom. iv. Obs. 104. [4] Diction. des Sciences Méd. tom. xlii. p. 545.

Sandifort seems to have been aware of the nature of some of the morbid appearances in the placenta and ovum that I have described, and to have observed some of the principal changes which the extravasated blood undergoes, as appears from the following passage,[9] "vasa disrupta effunderant inter chorion et deciduam reflexam, sanguinem, qui ibi in grumos collectus erat. Talis effusiosi contigit, ova depurari omnino nequeunt, etiamsi diutissime aqua contineatur, nisi decidua externa et reflexa a chorio penitus tollantur. Contra solvi, tolli, coagulatus ille sanguis valet, qui membranis extrinsecus adhaeret, efficitque, ut *ova, abortu rejecta, referant ferme semper massam cruentam, sic ut quadam veluti carne condentur, atque tunc nitidæ puraeque membranae omnes apparent.*" Vater, as quoted by A. C.

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[1] Archiv. Général. de Médecine, tom. xxv. p. 124.

[2] Thesaurus Anatomicus Sextus, Tab. ii. fig. 4, and Tab. iii. fig. 7.

[3] Observationes Pathol. Lib. iii. Tab. viii. fig. 4.

[4] Etudes Anatomiques, &c. d'Œuf. Pl. iv. figs. 3.

[5] Embryologie Humaine, Pl. ii. figs. 8 and 9.

[6] On Menstruation and Abortion, Pl. vi. fig. 20.

[7] Kleinert's All. Repertorium, v. Jahr. 1 Heft. S. 35.

[8] This specimen was contained, with a second similar preparation, in the Museum of St. Bartholomew's Hospital.

[9] Observat. Anat. Path. Lib. iii. Cap. vi. p. 95.

audelocque,[1] mentions having seen an ovum like the one figured y Ruysch,[2] and afterwards a second ovum with the coagula in a ifferent state.[3] "Eandam fere faciem (ut Ruyschii) habebat ovum liquando oblatum, sed mulierum morositate iterum ereptum, hâc amen differentiâ, quod eminentiæ interius occurrentes, non duræ, ed vesiculæ sanguine grumoso plenæ fuerint."[4] Baudeloque decribes an ovum which he had himself met with, in 1823, having a umber of small tumors in its coats, consisting, as their examinaion convinced him, of effusions of blood in different stages of alteraion, "dans quelques tumeurs il n'y avait que la fibrine presque écolorée; dans d'autres la consistance du caillot etait moindre et a couleur plus foncée." It appears to be this same specimen that elpeau has figured.[5] He briefly describes the "bousselures" upon t, as made up "d'anciennes masses fibrineuses degenerées," and entions having seen, in 1833, one other similarly diseased ovum. reschet states, in reference to the case which he has figured, that e had not been able to determine the nature of the tumors seen rojecting on its amniotic surface: "Œuf humain que nous consierons comme offrant une alteration organique dont nous ne pouvons as determiner la nature."[6] Dr. Granville describes "the tubercles f placentas" as "formed by the enlargement of these filiform vessels hich exist on the inside of the chorion."[7] I consider it only as an ct of justice to myself, and, at the same time, as a strong corroboation of the accuracy of the pathological views which I have eneavored to develope in the preceding pages, to state, that I had rrived at these views with regard to the various changes which the blood effused in placental ecchymosis undergoes, altogether from he examinations of diseased ova which had fallen in my way, and before I was in any degree acquainted with the cases and notices of it that had been previously published by Sandifort, Vater, Baudelocque, and Velpeau, in the different passages that I have just now uoted from their writings.

Having taken up so much time in describing the anatomical chaacters of placental congestion and hemorrhage, I shall be very brief n my remarks upon its causes and symptoms, referring on these ubjects to the different treatises on abortion and uterine hemorhage, and more particularly to the excellent work of Baudelocque pon the latter.[8]

Causes of Placental Congestion.—The occasional exciting or determining causes of placental congestion may be considered as existing either on the part of the child or of the mother.

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[1] Traité des Hémorrhagies de l'Uterus, p. 57, 58.

[2] Loc. cit. tab. iii. fig. 7.

[3] See Vater, in Haller's Disput. ad Morb. iv. p. 706.

[4] Dissert. Inaugural. Mola Prægnans, &c., p. 22.

[5] Embryolog. p. 89, pl. ii. fig. 8.

[6] Etudes, &c. p. 156.

[7] On Abortion, p. 21.

[8] Traité des Hémorrhagies Internes de l'Uterus, Paris, 1831.

With the causes existing on the part of the child, we are as yet but very imperfectly acquainted; but it seems probable that all such morbid states and physical lesions and malformations of the fœtus and umbilical cord as have a tendency, directly or indirectly, to prevent or impede the free return of blood through the umbilical vein, will have the effect of producing more or less marked congestion in the minuter placental ramifications of this vessel, and perhaps may even lead in some cases, particularly if rapidly developed, to extravasations of blood from these ramifications. We now know that the fœtus in utero is liable to various febrile, contagious, malarious, and inflammatory affections—to plague, small-pox, and perhaps measles and scarlatina—to ague, and a number of acute internal inflammations; but whether the presence of these diseases in the fœtal economy is capable of exciting placental congestion or not through the long and tortuous tract of the umbilical arteries, and what morbid effects they produce upon the placental circulation in general, are points still open for investigation.

As occasional exciting or determining causes, on the part of the mother, of placental congestion and sanguineous effusion into the substance and upon the surfaces of the placenta and ovum, I may mention physical injuries, such as blows, falls, &c., great muscular exertion,[1] and bodily fatigue, excess of venery,[1] morbid irritations of the uterus and neighboring organs, acute febrile and inflammatory diseases, strong mental emotions, and, in general terms, all those circumstances which have a tendency to produce plethora and increased action of the whole maternal vascular system, and of the uterine system in particular. These various causes may produce placental congestion and hemorrhage in different ways; certain of them, as the physical injuries alluded to, may occasionally lead to this effect by directly or mechanically rupturing, by the concussion which they create, some of the placental and utero-placental vessels and attachments; others of them, and even the very same causes at other times, may act by inducing a morbid determination of blood to the uterus and placenta, or by exciting such muscular contractions of the uterus, as will separate to a greater or less extent the mutual uterine and placental connections.

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[1] Since writing the above, I find that M. Velpeau, in the last edition of his most elaborate "Traité de l'Art des Accouchemens," is inclined to doubt if succussions of the body, blows upon the uterine region, falls, &c., are really capable of producing any mechanical detachment of the placenta from the surface of the uterus. "Quand on se rappelle que l'œuf remplit exactement l'uterus, qu'il est exactement rempli lui-même par l'eau de l'amnios, on voit bientot que les mouvements imprimés a la femme par des secousses sont presqu'aussi incapables de separer le placenta de la matrice, ou le chorion de l'amnion, qu'ils le seraient d'isoler deux vessies emboitees l'une dans l'autre et dont l'interne serait completement rempli de liquide." Without discussing the point whether the uterus is always kept distended or not by its contents, in the manner here mentioned, a circumstance which appears to be extremely doubtful at least (see Ingleby on Uterine Hemorrhage, pp. 132, 133), and perhaps liable to variations in connection with the relative state of fulness of the uterine vessels, or rather with the relative state of tension of the uterine walls, resulting from the condition of its bloodvessels, and without appealing to the abstract laws of physics in refutation of M. Velpeau's opinion, I would merely adduce for that purpose the morbid phenomena that are every day observed to take place within the cranium—a cavity which we know for a certainty to be placed, after the fontanelles are closed, in the same physical relations with its investing membranes, and solid and fluid contents, as M. Velpeau conceives to exist between the gravid uterus, and its contained ovum. Concussions and external injuries of the head are well known to occasionally produce a rupture of the bloodvessels running between the cranium and dura mater, and a greater or less consequent effusion of blood between their detached surfaces, without any depression whatever of the cranium. I have known a case of this kind, where a large circular coagulum, three inches in breadth, was found effused in this situation, on the body of a man who was discovered in the street during the night, in a state of profound coma. In other cases of traumatic apoplexy, we find blood effused on the

With respect to the mode of action of general and local plethora and increased vascularity, in producing placental congestion and hemorrhage, I shall only venture to make one remark. If we admit, with some authors, that the newly formed vascular canals, which, on the doctrine of the Hunters and others, are believed to pass from the uterine vessels into the placenta, are less firm and resistant in their coats or texture than the other bloodvessels in the maternal system, it will follow, that when a greater than usual plethora or vascular action happens to occur in the sanguiferous system of the mother, or when, from any causes, the blood is determined in a particular manner to the divisions of the uterine arteries and veins, it will be most apt to become congested in the naturally little, if at all, elastic vascular tubes and canals of the placenta; and further, if the momentum of the whole mass of blood in the body of the mother, or of that in the branches of the uterine vessels alone, be preternaturally increased, the placental or utero-placental vessels or

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cerebral side of the dura mater, between it and the arachnoid, or between the arachnoid and pia mater, and in other rarer instances again, it is seen to be extravasated into the ventricles, or into the substance of the brain itself.

[1] In an interesting memoir on abortion among our domestic animals, inserted by M. Flandrin in Chabert's "Instructions et Observations sur les Maladies des Animaux Domestiques," vol. vi. pp. 107–164, it is stated that pregnant cows, mares, and sheep, miscarry in about twenty-four or thirty-six hours after sexual connection. The same effect is not observed in the sow. Entire herds of cows are said to have been seen to abort with dead fœtuses after unusually violent thunder-storms, probably, it is conceived, in consequence of the terror that seems to be excited in the minds of these animals by such an occurrence. Occasionally, all the cows of a dairy, or even of a whole district of country, have been observed to miscarry, one after another—a circumstance which some authors have endeavored to explain on the principle of contagion. See Tessier, in "Memoires de la Société de Médecine," for 1782–83, p. 549. Various curious notices on this interesting point in comparative pathology, are given by Mr. Youatt, at pp. 528–531 of his volume on the Breeds, Management, and Diseases of Cattle, in the Library of Useful Knowledge.

canals will be the first to yield or rupture under the generally or uniformly increased degree of internal pressure, inasmuch as they are the least capable of affording the requisite resistance to the distending force and equiversant pressure of the contained fluid. All the above effects will be naturally more or less promoted by the existence of any such diseased state or states of the tissue of the placenta, or of the placental and utero-placental vessels themselves, as diminish the elasticity and power of cohesion of the component tissues of these vessels, such as their stearoid, cartilaginous, or calcareous degeneration, &c.

Symptoms of Placental Congestion.—The symptoms produced by placental congestion and hemorrhage are, in many instances, obscure and difficultly appreciable, but in other cases, especially in those in which sanguineous extravasation occurs, the preceding and accompanying phenomena are frequently sufficiently characteristic and well marked.

Simple placental congestion, whether primitively seated in the fœtal or maternal vessels, seldom, perhaps if ever, exists for any considerable period of time, without more or less speedily superinducing a degree of uterine congestion; and again, the state of congestion or determination of blood to the vessels of the uterus, when occurring as the primary pathological affection, is probably as generally and directly followed by a similar condition of the placental vascular system or systems. Placental congestion, therefore, whether proceeding or not to extravasation of blood, usually appears to be either preceded or accompanied by those symptoms, which denote an increased activity in the uterine sanguiferous vessels, as by a feeling of oppression or weight, and tension or bearing-down in the uterine and pelvic viscera. If there exists a general plethoric state of the system, various febrile phenomena may also present themselves. Occasionally, the uneasy feelings in the uterine region proceed to a degree of fixed or intermittent pain, confined to one part of the uterus or uterine region only, or diffused over it generally. This pain seems frequently to extend to the loins and thighs, and a similar sensation appears in protracted cases to be sometimes even felt in the mammæ. At the same time the morning sickness and vomiting, with the other sympathetic affections, which so frequently attend upon the natural development of the uterus during pregnancy, are in most instances manifested in an increased or unusual degree.

When the placental congestion has proceeded to the extravasation of blood, besides the symptoms already mentioned others of a more unequivocal character generally present themselves. The effused blood may, as we have already seen, be poured out in situations from which it cannot escape externally. Perhaps, however, in the earlier months at least, in the greater number of cases, some portion of the blood is effused into situations such that a small quantity of it does escape per vaginam; and this circumstance, when found in connection with other symptoms, particularly with lumbar and uterine pains, and with the knowledge of the previous action of any of the directly exciting causes of placental congestion or hemorrhage, may be considered as furnishing very unequivocal marks of the existence of these states. If a pregnant woman, for instance, immediately after receiving a fall or blow, or any sudden succussion of the body, such as that occasioned by severe coughing or retching, be seized with sudden distensive uterine pain, and more or less effusion of blood externally, little doubt can, in most cases, be entertained, with regard to the nature of the internal morbid conditions which these symptoms indicate. One of these symptoms, however, the uterine or lumbar pain, or the external hemorrhage, may alone be present. The feeling of uterine pain, if occurring alone, can perhaps be but little trusted to as a diagnostic mark in the state of pregnancy, unless when taken into consideration with various collateral circumstances. In placental hemorrhage it is probably produced, as Baudelocque has supposed, by the mechanical distension of the uterus with the blood effused, and this distension can, it has been alleged, be sometimes, though I should conceive very rarely, actually traced externally in the enlargement of the absolute volume or bulk of the uterus. In some instances the pains remit or disappear entirely in the course of a few days or weeks, when the uterine enlargement has again proceeded considerably in its course, and in many other instances they pass sooner or later into the intermittent and regular pains indicative of uterine contraction. The external hemorrhage is, in some instances, observed to be in considerable abundance from the beginning; but more frequently it presents itself, in the first instance, as a few drops of blood, or of blood and serum only, afterwards becoming increased or not in quantity. In many cases this hemorrhage is seen to remit and return repeatedly, after the lapse of a few days, or at the distance of several weeks.

When placental hemorrhage occurs in the latter months of pregnancy, the internal effusion of blood is sometimes so great, particularly when proceeding from a rupture of the utero-placental vessels, as rapidly to produce all the most alarming symptoms caused by extreme loss of blood, and under such circumstances the case may even proceed to a fatal termination, without, as I have already mentioned, any sanguineous discharge whatever appearing externally. In other cases of placental congestion and hemorrhage occurring in advanced pregnancy, there is an external discharge as well as an internal effusion of blood; although no part of the placenta is found on examination to be placed over the os uteri—a case which I have not included, and consequently have avoided all allusion to, in the preceding observations.

Effects of Placental Congestion on the Fœtus.—When placental congestion occurs after the period of quickening, and is very acute in its character, or rapid in its appearance, the motions of the fœtus would appear to be occasionally rendered suddenly irregular, and at times almost convulsive. If the congestion is more chronic in its nature, the motions of the fœtus occasionally become extremely languid, or not at all appreciable; but under a timely detraction of blood, they may, as Madame Lachapelle[1] has remarked, again be reproduced after having ceased for several days. How much we may be assisted in the latter months of pregnancy by auscultation, in this and in similar cases, in our detection of the morbid states of the placental and fœtal circulations, it is difficult, if not impossible, at this moment to determine; but more accurate and extensive observations upon the subject may yet lead to some not unimportant results.

The occurrence of sanguineous effusion from the placental, utero-placental, or exposed mouths of the uterine vessels, though often in the latter and almost always in the early months of utero-gestation unattended with any great danger to the mother, and very frequently making no impression whatever upon her system, or not even betraying itself by any external discharge, must always be looked upon as a circumstance threatening the most direct and imminent danger to the life of the fœtus, for a very small quantity of blood, whether extravasated in the earlier weeks upon the villous surface of the chorion, or at a later period into or around the placenta, may be amply sufficient to arrest the processes of fœtal nutrition or respiration to such a degree, as effectually to impede the development, or destroy the life of the embryo, and in this latter way prove an indirect cause of abortion. For my own part, I feel assured that the preparations of aborted ova, contained in the pathological museums of this country, fully bear out, so far as I have myself seen them, the opinion of Professor Duges,[2] that the most frequent cause of abortion is active placental congestion, a term under which he at the same time includes placental hemorrhage. This remark, I believe, will be found in particular to apply to abortion in the earlier months. In the greater proportion of cases which I have met with, of ova aborted in consequence of internal placental hemorrhage, I have found the ovum itself of the average size of an orange, and the embryo arrested at about the fifth or sixth week of development, or at that stage at which the extremities of the body first begin to appear in the form of rounded buddings. Expulsive uterine contractions had not, however, supervened in many of these cases for several weeks after the internal sanguineous effusion had actually taken place, and in some of them, the general growth of the membranes and placenta appeared to have gone on during that interval, while in one or two cases the embryo itself had also enlarged to nearly the size of the thumb, although the individual development of its extremities, and perhaps of other organs, had been arrested.

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[1] Pratique des Accouchemens, tom. ii. p. 323.

[2] Diction. de Médecine et Chirurg. tom. viii. p. 309.

INFLAMMATION OF THE PLACENTA.

The next series of Morbid Conditions of the Placenta to which I have to direct attention, consists of *Inflammation and its various consequences*. It is only within these few years, that the state of inflammation has been distinctly recognized by pathologists, as a morbid action liable to present itself in the placenta, and though much of the pathological and nosological history of the disease certainly still remains to be made out, yet I believe a considerable store of interesting information may, even at the present moment, be collected in regard to it, by taking a generalized view of the different scattered cases and notices of the affection, which have been published by Brachet in his original memoir on this affection, and by Cruveilhier, D'Outrepont, Dance, Stratford, and other authors.

Placentitis, or Inflammation of the Placenta, would appear to be in some cases acute in its character and progress, in others chronic, most frequently perhaps the latter. It may have its seat either in the proper parenchyma of the organ, in the membranes investing its fœtal, or in that investing its uterine surface, or in two or all of these parts at the same time. When seated in the parenchyma of the placenta, or on its uterine surface, it would seem to be in some cases strictly limited to one lobe or cotyledon of the organ; in other cases it coexists in two or more isolated and even distant lobes; but more frequently it appears to attack simultaneously or successively several adjacent lobes, and in this way it may come to involve a third, the half, or even the whole extent of the viscus. In a considerable proportion of instances of Placentitis, the inflammatory action seems to be originally propagated from the adjoining substance of the uterus to the placenta, the first symptoms being those usually considered as characteristic of Metritis, but in other instances the inflammation appears to originate in, and to be strictly confined to, the tissue of the placenta itself or its membranes, without spreading to the substance of the contiguous portion of the uterus; and in other cases, again, there is every reason to believe, that, though originating in the placenta, the inflammation may spread from it to the uterus. The occasional primary origin of inflammation in the substance of the placenta itself seems to be fully proved by various considerations—by its sometimes appearing only in one lobe, or in several circumscribed distant and isolated parts of the viscus at the same time—by its attacking one placenta only, in cases of twins or triplets—and by its being occasionally strictly confined in its seat to the fœtal surface or side of the organ alone.

Anatomical Characters of Placentitis.—The modes in which placental inflammation terminates, and the morbid lesions and products to which it gives rise, differ greatly in different cases, according to the more or less acute or chronic character of the inflammatory action, the particular seat which it occupies, and other circumstances connected with it. In arranging the observations to be made upon the morbid anatomical characters of the disease, I shall divide it into three stages, the first including the stage of inflammatory congestion, and effusion or secretion of serous fluid; the second, that of the effusion or secretion of fibrin or coagulable lymph; and the third, that of the secretion or effusion of purulent matter.

First Stage.—In the first stage of placental inflammation, or that of *Inflammatory Congestion*, the placenta is larger in volume, and darker in color, than natural, owing to the unusual accumulation or engorgement of blood in its vessels: it feels comparatively denser or more solid to the touch, and, as it approaches the second stage, an effusion of serum, or of serum and coagulable lymph, occurs, and the tissue of the organ becomes more lacerable under the pressure of the finger.

It would be extremely difficult, if not impossible (if we may judge from the analogy of the lungs and other organs), to distinguish, by any specific anatomical characters, between the sanguineous engorgement of the placenta, which constitutes the first stage of inflammation, and that active non-inflammatory form of placental congestion which I have already endeavored to describe. Indeed it is highly probable that the non-inflammatory congestion or engorgement occasionally passes into that which is inflammatory, and yet they would appear to be two morbid states intrinsically distinct from each other, and differing widely in the morbid effects and products to which they give rise. The non-inflammatory congestion will perhaps be found, almost always, generally diffused over the whole placenta, while the congestion of inflammation is often confined to a number of particular lobes. The former leads to an extravasation of blood, and sometimes of serum—effusions purely the result of mechanical over-distension of the vessels; the latter, again, gives rise to various morbid products which are observed to result from inflammatory action in other organs of the body, and which are not mechanical extravasations, but vital, though morbid, secretions. In any individual case, however, of the first stage of placental inflammation, and of congestion, it is often difficult, and not unfrequently impossible, to determine its true nature from the mere anatomical character of sanguineous engorgement of the organs alone, and frequently it is only when we find this state of the placental mass combined with other more decisive proofs of the action of inflammation—as with softening of the substance of the organ, with effusion of turbid serum, of coagulable lymph, or of pus, that we can decidedly pronounce upon its actually inflammatory character.

Second Stage.—The second stage of placentitis is, as I have already stated, characterized by the inflammatory action having proceeded to the effusion or secretion of coagulable lymph. The morbid appearances and effects produced by this second stage of the disease vary greatly, according as the effused coagulable lymph is deposited —1st, in the interstitial or parenchymatous substance of the placenta, or is effused, 2d, upon its uterine, or 3d, upon its fœtal surface.

1. Few well-marked or accurately detailed cases of *acute* Placental Inflammation, producing an effusion of coagulable lymph into the parenchymatous texture of the organ, have as yet been put upon record. From the morbid appearances, however, observed by Brachet,[1] D'Outrepont,[2] and others, in a few cases of placentitis referable to this stage, and from what I have myself observed in a recent specimen of this form of the disease, the following, I believe, may be deduced as its principal characteristic anatomical characters. The affected part of the organ is of a more or less deep red color; its texture when cut into, is found solid and dense, and resembling in consistence and appearance a portion of healthy liver, or of pulmonary structure affected with red hepatization. It is inelastic to the touch, but though increased in compactness and solidity, it is still in general so friable, particularly when the inflammation is acute or recent, as to be readily lacerated or ruptured under any considerable pressure. It generally contains more or less of a brownish or reddish turbid liquid, which exudes from its incised

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[1] Loc. cit. Obs. 1, p. 11.

[2] Gemein. Deutsche Zeitschrift, Bd. v. Heft. 4, s. 556.

surfaces under gentle pressure, or when these surfaces are scraped with the back of the scalpel.

But the form under which we most frequently meet with the deposition of coagulable lymph in the parenchymatous structure of the placenta, is that of reddish and grayish-yellow *induration*, the effect, I believe, of *chronic* inflammatory action. This morbid state of the placenta has been long known to accoucheurs, and, with some other morbid states of the organ to be afterwards mentioned, has generally been described by them, under the name of scirrhus of the placenta. It has evidently, however, no pathological relations with scirrhus, at least in the sense in which that term is used at the present day, as designating a carcinomatous state or degeneration of an organ. Chronic induration of the placenta occurs in cases in which we have no evidence of the presence of the carcinomatous diathesis, in the coexistence or subsequent occurrence of scirrhous disease in any of the other organs of the body, either of the mother or of the infant—it has never been remarked in any instance, so far at least as I am aware, to ulcerate, or fungate, or take on any of the usual malignant action of scirrhous degenerations—and the anatomical characters which the indurated placenta exhibits, differ from those which are usually considered as characteristic of true scirrhus.[1]

On the other hand, that Induration of the Placenta is the result of chronic inflammatory action, and the deposition of coagulable lymph, seems proved by the consideration of the nature of the morbific causes by which it is produced—generally blows, falls, and such external injuries as are calculated to give rise to inflammatory action, received several weeks or even months previously to the expulsion of the indurated organ—and by the fact, that it is sometimes possible in different placentæ, or even, as I have seen, in different parts of the same placenta, to trace the gradations between the increased solidity of acute hepatization, and that of the most advanced and best marked variety of chronic induration.

The anatomical characters of Induration of the Placenta appear to vary considerably in different cases, according to the extent of the disease, and the more or less chronic character of the inflammatory action which produces it. In most cases the indurated part is of a reddish or pink-gray hue, but sometimes, probably in its more advanced stages, it presents a citrine yellowish color. Its section sometimes exhibits a uniform, very compact, and occasionally a somewhat stearoid, lardaceous, or fatty-looking structure; it contains little or no fluid, and in some instances has a dry and desiccated pearance, approaches the density of cartilage, and emits when cut creaking sound. It very often affects only one lobe, or two or ore contiguous or distant lobes, and in other instances it implicates e whole or nearly the whole of the placental mass. The portion portions of placenta affected with chronic inflammatory induraon are occasionally found united by adhesive inflammation to the joining portion of the uterus, in whole or in part; but in other stances no such adhesive action has occurred, and indeed the isease may be limited altogether to the fœtal side or surface of the lacenta.

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[1] Portions of placenta are often indurated in cases of its presentation over the os uteri—See Collins' Pract. Treatise, p. 102; and Dr. Moir in Hamilton's Observ., last page. See also vol. i. of this work, page 647.

2. The effusion or secretion, however, of coagulable lymph, as a rmination of placental inflammation, is best known by the effect which it not unfrequently gives rise, of producing more or less timate and extensive *adhesion* between the uterine surface of the lacenta and the inner corresponding surface of the uterus, constitting a morbid state of the after-birth that frequently forms a most rmidable and dangerous cause of uterine hemorrhage after the pulsion of the child.

The extent of the morbid adhesion of the placenta to the uterus -the seat of the adherent portion—and the strength or intimacy of e adhesion, are found to be liable to great variation in different ses. In a few rare instances the whole or almost the whole of the rgan is found intimately connected with the internal surface of the terus, but far more frequently the adhesion is limited to a single be, or to a few adjoining lobes. The adherent lobes are occaonally those seated in the centre of the placenta, and in other ses those placed at the circumference of the organ only. I may fer to Dr. Ramsbotham's[1] third case, as an instance of the former, nd I shall afterwards have particular occasion to notice a remarkable ample of the latter observed by Dr. Hamilton.[2]

Occasionally, when the placenta is only partially adherent to the ternal surface of the uterus, the non-adherent portion is brought way after the expulsion of the child, by traction at the cord, or by e introduction of the hand, while that which is morbidly adherent still left more or less firmly attached to the internal surface of the terus. Dr. Ramsbotham[3] has described a series of instances of this ind, under the title of disruption of the placenta; and Mr. Ingleby,[4] his able work on Uterine Hemorrhage, has referred to several fatal ses and post-mortem inspections, in which a similar accident had curred. In one of these post-mortem inspections, where the woman d died on the eleventh day after delivery, a number of pieces of acenta were found still firmly adherent to the fundus uteri. In a second case, where death had occurred twenty or twenty-one days after delivery, many small masses of placenta, in a black and offensive state, were seen studding the internal surface of the fundus, and part of the body of the uterus; these masses did not separate by maceration. In a third case, a portion of placenta, about the size of a small egg, was attached very firmly to the fundus of the uterus, and could not be separated either by the finger or by maceration. The patient had died of irritative fever on the thirteenth day after delivery. In a fourth case, where the patient sank on the eleventh day after delivery, Mr. Ingleby found a layer of placenta, covered with a thin purulent-looking fluid, nearly circular, and three inches in diameter, very fibrous, and rather white in appearance, adhering somewhat tenaciously to the fundus. In a fifth case, he found, five weeks after delivery, a portion of placenta about the size of a walnut, attached to the fundus of the uterus. The adherent portion of placenta had apparently acquired or retained a degree of organization; it was of a somewhat florid color; the lining membrane of the uterus covered the greater portion of the mass, though not its centre, which was ragged, and vessels could be traced opening upon it.

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[1] Practical Observations in Midwifery, part i. p. 99.

[2] Practical Observations on various Subjects relating to Midwifery, 1836, part i. p. 286.

[3] Practical Observations, p. 164, et seq.

[4] On Uterine Hemorrhage, p. 205, et seq.

Dr. Biancini,[1] in a lecture on the connection between the blood of the mother and fœtus, published at Pisa in 1833, mentions a case similar to the above. A woman who had suffered greatly during parturition died eight days after delivery. The attempts made to bring away the placenta had occasioned a profuse and alarming hemorrhage. After death an injection of glue and vermilion thrown into the uterine vessels, filled all the uterine arteries and penetrated into the vessels of a considerable portion of placenta, which was still adherent.

The intimacy or strength of the morbid adhesions between the placenta and uterus is liable to vary greatly according to the degree of acuteness of the inflammatory action which has originally produced the effusion of coagulable lymph, and the length of time it has been deposited. When the coagulable lymph, on which the adhesion depends, is soft and recent, and the effect of acute inflammation, the degree of adhesion between the uterus and placenta is scarcely greater than natural. If, however, the inflammation be more chronic, and the deposition of coagulable lymph has become organized, the placenta is with greater or less difficulty separated from the uterus, by the uterine contractions, or by the hand of the accoucheur. In some instances the organic union between them is so firm, and the corresponding surfaces of the uterus and placenta are so intimately blended together, that the uterine contractions are ltogether inadequate to break up the morbid organic connections xisting between them; and occasionally it has been found impos-ible to disunite them completely from one another by any manual fforts during life, or even by dissection after death.

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[1] Sul Commercio Sanguigno tra la Madre et il Feto, 4to Esperimento.

The thickness of the layer of coagulable lymph uniting the pla-enta to the uterus, in cases of morbid adhesion between their con-iguous surfaces, is sometimes very considerable. Wrisberg[1] men-ions a case of very strong, and apparently universal, adhesion of he placenta, in which he found the uterine surface of the organ overed with a membranous substance two lines and a half thick, a the intervals between the lobes, and from a line and a half to wo lines in thickness where it covered the surface of the cotyledons. In this and similar cases, is the coagulable lymph effused, at first, into he substance of the placental decidua, causing its morbid thicken-ng; or is it principally thrown out upon the uterine, or upon the etal side of that membrane; and are the cavities of the decidual tero-placental vessels liable to become obstructed with it? Is the nflammatory effusion of lymph, producing the morbid adhesion of he placenta to the uterus, furnished by the decidual, or decidual nd proper placental vessels? or is it chiefly thrown out by the essels of the adjoining portion of the internal surface of the uterus? r are not all of these vessels generally the seat of inflammation nd the sources of the effusion at the same time, owing to the in-nflammatory action, when it originates in any of them, in most nstances speedily spreading by contiguity of texture from the one o the other? Occasionally the coagulable lymph effused between he placenta and uterus is changed not only into a filamentous or ellular morbid structure, but as happens also in the false membrane f chronic pleuritis, it sometimes assumes a tendinous or cartilagi-ous appearance. Thus Wrisberg states that, in the case above eferred to, the thick membrane covering the placenta was whitish, ompact, and very dense, and of a cellular or rather tendinous tructure (*albicante, compactâ et durissimâ, cellulosâ quasi tendineâ*). his membrane, forming the bond of union between the adherent lacenta and uterus, may also, as we shall afterwards find, be the eat of calcareous deposits, &c.

Cases such as I have alluded to above, in which the morbid nion between the placenta and uterus is so intimate as not to per-it of the two organs being readily separated by the hand, or by e scalpel even upon the dead body, are certainly of rare occurrence. alzmann,[2] however, Plater,[3] and Morgagni,[4] may, from among others, be cited as having recorded striking examples of it. Salzmann's case, the woman had died immediately after deliv and on opening the body, he found "secundinam undiquaque utero tam firmiter adhaerentem ut vix et nonnisi labore sepa potest." Plater describes the placenta as so adherent to the ut on the dead body of a woman who had sunk under similar circ stances, "ut aegre manibus avelleretur, et tracta laceraretur." case recorded by Morgagni is one of still greater interest. subject of it, a woman who during the course of pregnancy had b lifting very heavy weights, suffered a premature labor at the seve or eighth month, and died, with the placenta almost wholly retai on the eleventh day after delivery. On the post-mortem ins tion of the body, part of the placenta was found detached, and ha ing down through the os uteri; but another portion of it was sti strongly adherent to the uterus, that it could scarcely be separ from it even with the aid of the scalpel (*utero dicte adeo affigebatu vix posset, cultri etiam ope, separari*). The adherent portion of placenta was indurated, and the corresponding surface of the ut inflamed.

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[1] Commentat. Medic. Obstetric., &c., Argumenti, p. 45.

[2] Observat. p. 43; Bonetus' Sepulcretum Anatomicum, tom. iii. p. 121.

[3] Observat. Lib. i. p. 231; Bonet. loc. cit. p. 121.

[4] De Sedibus et Causis Morbor. Epist. xlviii. Art. 28.

Of late years several cases have been published in Germany Professor Naegele[1] of Heidelberg, and by Salomon[2] and Burger England by Dr. Rigby,[4] in France by M. Gabillot,[5] and in Ame by Dr. Porcher,[6] in which adherent and retained placentæ been supposed to be either totally absorbed by the uterus or to become organically and permanently incorporated with its substa Madame Boivin[7] has endeavored to explain away the cases give Naegele and Salomon, by supposing, that in those instances w the placentæ had been conceived to be retained, they had actu passed off, but escaped observation, from their consisting of a me membranous structure, or being otherwise malformed. Gran that this explanation may bear upon some of the cases that been published, it certainly, I think, cannot be made to appl others; and without at all questioning the good faith or power observation of those who have given the cases hitherto recor the question may at present be permitted to remain *sub judice*, u more decided evidence is brought forward either in its favor against it, and in particular until an opportunity occurs in whic uterus can be examined in which this supposed process of place absorption is actually going on, an important link in the chai evidence, but one which, so far as I know, is still wanting.

[1] Froriep's Notizen; Bibliotheque Médicale, 1829, tom. i. p. 366.

[2] Kleinert's Allgemein. Repert. vi. Jahrgang. 2. Heft. s. 72.

[3] Rust's Magazin für Gesam. Heilkunde. Bd. xxxv. s. 156.

[4] London Medical Gazette, vol. xiv. p. 333.

[5] La Clinique, 29th August, 182

[6] American Journal of the Medical Sciences, No. xx. p. 391.

[7] Observations et Reflexions sur les Cas d'Absorption du Placenta, Paris, 1829.

Various pathologists seem inclined to reject at once the idea of the absorption of the placenta, upon the ground, that it is altogether impossible that the uterine absorbent vessels could ever remove so large and solid a mass as that of which the human placenta is composed. If the facts, however, upon which such an objection can alone be legitimately founded are more carefully examined, they will, I fear, be found to weigh, so far as they have been hitherto observed, as much at least in favor of the question at issue as against it. Salomon has adduced various instances from Bartholin,[1] Carus,[2] and Huzard,[3] of what appear to be accurately observed cases of the removal from the uterus of the cow, of the soft parts of the fœtal calf, nothing having been found in the uterine cavity after death except a mass of fœtal bones. This occurrence seems to take place after the full term of pregnancy has been completed, and when the uterus has been unable to rid itself of its contents, in consequence of some obstacle in the pelvis, or otherwise, to the passage of the calf. Cases of this kind, I believe, are not extremely rare.[4] I have myself had two instances reported to me, by persons upon whose powers of observation I can implicitly rely, where the uterus of cows contained a mass of fœtal bones. Mr. King has recorded a similar case in the Veterinarian for January, 1834. Jæger[5] mentions having discovered a fœtus reduced to a mere skeleton, in the uterus of a deer.[6] In extra-uterine pregnancy in the human subject, the fœtus has been occasionally discharged by the intestinal canal, through the abdominal parietes, or through the cavity of the urinary bladder or vagina, in the form of loose bones.[7] In these cases, are the soft parts of the body of the fœtus and its placenta always, by a suppurative or other morbid action, decomposed and diffused through the fluid contents of the extra-uterine cyst, and afterwards discharged externally, previous to or along with the fœtal bones? In the case of the cow, does a similar process of decomposition and discharge of the soft parts take place while the bones are retained in utero?—or are the soft parts and appendages of the fœtus ever removed in either of these two sets of cases from the cavities containing them by a process of absorption? I am not aware of any facts observed with a requisite degree of accuracy, from which to deduce any decided answer to these questions, but they appear to me to be subjects that deserve to be minutely and carefully attended to, in the observation of future similar cases; and if, in any sufficient number of instances, it can be proved that the uterus of the cow is actually capable of removing by an act of absorption, the soft parts and appendages of a contained fœtus, as is supposed by Salomon, and admitted also, I believe, by Carus and Huzard—or if by a similar process the placenta and soft parts of a human fœtus can be removed from an extra-uterine cyst, it would certainly seem unreasonable to reject altogether the idea of the absorption of a morbidly adherent human placenta, upon the sole ground that an absorbent action of the degree or extent required for that purpose, could not possibly be carried on by the internal uterine surface. When an extra-uterine fœtus becomes incrusted with calcareous matter, and is retained for a long period in the body of the mother, its placenta appears to be sometimes either removed or changed in such a manner that it cannot be recognized. In the case, for example, recorded by Dr. Cheston,[1] where the fœtus was supposed to occupy the cavity of the uterus itself, it is stated, that no remains of the membranes, placenta, or navel-string could be discovered, except the insertion of the last into the body of the child.

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[1] De insolitis Partûs Humani viis, p. 37.

[2] Zur Lehre von Schwangerschaft und Geburt-Erste. Abtheil. 1822. s. 2, 50.

[3] Memoires de l'Institut National des Sciences et des Arts, tom. ii. Scien. Mathem. et Physiq.

[4] Dr. Plot, in his "Natural History of Staffordshire,'' 1686, p. 262, mentions a cow, "that being observed not to cast her calf in due time, was fatted up and killed, in whose matrix (when opened) there was found the skeleton of a calf, all the bones hanging entirely together, as in tab. 22, fig. 13, lying in a reddish weighty substance somewhat like bole Armeniac, the fleshy parts being either wasted by corruption, or dried up by the heat of the womb, like the calf of John Huswig, minister of Fredericksburg, mentioned by Bartholin."—Bartholin, Hist. Anat. Cent. 2, Hist. 2.

He mentions (p. 259), "a cow killed at Hopton that had an entire perfect calf, and the bones of another found in her."

[5] Meckel's Archives für Anat. und Physiologie, Bd. xi. s. 91.

[6] Dr. Plot again remarks (p. 259)—"In the breeding of deer, it has also been observed at Chartley and elsewhere in this county, that upon killing of them there has been sometimes found the bones of young fawnes in the matrixs of the does, which doubtless have dyed in them upon some unfortunate blow or other accident."

[7] Montgomery on Pregnancy, pp. 68, 69. Cases in the human female of retained fœtal bones discharged during a following pregnancy. Dr. Joy, American Journal of the Med. Sciences, No. xi. p. 33—Case of a woman where, after liquor amnii was discharged, labor pains ceased. Afterwards a discharge of matter and bones took place from the vagina.

3. The fœtal as well as the uterine surface of the placenta, or at least the membranes covering the fœtal surface of the organ, are also liable to be the seat of the inflammatory secretion of coagulable lymph, or, in other words, of adhesive inflammation. Cruveilhier has mentioned a case[2] where the placental surface of the chorion was penetrated with false membrane, the cellular tissue between the chorion and amnion likewise infiltrated, and the placenta itself enlarged and œdematous, or containing a considerable effusion of serum. Mr. Stratford of Boston has also detailed the particulars of an interesting case of absorption at the third month of utero-gestation, where the placenta was large, soft, and spongy,

Head removed by operation, an incision being made through abdominal parietes into the uterus (?) near umbilicus, where an opening had formed.

[1] Medico-Chirurgical Transactions, vol. v. p. 231.

[2] Anatomie Pathologique, livr. xvi.

d covered with flocculi of coagulable lymph, particularly on its tal surface. The membranes of the ovum appeared to be thicker d more opaque than natural, and yellow flocculi of coagulable mph were floating in the liquor amnii.

I have seen more than one instance in which the portion of the nnios covering the placenta, and other parts of the surface of the embranes, were partially coated by an effusion of coagulable mph; and M. Mercier,[1] in his papers on dropsy of the amnios, s alluded to the same morbid appearance. I shall presently have casion, when speaking of the effects of Placentitis, upon the tus, to allude to a series of interesting cases, in which the interl surface of the placenta has been found intimately attached by hesive inflammation to the head and abdomen of the child, and s seemed in consequence to act as a cause of some varieties of alformation.

Third Stage.—The third stage of Placentitis, according to the vision previously proposed, consists in the effusion or secretion of urulent matter. The purulent matter secreted in placental inflamation has now been found occupying several different situations, d deposited under different forms. In one case mentioned by Brachet,[2] he found two circumscribed abscesses, each as large as pigeon's egg, situated beneath the fœtal surface of the placenta; ese deposits were circumscribed in their locality, although no disnct cysts isolated them from the surrounding placental tissue. In nother placenta, M. Brachet[3] found an abscess occupying threeurths of the extent of the organ, protruding towards its fœtal rface, and containing as much as a wine-glassful of purulent atter mixed with blood; and in a third case,[4] he observed a numr of little abscesses, varying from the size of a small pea to that a nut, scattered throughout the substance of the placenta, and rrounded by hepatized structure.

A case of abscesses in the placenta is also given by D'Outrepont.[5] this case, the mother, who was hereditarily phthisical, had been egnant for the first time. The child, an emaciated boy, was born at e eighth month, alive, but died in a few days. Between the ssels of the placenta there was found a fœtid, brownish fluid, and several points hard and friable nodules; in general structure, it sembled hepatized lung. Cruveilhier[6] seems to have met in veral instances with concrete purulent matter, infiltrated through the tissue of the placenta, accompanied by an effusion of coagulable lymph between the portions of chorion and amnion investing the fœtal surface of the organ, and surrounding its circumference. In these cases, the texture of the placenta was denser than usual, much less vascular, of a somewhat lardaceous appearance, and, as it were, toughened and indurated.

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[1] Journal Générale de Méd. xlv. p. 256, &c.

[2] Loc. cit. Obs. v. p. 22.

[3] Ibid. Obs. v. p. 23.

[4] Ibid. Obs. vi. p. 25.

[5] Salzburg Zeitung, 1831, vol x. p. 185.

[6] Anat. Pathol. Livr. i. and Revue Médicale, 1830, tom. ii. p. 410.

The deposition of purulent matter on the uterine surface of the placenta is mentioned by the late M. Dance,[1] in an interesting case of fatal vomiting and inanition during the early months of pregnancy. Between the placenta and uterus there was a layer of concrete pus, which could be lifted up by the back of the scalpel, in the form of yellowish flocculi, and whitish pseudo-membranous concretions existed between the membrana decidua and the internal surface of the uterus. Breschet,[2] in his essay on the human ovum, states that he had met with two analogous cases of purulent and pseudo-membranous effusions between the placenta and uterus, and over the surface of the membrana decidua, the substance of this membrane being at the same time thickened and infiltrated with purulent matter. In a memoir on the diseases of the uterus, inserted by M. Dance in the Archives Générales for October, 1829,[3] he has described a case of abortion preceded by febrile and inflammatory symptoms, and where there was a layer of soft coagulable lymph and purulent matter deposited, not, as in the preceding instances, on the uterine, but on the fœtal surface of the placenta, between its chorion and amnion; the placental portion of the chorion was at the same time opaque, and greatly thickened.

Before concluding the description of the anatomical characters and effects of placentitis, it is almost unnecessary for me to remark, that in the same placenta, two, or all the different stages of inflammation are very often found coexisting in different cotyledons and surfaces of the organ, or even in the same cotyledon and surface; and that every shade and transition may be occasionally observed in an inflamed portion of the viscus, from congestion to the effusion of coagulable lymph and of purulent matter, either in an acute or chronic form. In proof of this remark, I may merely refer to the instances of the disease that I have last quoted, and to the 1st, 5th, and 6th cases of it reported in M. Brachet's Memoir.

In most of the cases of placental inflammation hitherto published, the disease has only been recognized at the time of delivery, and neither the causes which gave rise to it, nor the symptoms which it had produced, have in general been accurately traced. Very limited data, therefore, it must be allowed, exist with regard to these parts of the history of Placentitis. These data, however, so far as they go, are not without interest or practical importance; and in order, if not to arrive at, at least to approximate to, as accurate a degree of information as they are calculated to afford, I have selected twenty of the recorded cases of the disease, from among others given in the writings of Ramsbotham, Brachet, Dance, Stratford, and D'Outrepont, for the purpose of comparing and analyzing their phenomena with particular reference to the points I have mentioned. In making the selection of these twenty examples of Placentitis, I have acted solely on the principle of uniformly rejecting all those cases of the disease to which no kind of commemorative history was attached, and which, consequently, could be of no use for the purpose at present in view, while I have as uniformly retained all those other instances of the affection that had any commemorative history connected with them; and I only regret that in a considerable number of these cases, this history is so imperfectly given as to furnish little or almost no precise information.[1]

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[1] Repertoire Générale d'Anatomie, tom. iii. p. 71.

[2] Etudes Anatomiques, &c. Sur l'Œuf, dans l'Espece Humaine, pp. 123–24.

[3] Archives Général. de Médicine, tom. xxi. p. 222.

Causes of Placentitis.—With respect, however, to the *exciting* causes of placental inflammation, I may, in the first place, mention that M. Brachet enumerates as such, blows upon the belly, falls, violent succussions, sudden and great movements, frights, emotions, all kinds of lively and profound sensations, and diseases of the mother, particularly metritis and other inflammatory complaints. In turning to the individual cases given by M. Brachet I find that in two instances (Cases X. and XVI.), the disease seems to have come on while the mother was suffering under great mental anxiety;—in a third case, the only assignable exciting cause was a severe fright which the mother had experienced ten days previously to an abortion at the fourth month of pregnancy, the placenta of which was in the first stage of inflammation; and in a fourth instance, the mind of the patient had been kept harassed during the whole period of pregnancy by domestic quarrels. In two other of M. Brachet's cases (XII., XIII.), as well as in two cases of adherent placenta, mentioned by Dr. Ramsbotham (I., VIII.), the inflammatory action was apparently produced by falls. In one of Dr. Ramsbotham's cases (VII.), the affection was attributed to carrying a heavy basket against the side during pregnancy, and in the instance of acute placentitis given by D'Outrepont (XX.), too tight lacing, with a view to conceal pregnancy, appears to have acted as the exciting cause. In two of Dr. Ramsbotham's cases of adherent placenta (II., V.), the inflammatory action productive of the adhesion seems to have been excited by blows upon the abdomen. In one of these cases (V.) where the blow was received upon the fore part of the abdomen, the placenta is afterwards mentioned in the history of the delivery as firmly united to the fore part or corresponding portion of the anterior surface of the uterus. I refer to this case the more, that Saxtorph[1] and Wenzel[2] state that the placenta when attached to the anterior wall of the uterus is frequently morbidly adherent—a circumstance that D'Outrepont[3] says is consonant with his own experience, and one, I conceive, for which we may perhaps account by supposing, that the organ, when occupying this situation, will be more exposed to the physical injuries already alluded to as capable of giving rise to placentitis either directly or indirectly, by exciting, in the first instance, inflammatory action in the contiguous or corresponding portion of the uterine parietes. Of the remaining eight cases of placentitis upon the list, no allusion is made to any exciting cause of the disease in the history of five of the cases (III., IV., VI., XVII., XVIII.), and in the remaining three instances (IX., XIV., XIX.) it appears that no exciting cause could be traced, although inquiry was made for it.

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[1] The following note contains a list of these twenty cases. In order to facilitate the references that are made in the text to the cases individually, I shall affix a particular numeral to each, and at the same time state, for the sake of greater precision, the kind of inflammatory lesion which the placenta exhibited.

Ramsbotham, Practical Observations, part 1,—Case I. (p. 94); II. (p. 106); III. (p. 108); IV. (p. 109); V. (p. 122); VI. (p. 125); VII. (p. 129); VIII. (p. 131); IX. (p. 179). All instances of adherent placentæ; in Case II. the membranes were likewise singularly adherent.

Brachet, Journal Génér. de Médecine, tom. cii.—Case X. (p. 13), extensive chronic inflammatory induration; XI. (p. 15), the half of the placenta in a state of acute and inflammatory congestion; XII. (p. 20), chronic inflammation; XIII. (p. 22), two abscesses on the fœtal side of the placenta; XIV. (p. 23), a very large abscess on the fœtal side of the organ, with surrounding hepatization; XV. (p. 25), effusion of coagulable lymph and pus at many points throughout the placental parenchyma; XVI. (p. 33), uterine surface of the placenta adherent.

Dance, Repertoire Générale d'Anatomie, &c. tom. iii.—Case XVII. (p. 71), an effusion of coagulable lymph and purulent matter between the uterus and placenta, and over the external surface of the decidua vera. Archives Générales de Médecine, tom. xxi.—Case XVIII. (p. 222), an effusion of pus and coagulable lymph between the placental chorion and amnion.

Stratford, London Medical and Surgical Journal, 1829, vol. ii—Case XIX. (p. 89), inflammation of placenta and membranes of the ovum, with effusion of coagulable lymph.

D'Outrepont, Gemein. Deutsch. Zeitschr. für Geburt. Bd. v. Hft. 4.—Case XX. (p. 556), acute inflammatory hepatization.

Among the *predisposing*, if not among the exciting, causes of placental inflammation may, I believe, be justly enumerated the existence of the same state of disease in a previous pregnancy. It is well known that morbid adherence of the placenta to the uterus is occasionally met with in several successive pregnancies in the same in.. Dr. Hamilton states in his Practical Observations,[1] that he nown several mothers of large families, who had in the course ir childbearing life, incurred, three or four or even five times, danger from this circumstance. Mauriceau mentions two , in one[2] of which abortion had occurred either four or five , and in other,[3] five times in succession, between the sixth and th months of pregnancy, and without any evident exciting . The infants appeared to have died some days before their lsion, and the placentæ of all of them are stated to have been ious, or affected with chronic inflammatory induration, gene- throughout almost their whole extent. In one of M. Brachet's nts (case XIV.), abscesses were found in the placentæ of two cutive pregnancies, and this case is the more remarkable, that lacental inflammation in the first instance was evidently the quence of a fall, while, as I have stated above, no exciting for the disease could be traced, to account for the disease in ubsequent pregnancy. In connection with these facts, I may at the same time mention that other forms of placental disease es inflammation show the same tendency to recurrence in dif- t pregnancies in the same individual. The tendency to the re- ion of abortion in women who have once suffered from it, is a which the practical accoucheur has constantly occasion to ob- , and numerous striking examples of the great frequency of its ition in the same woman have been put upon record.[4] In many ese cases of recurrent abortion, the affection appears to be con- d either with placental congestion or inflammation. Dr. h,[5] in the second volume of his Chirurgical Works, has recorded istance where the placenta was found ossified to a greater s extent in the same patient three several times; and Bonus[6] ved a case of a similar triple recurrence of the hydatidgenous ition of the placenta or chorion.

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[1] Gesammelt. Schrift. ii. s. 359.

[2] Gemein. Deutsche. Zeit. für Geburt. Bd. v. Hft. 4, p. 556.

[3] Kleinert's Allgemein. Repert. loc. cit. s. 82.

would be interesting, and in some respects practically import- to ascertain the duration of the different stages of placentitis, other words, the variety of morbid lesion which the organ it be most apt to present to us, according to the distance of time may, in any given case, have elapsed between the application e exciting cause of the disease, and the ultimate expulsion of rgan from the uterus. But the twenty cases which I have

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actical Observations on various subjects relating to Midwifery, pp. 286–87.

servations sur la Grossesse et l'Accouchement, Obs. 283, p. 235.

d. Obs. 141, p. 198.

Plouquet's Literatura Medica, tom. i. p. 8, a number of references may be found to rks of authors who have mentioned examples of the frequent recurrence of abortion.

ses and Practical Remarks in Surgery, vol. ii. p. 250.

ad. Boivin, Sur la Mole Vesiculaire, p. 24.

above noted afford very little satisfactory information relative to this point. I find that in nine only out of the twenty cases are any data mentioned, by which the time between the application of the probable exciting cause of the placentitis and the time of delivery can be ascertained with sufficient accuracy. In one of these cases (XI.), where the disease appeared to be of ten days' duration, the half of the placenta was affected with the first stage of inflammation; in a second (XIX.), in which the symptoms had lasted for thirteen days, there was an effusion of coagulable lymph and purulent matter between the placental chorion and decidua; in a third case (VI.), of three weeks' duration, there were adhesions between the uterus and placenta, that were capable of being easily separated; in a fourth case (V.), the adhesions between the uterus and placenta were rather strong, although the disease was only apparently of a month's duration; in a fifth case (XIII.), in which two abscesses were found in the placenta, the first symptoms of the disease had occurred after a fall about seven weeks previously; in two other cases (I., XVI.), there were chronic adhesions with the uterine surface, and in one of these (XVI.), the adhesions are described as very strong, the affection having lasted for several months; and in the two remaining instances (X., XII.), where chronic induration of the placenta was found, the placentitis seems to have continued in one case (X.) for four and a half months, and in the other (XII.) for two months.

Symptoms of Placentitis.—The symptoms accompanying placentitis must necessarily be considerably varied, by its more or less acute or chronic character, by the extent and seat of the inflammatory action, and particularly by its happening to be complicated or not with inflammation of the contiguous portion of the parietes of the uterus. When we consider the frequent chronic nature of placental inflammation, its apparent occasional confinement strictly to the substance of the placenta, and the fact that though nerves have been found in the umbilical cord and placenta itself, none have hitherto been distinctly shown to pass between it and the uterus, or, in other words, between the placenta and the mother, we may be disposed rather to wonder at placentitis giving rise so generally to symptoms more or less marked in the maternal system, than that the disease should sometimes be latent in its character.

The symptom which seems most constantly to accompany placental inflammation, is pain or pains fixed to the uterine or lumbar regions, or to some part of these regions, and differing in different cases in intensity, character, and duration.

Pain in the uterine and lumbar regions has been observed in almost all of those cases of placentitis, the history of which has been traced with any degree of care. But this symptom, it must be confessed, may present itself also during pregnancy, as an effect of other pathological states of the uterus or its contents than placental inflammation; and, again, it appears to have been wanting in some instances in which this disease actually existed. When the full histories, however, of individual cases of placentitis shall come to be more patiently and accurately investigated, such exceptional cases will probably diminish greatly in number. In the meantime, I may only observe, that in estimating the value of this symptom as a pathognomonic mark of placental inflammation, we must not reject it as an uncertain one (as many British accoucheurs seem inclined to do), from the fact of its occasionally occurring, in a well-marked degree, during a greater or less part of utero-gestation, in cases where the placenta is ascertained at the time of delivery not to be morbidly adherent; for we must recollect that this adhesion of the placenta to the uterus is only one of the various morbid effects or lesions to which placentitis gives rise; and in many such cases as those to which I allude, if the placenta itself be carefully examined after its expulsion, one or more of these lesions will generally be detected in its substance, or in its membranes.

When after the action of any of the exciting causes of Placentitis that I have previously enumerated, a local lumbar or uterine pain supervenes, we may, as M. Brachet has stated, in general presume with considerable confidence upon the presence of placental inflammation; and here, it appears to me, we should in some cases be able to make a useful practical application of uterine auscultation in the diagnosis of this disease; for if we find the seat of pain to correspond with the seat of the placental soufflet, the certainty of the connection of that pain with placental inflammation will necessarily be greatly increased. I make this remark on the supposition that Monod, Kennedy, and Paul Dubois are correct in referring the sound in question solely, or in its greatest degree of intensity at least, to the seat of the attachment of the placenta to the uterus, if not to the utero-placental vessels themselves.

In endeavoring to ascertain the exact seat of the local pain in any given case, more caution is required than seems to be generally believed. On this subject I cannot, I believe, do better than copy the just remarks and judicious injunctions given by Dr. Ramsbotham.[1] "We frequently meet," he observes, "with great vagueness in the description of pain, and particularly in the description of the situation of that pain, and if anything like precision be desirable, the patient should be requested to lay her hand on the part. A woman will tell you she has got a pain at her heart, and if you apply this test of the situation of the pain, she probably applies the hand to the epigastric region, or to any part but that over the heart. If she complain of a pain in the side, she probably applies the hand to the side of the belly; but even this test will not be sufficient to enable a professional man to discriminate between a pain in the parietes of the belly, and one situated in the uterus or in the parts underneath. If he wish to arrive at any degree of accuracy in this respect, he must examine the part with his own hand. The apparent indelicacy of this act must give way to the patient's welfare."

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[1] Practical Observations, p. 77.

On consulting the histories of the twenty cases of placentitis that I have previously enumerated, I find that pain in the region of the uterus or loins was complained of in seventeen; in one of the remaining three cases (XIV.), the sensation amounted only to a feeling of dragging, *tiraillements*, in the lumbar region; in another (IV.), the patient is merely described as having been out of health for two months previous to delivery; and in the third (XVIII.), it is distinctly mentioned that no local uterine pain was ever complained of. Of the seventeen cases in which uterine or lumbar pain was present, in two (V., VII.), the particular seat of the pain is not specified; in two (XI., XIII.), it is described as existing both in the uterus and loins; in five (X., XII., XV., XVI., XVII.), it seems to have been felt only or principally in the lumbar region, and in the last (XVII.) of these five cases it extended down from this region along the thighs. In another case (XIX.), the pain in the loins was accompanied by a strong bearing-down sensation in the uterine region; and in seven cases (I., II., III., VI., VIII., IX., and X.), it appears to have been entirely confined to some part of the uterine tumor. Out of these seven cases in which the pain was seated in some part of the uterus alone, in three (VIII., IX., XX.), it is mentioned as having been seated towards one side of that organ; in one (I.), as felt between the umbilicus and pubis; and in three (II., III., VI.), it was felt in the region of the umbilicus or the fore part of the uterine tumor.

The character of the pain is described in three cases (VI., IX., XX.), as constant; in one (I.), as almost constant; in another (VII.), as at first remittent, and afterwards constant; and in six as remittent (I., V., X., XII., XIII., XV.) Of the six cases in which it was remittent, in one (XIII.), it was increased by leaving the horizontal posture; in two (I., V.), it was increased during the night, and in one of these two (I.), the part which was the seat of the pain (viz., the lower and anterior part of the uterine tumor) was tender to the touch, but the pain itself was not rendered greater by motion. In one (III.) of the instances in which the pain was confined to the fore part of the uterus, the patient appears to have found relief in lying upon her belly and face in bed. In most of the above cases the particular nature of the pain is not mentioned, but in others it is variously described as dull, heavy, burning, obscure, &c.

In the greater number of the cases, the pain supervened within a few hours, or at most within two or three days, after the application of one of the exciting causes already mentioned, and generally continued more or less from that period to the end of utero-gestation. In one case (X.) the pain was relieved, and in two others (XII., XVI.), removed by the detraction of blood.

In one of the cases (XVIII.) recorded by M. Dance, already particularly alluded to as not accompanied by uterine pain, there was most obstinate vomiting for three months, defying all remedies, and at last producing death, apparently by inanition, between the third and fourth month of pregnancy; and in the two instances previously referred to as met with by M. Breschet,[1] in which anatomico-pathological appearances were found similar to those in this case of M. Dance's, a similar state also of frequent, obstinate, and very violent vomiting accompanied the disease. In M. Dance's case, although the uterine surface of the placenta was extensively inflamed, yet the fœtus did not appear to have suffered, and it seems probable, therefore, that the placental inflammation only supervened towards the last days of life, at the same time, perhaps, that the slight attendant febrile symptoms first appeared. In two of the acute cases of placentitis (XVII., XIX.) mentioned by Stratford and Dance, shivering with febrile symptoms supervened in the course of the disease; and in one of the instances (XII.) given by Brachet, febrile symptoms came on with lumbar pains, on the night after the patient was exposed to the injury which apparently acted as the exciting cause of the placental inflammation.

In several instances of suppurative inflammation of the placenta alluded to by Cruveilhier, he appears to have observed as a very uniform symptom, a kind of continued hectic febrile movement, with an exacerbation in the evening, preceded or not by shivering. Dr. Burns[2] describes a species of fever which he has observed to affect women about the middle period of pregnancy, and that appears to be not unlike in its principal characters to the disease described by Cruveilhier; he states, that this affection makes its attacks suddenly like a regular paroxysm of ague; that it soon puts on rather an appearance of hectic; and on the whole, he conceives, it bears a great analogy to the infantile remittent fever. This disease, Dr. Burns adds, is very obstinate, and often ends in abortion. Has not the placenta in such cases been the seat of inflammatory action?

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[1] Etudes, &c. de l'Œuf, p. 124.

[2] Principles of Midwifery, 1832, p. 224.

[illegible] mentions also slight fever and great heat, as [illegible] of placental inflammation, and when this inflam- [illegible] terminate in suppuration, he describes a drag- [illegible] and shivering as supervening, with dull heavy pain [illegible] and thighs, that are more especially felt on standing [illegible] The same author states, that in placentitis, the urine [illegible] passed in drops, and with a sharp pain, and that when [illegible] on to induration or hepatization, the dull local [illegible] pains are occasionally accompanied by a white, irritating [illegible] discharge from the vagina. In Mr. Stratford's case [illegible] discharge of blood from the vagina appears to have pre [illegible] a short time the expulsion of the diseased ovum; and [illegible] mentions in the details of one of his cases of recurren [illegible] from indurated placenta, which I have formerly allude [illegible] a small quantity of blood was usually voided per vaginam [illegible] fifteen days previously to the labor-pains coming on. In [illegible]'s first case (XVII.), the aortic pulsations were unusuall [illegible] a symptom that may perhaps be pointed out as affording [illegible] instances in which it occurs, an indication of increased o [illegible] action in the extremities of the vessels more immedi- [illegible] arising from that artery.

[illegible] considering the different symptoms of Placentitis which I have [illegible] occasion to mention in the preceding pages, it is difficult, and, [illegible] in the present stage of our knowledge, altogether impossible, [illegible] justly what degree and variety of these symptoms ought [illegible] attributed to the placental inflammation itself, and how much [illegible] character and combinations are liable to be varied by that [illegible] of the adjoining internal surface and parietes of the [illegible], by which the placentitis is often accompanied and compli- [illegible]. In order to arrive at anything approaching to accurate [illegible] upon this point, a much greater number of carefully [illegible] facts than at present exist upon the records of medicine, [illegible] be required. But I may here observe, that, out of the twenty [illegible] of inflammation of the placenta previously enumerated, in two [illegible] instances (XIII., XIX.), in which the disease appears to have [illegible] principally confined to the fœtal surface of that organ, the [illegible] characteristic pains were well marked, and even severe, while in [illegible] only case (XVIII.) in which this local pain was entirely absent, [illegible] inflammatory action was seated in the uterine surface of the placenta and membranes, and the tissue of the uterus itself was, on post-mortem inspection, found to be softened. The local pain, also, seems almost always to cease after the process of labor and the

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[1] Ryan's Medical and Surgical Journal, vol. vi. p. 573.

[2] Observations sur la Grossesse, Obs. 285, p. 233.

xpulsion of the diseased placenta. In one case (XVI.), however, he region of the pubis continued tender for some time after delivery.

Morbid Adherence between the placenta and uterus, the result of dhesive inflammation between their contiguous surfaces, besides eing accompanied by more or fewer of the above-mentioned symptoms during pregnancy, is almost uniformly marked, at the time of lelivery, by a more or less profuse and sudden flow or gush of blood ttending upon each recurrence of uterine pain or contraction after the birth of the child, and even sometimes also during the act of its expulsion. The exceptions to this general rule, more especially to the occurrence of hemorrhage with each after-pain, so long as the placenta is allowed to remain adherent, are very rare. In instances, however, such as those met with by Wrisberg,[1] Desormeaux,[2] and Ramsbotham,[3] of apparently universal adhesion of the placenta, no hemorrhage can be expected, and Dr. Hamilton[4] has recorded an example of another form of morbid adhesion, where no hemorrhage took place after delivery to indicate the diseased state of the afterbirth; in this instance the central portion of the placenta was healthy, but around its whole border or circumference the organ, for the breadth of an inch, was morbidly and firmly adherent to the uterus.

Danger of Placentitis to the Mother.—The morbid inflammatory adhesion of the placenta to the uterus, just now alluded to, is not unfrequently attended with great danger to the mother at the time of delivery, or subsequently to that event, from the extreme hemorrhage to which it sometimes gives rise, from its leading under unskilful management to inversion of the uterus, or afterwards from its retention producing, as it so frequently does, a very fatal form of irritative fever. As illustrative of its frequency and, at the same time, of the danger liable to result to the mother from this consequence of placental inflammation, I may remark, that, from the annual reports of the Royal Maternity Charity of London, in the years 1828–29–30 and 31, published by Dr. Francis Ramsbotham, in the 3d, 5th, 7th, and 9th volumes of the Medical Gazette, it appears that during the above period, 8967 women were delivered in the district of the Charity which is under Dr. Ramsbotham's more immediate care. Out of these 8967 cases, there were 67, or nearly one in every 134, in which the placenta required the introduction of the hand for its removal, either on account of its being morbidly adherent to the uterus, or from its being retained by the atony or irregular contractions of this organ;[1] 45, out of 8967 women delivered, or nearly one out of every 199, died, and 13 out of these 45 deaths, or a little more than one-third of the whole, were directly attributable to the effects resulting from morbid adhesion of the placenta—in other words, one woman out of every 689 that were delivered perished in consequence of this single consequence or termination of placental inflammation.

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[1] Comment. Med. Physiol. Anat. et Obstetr. Argum. tom. i. p. 44.

[2] Dictionnaire de Médecine, tom. vi. p. 420.

[3] Practical Observations, p. 82; see also Collins, Pract. Treatise, p. 193, case 60,—child alive.

[4] Practical Observations, part i. p. 286.

But besides the fatality occasioned by this single termination of placental inflammation subsequently to the expulsion of the child, inflammation of this organ sometimes proves a source of danger to the mother during the progress of pregnancy, by producing in her a state of general bad health, or by giving rise to abortion or premature labor.

Thus out of twenty cases of placental inflammation, to which I have so frequently adverted, abortion occurred in one (XVII.) at the third month, in a second (XVIII.), between the third and fourth, in a third (XIX.) at the fourth, in two (XI., XVIII.), at the sixth, and premature delivery in one (XX.) at the eighth, and in another (XIII.) at the eighth and a half month of utero-gestation. In several of the remaining cases, the time of utero-gestation at which labor came on is not mentioned, but in most of them the placental inflammation was of a subacute or chronic character, and the patient seems generally to have gone on to the full period of pregnancy, and even, it is stated, in two cases (X., XIV.), some days beyond that period. From the former cases, however, and from all that I have observed, I am convinced that placental inflammation will be found to be a much more frequent cause of abortion and of ill health to the mother during pregnancy, than seems at present to be at all suspected. It is known as a general observation among obstetrical practitioners that, in cases of abortion, the placenta is not unfrequently retained in the uterus for days or even for weeks after the fœtus is expelled. Does this retention depend in some of these instances upon inflammation having taken place in the placenta, or in the contiguous surface of the uterus, and having produced a morbid degree of adhesion between these two organs?

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[1] It does not appear from Dr. Ramsbotham's tables, in how many of these 67 cases the placenta was actually retained by morbid adhesions with the uterus, and in what proportion other causes acted in producing that result. I have not been able to find any sufficiently extensive series of obstetrical observations, by which to judge of the average number of cases in which the placenta becomes morbidly adherent to the uterus, but I am inclined to think that the ratio given in Dr. Ramsbotham's tables is considerably higher than usual, owing probably to the patients applying to the Dispensary, belonging to a rank of life in which they are exposed to many of the exciting causes of placental inflammation. Adhesion of the placenta to the uterus being only one of the effects of placentitis, and commonly of placentitis in a chronic form, it would be impossible to judge from the frequency of utero-placental adhesion alone, of the frequency of placental inflammation in general. See also Collins, Pract. Treat. p. 181. Ten cases required introduction of the hand from adherent placenta; and on page 127 he states, that in 13 cases of hemorrhage, the placenta, on the introduction of the hand, was found firmly adherent. Four of the women died, p. 137. See also his remarks, p. 140.

Effects of Placentitis on the Fœtus.—Placental inflammation occasionally proves fatal to the fœtus in utero. Professor Wilde, in his brief enumeration of the symptoms of this disease, mentions as one, that the motions of the child gradually become weaker, and at last cease altogether—a circumstance observed in two of M. Brachet's cases (X., XIV.), and in those previously quoted from Mauriceau.[1] Out of the eight cases of placental inflammation given by M. Brachet, in one (XVI.) the state of the infant is not adverted to; in three the children were dead born, viz., in the two cases (X., XIV.) to which I have just alluded; and in a third instance of twins,[2] one child was dead and beginning to putrefy, and the whole substance of the placenta corresponding to it was in the second stage of inflammation. The second child and placenta were healthy. Of the other four cases of placentitis mentioned by Brachet, in one (XI.) the child was lively, but died thirty hours after birth, and in all the other three (XII., XIII., XV.), the infants were feeble and excessively pale, wan, and lean; one of them died a few days after birth; the other two survived.[3]

The placenta being apparently directly subservient both to the respiration and nutrition of the fœtus, performing for it at once the functions of the lungs and of the stomach, or perhaps more properly of the mouth, during the greater part of its intra-uterine life, it is not wonderful that chronic inflammatory disease of this organ by more or less completely arresting or impeding the performance of these functions, should sometimes destroy the life of the child and in other instances produce in it that state of *phthisical marasmus*, if we may so term it, which the three infants mentioned by Brachet exhibited. That the state of these infants was really dependent, not upon any pathological condition in their economy, but only upon the morbid condition of the placenta, and the mal-performance of its functions, was proved on the one hand, by no abnormal or morbid lesion being discovered upon a careful post-mortem inspection in two of the fatal cases, and, on the other hand, by two of those infants that were born alive rapidly gaining plumpness and strength, when transferred to the care of a good nurse. The above were instances in which the inflammation of the placenta had been chronic, and the disorganization it produced consequently slow in its progress and more or less of the organ had been left in a state fit to perform its functions. Where the inflammatory action is more acute in its nature, or more general in its extent, the fœtus may be killed by it more rapidly, and before its state of nourishment is affected. This appears to have happened in Brachet's fifth case (XIII.), where at the 8½ month of utero-gestation, the child was born dead and putrid, but of large size and sufficient weight. Three-fourths of the placenta, as I have already stated, were occupied by a large abscess, which the history of the case shows to have been of comparatively recent formation.

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[1] Loc. cit. Obs. 241 and 283.

[2] Journal Générale, cii. pp. 11, 12.

[3] In three of Dr. Collins' cases of adherent placenta, the children were *dead* and *putrid*, p. 198;—in one of them, a twin case, one child and placenta were healthy, the other child was *putrid*, and its placenta adherent, p. 195.

There is one other series of effects produced by placental inflammation, or at least, by inflammation of the membranes covering the external surface of the placenta, upon the fœtus, which I regret that the already too extended limits of this essay will allow me merely to allude to—I mean the morbid connection of the internal surface of this organ, by adhesive inflammation, with some part of the body of the fœtus, and the production, in consequence of that connection, of different forms of malformation by displacement and arrestment of development, in more or fewer of the fœtal viscera and members.

Paul Portal, in his work on the Practice of Midwifery,[1] has given the first, and among the older authors the only, distinct instance which I have been able to find of this variety of monstrosity;[2] but since the beginning of the present century, eight cases at least have been published by different authors, of malformed fœtuses born with the fœtal surface of the placenta more or less intimately connected by morbid adhesions with their head and face. The first of these cases was published by Dr. Poole[3] of London, the second by Mr. Cam[4] of Bath, the third by Necati,[5] a fourth, first recorded by chateau,[1] and afterwards minutely and ably described by Geoffroy Hilaire,[2] a fifth by Breschet,[3] three by Charles Rudolphi,[4] and last and eighth by Lauray.[5] Besides these nine published cases, ay mention that I have myself had an opportunity of examining e additional specimens of this curious species of monstrosity, in the Museum of Guy's Hospital, a second in that of the idon University, and a third in the anatomical collection belong- to the London College of Physicians.

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[1] La Pratique des Accouchemens, 1685, pp. 192–199. Two figures of the child and placenta are given.

[2] Dr. Thomson has pointed out to me an instance of monstrosity figured in the Ephemer. Nat. Curios. for 1673, p. 166, by Lachmund, as in all probability an example of the morbid attachment of the placenta to the head of the child, but the description given of the case is very indistinct. In the works of the older writers on monstrosities, as in those of Lycosthenes, Licetus, Aldrovandi, &c., I have not found any instance of malformation, in which it is distinctly stated that the placenta was connected by morbid adhesions to the fœtus in the manner mentioned in the text, but it is probable that some of the instances of torn membranes, occasionally described by these authors as attached to the heads of malformed infants, and fancifully supposed by them to resemble the ears of the lower animals, &c., were the remains of ruptured placental adhesions. An instance of monstrosity quoted from Fincelius by the younger Schenkius (see his Monstrorum Historia Memorabilis, 1609, pp. 8–9), was perhaps an example of the attachment of the placenta to the head of the fœtus. "Habuit ille in vertice *massam carnis, imaginem praebens testudinus rufae;* in cervice caudam carnosam habuit instar muris sed abescentem, qualis erat cutis sine pilis, capite erat Thersitico, oculis extantibus."

[3] London Medical and Physical Journal, vol. iii. 1800, pp. 397 and 497.

[4] Ibid. vol. vii. 1802, p. 385.

[5] Specim. Anat. Pathol. de Labii Leporini Congeniti naturâ et origine. Amsterdam, 1822.

Vithin these few years several other cases have been put upon ord of malformed fœtuses born with the placenta adhering, either ctly or by means of a connecting membrane, to the parietes of unclosed abdomen of the fœtus, or to its extroverted abdominal era. Dr. Poole, in[6] the short paper already referred to, men- is having seen a case of this kind; Mr. Hill[7] has recorded what ears to be a second similar instance; in 1817, a third was wn to the Society of Medicine by Chaussier;[8] and three addi- ıal cases have been more lately described by Dr. Knox,[9] Profes- Henot,[10] and Dr. Montgomery.[11] Of this variety of malformation l morbid placental adhesion, I have seen five instances—two of m in the London University Museum, one in the Museum of . Blundell, another in that of Mr. Langstaff, and the fifth in the lection of M. Dupont at Paris.[12]

[am not aware of any cases of attachment of the fœtal membranes the head among monstrosities in the lower animals, but Pro- sor Gurlt of Berlin,[13] has described several cases in which these mbranes were attached to the abdomen of different animals, and has delineated three instances of it in his plates, in the calf, lamb, d kid. In all these cases the spines and bodies of the animals re much contorted, as is generally the case also with this kind of ılformation in the human subject. In four out of the five instances ıich I have seen in the human fœtus, the trunk was more or less completely doubled backwards or laterally upon itself, and in three of the cases, one of the sides of the body was less perfectly developed than the other.[1]

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[1] Journal Complementaire des Sc. Médicales, tom. viii. 1820, pp. 377–78.

[2] Anatomie Philosophique, tom. ii. Monstruosités, 1822, pp. 151–221.

[3] Repertoire d'Anatomie, &c. tom. ii. 1826, p. 288; London Med. Chirurg. Trans. vol. ix.

[4] Monstrorum Trium cum Secundinis Coalitorum, Berlin, 1829.

[5] Bibliotheque Médicale, 1829, tom. ii. p. 288.

[6] Lond. Med. and Phys. Journ. vol. iii. p. 399.

[7] Ibid. vol. xxiv., 1810, p. 61.

[8] Bulletins de la Faculté, &c. tom. v. 1817, p. 310.

[9] Edinburgh Journal of Medical Science, vol. i. 1826, p. 343.

[10] Archives Générales de Médecine, tom. xxii. 1830, p. 313.

[11] Dublin Medical Transactions, New Series, vol. i. 1833, p. 375.

[12] See also cases by Mr. Skinner in his Edit. of Dr. Gooch's Compendium of Midwifery, 91; Dietrich, Geburtshülfliche Demonstrationen, Hft. iv. Tab. xiv. et xv. Weimar, 1826; oriep, De Funiculi Umbilicalis Defectû, Berlin, 1832, p. 25.

[13] Lehrbuch der Pathologischen Anatomie der Haus-Säugethiere, Bd. ii. s. 99–143. Tab. . 2 and 3. and Tab. vii. 1.

If this were a fit opportunity, it would be easy, I conceive, to point out some very striking points of analogy in external figure and internal anatomical structure between the different individuals belonging to these two classes of malformations, and I am further persuaded that, by a study of these analogies, many of the cases of monstrosity, particularly of strongly marked malformations of the head and face, of defective conjunctions in the median line of the body, and of extroversions of the encephalic, thoracic, and abdominal viscera, that are to be found upon the records of medicine, might be shown to be equably referable, in the first instance, to the existence of morbid adhesions of the embryo to the placenta or membranes of the ovum, as their more immediate determining cause, although such adhesions were not observed at the time of birth, from the connecting and often tender bands or membranes of which they generally consist, having been in some instances ruptured in the act of parturition, and in others destroyed during the course of intra-uterine life, the solutions of continuity which they had left upon the body of the fœtus having become before birth more or less completely cicatrized and obliterated.

TREATMENT OF PLACENTAL CONGESTION AND INFLAMMATION.

I shall not lengthen out the present part of my paper, by obtruding upon the Society any very detailed remarks upon the prevention and treatment of placental congestion and inflammation; for on these subjects I have indeed little novel to offer, and few or no modes of practice to inculcate beyond those that are generally acted upon in the daily attempts that are made to alleviate and remove the several individual symptoms and combinations of symptoms ccurring in the state of pregnancy, that I have occasion to particuarize in the course of the preceding observations. The more accuate pathological investigation of the nature of any disease has very eldom led in a direct manner to the development of any new mehods of practice in its treatment, and hence, amid the many and trange revolutions to which medical theory has been subjected, it singular to remark, in the retrospect, how comparatively stationry a number of the leading principles of medical treatment in difrent affections have at all times remained. But though general and natomical pathology has not, in the great advances which it has now ade, more especially in modern times, led, with some very few exeptions, to the suggestion of any actually new remedial measures, et, assisted by clinical observation, it has confessedly, imperfect as still is, proved already of incalculable advantage to practical mediine; it has enabled us to generalize more accurately than before, ur knowledge and practical application of the ample store of theraeutic agents that we already possess, by, in the first place, affording s data to generalize upon the identity of morbid actions and morbid fects in different organs of the body; it has enabled us in many of e complicated diseased conditions of the human economy to deect the true causal relations in which various forms of morbid states nd symptoms, coexisting in different and distant parts of this conomy, stand to one another—to distinguish affections that are rimary, from those that are merely their secondary and sympathetic ffects—to fix upon the individual organ that forms the primitive eat of diseased action, and which, consequently, ought to constitute e main point and object against which all our remedial measures hould be, directly or indirectly, aimed; and besides, modern pahology, in a few remarkable instances, has afforded us important lues, that have served as guides by which we have traced back conrmed morbid states and lesions from their more advanced and more ncurable stages to their earliest commencement, and further, it has n some of these instances, shown us that the morbid actions of hich they consist at these, their first periods of development, are uch as can often be detected by a cautious and careful use of the mproved means of diagnosis which it has placed in our power, and t the same time be made the subject of a rational and successful medical treatment.

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[1] I had some time ago presented to me a delineation and short description of an anencephalous human fœtus, to which both of these last remarks strongly apply. I regret that the imperfect history which I have received of this case, does not enable me to state positively if the sides of the large opening in the anterior part of the body, through which almost all the abdominal and thoracic viscera protrude, stretched onwards in a membranous form to the placenta or not; but this interesting monstrosity resembles so exactly some of the other instances which I have seen in which this membranous placental connection existed, that I have little doubt such was the nature also of the present case. The trunk of this fœtus is doubled back to such a degree that the occiput nearly touches the lower part of the dorsal spine, to which it is connected by a short web of integument. The spinal column, at the same time that it is thus made to project forwards, is curved also towards the left side so as nearly to form a semicircle. The ribs on this side are merely rudimentary, consisting only of a few cartilaginous stumps. The ribs, again, on the right side, are long, and form a broad flat thoracic surface. This fœtus was the product of a premature labor, as the monstrosities of this class have almost always been.

General bloodletting, repeated or not, according to circumstances, along with more or fewer other antiphlogistic measures, has long been successfully employed by medical men, to subdue the various symptoms, direct and sympathetic, local and constitutional, that I have endeavored in the preceding pages to trace to congestion and inflammation of the placenta as their immediate or proximate cause;

and the inquiry into which I have entered is, I trust, calculated to show in a clear light, the value and rational character of this practice, upon the general principle of applying to the diseases of this organ the same remedial measures that have been found, by observation and experience, to be best fitted for combating congestive and inflammatory morbid actions in other parts of the body. Upon this general ground also, it would seem not unreasonable to anticipate, that, in the preventive treatment of some of the more common forms of abortion, and of the other local effects and symptoms of placental congestion and inflammation, as well as in the treatment of those various but more remotely situated sympathetic and constitutional affections to which these two morbid states of the placenta, but particularly its state of inflammation, so frequently appears to give rise—an active employment of leeching or other local depletory and sedative measures may be attended with highly beneficial results; and be advantageously followed up, if the nature of the case, or the obstinacy of the symptoms require it, by blisters and counter-irritation in the neighborhood of the region of the uterus, as, indeed, some American practitioners have already recommended to us in the management of cases of threatened abortion.[1] The kind of inquiry into which I have been led may also, I hope, prove ultimately useful, in showing that a large part of the great sacrifice of human life which is constantly occurring from the death of the fœtus, and from the act of abortion to which that death gives rise, primarily depends upon morbid actions originating and seated in the placenta, and that in many instances these actions are in themselves of such a pathological nature as to be generally considered amenable to medical treatment. And further, if the view which I have taken of the pathological nature of the adherent placenta be correct, namely, that it is the result of a state of inflammatory action in that organ, generally of a chronic character, or, as I ought perhaps more properly to say, of a state of adhesive inflammation in the contiguous surfaces of the placenta and of the uterus, it seems reasonable to expect that we may, by watching more diligently for the first symptoms and advances of this inflammation, be then able to arrest, by the employment of such measures as those above referred to, the morbid action in its earlier stages, and thus avert, or at least diminish, what, as I have already endeavored to show, ultimately proves one of the principal sources of danger and fatality to the parturient female, and constitutes altogether a case, than which (as Dr. Ramsbotham has justly remarked) "there is scarcely one in the whole circle of practical midwifery more pregnant with immediate and impending misief."[1] The above remarks on the early detection, and on the early d active treatment, of placental inflammation, are only, if possible, re strongly applicable to the management of the cases referred to a previous part of the paper, in which placental inflammation ws a tendency to recur in different pregnancies in the same man. But, indeed, under all circumstances, and in all instances, ile employing in the treatment of placental inflammation and gestion, more or fewer of the various general therapeutic means ve alluded to, and such other remedial measures, medicinal tetic, and regiminal, as the peculiarities of individual cases are, in practice of our profession, constantly found to require, we ought r to be guided and stimulated by the consideration, that on the ivity and success of our exertions, are dependent not only the sent well-being and future safety of the mother, but the health , and, it may be, the life of her infant.

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[1] See Jackson, American Journal of the Med. Sciences, No. iv. p. 299; and Cenas, in Ibid. No. x. p. 547.

ATURE OF HYDATIGINOUS DEGENERATION OF THE OVUM.[2]

(From Edinburgh Monthly Journal of Medical Science, May, 1847, p. 868.)

R. Simpson showed a specimen of hydatid ovum, where the em- o was extremely small, not larger than a pea; and the placenta arged into a mass of hydatids, fibrin, and decolorized blood, ighing thirteen ounces at the time of its expulsion. The patient koned herself gone beyond the full time of utero-gestation.

r. S. stated it as his opinion that the hydatiginous ovum or hy- id placenta was a morbid state of a compound character.

The enlarged hydatids were, no doubt, merely the enlarged villi the chorion. So far, the affection was a kind of *malformation* m arrest of development, the villi of the chorion remaining of ir early embryonic type, and continuing to increase and grow, der this retained type of structure. But—

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[1] Out of the nine cases of adherent placenta extracted in a preceding page from Dr. Rams- iam's work, in two (II. IV.) the mothers died, one (II.) in eight hours, and the other (IV.) hree hours after the birth of the child. A third patient (I.) out of the nine, died nine s after delivery, but apparently not at all in consequence of the previous adhesion of placenta, or its effects. If we keep this last case entirely out of the calculation, it ap- rs that out of twenty-three cases in all, of adherent placenta, described by this author act. Observ. pp. 98–141), in nine of the instances the women died, seven of them from orrhage and exhaustion, at intervals varying from one hour and a half to eight hours r delivery; one from inversion of the uterus, apparently in the course of two or three rs; and the remaining ninth patient survived till the fifth day, when she sank under nptoms of reaction and fever.

[2] Extracted from Proceedings of Edinburgh Obstetric Society, March 10, 1847.

2. The cells of the villi, constituting the hydatid placenta, appeared at the same time to be generally broken up in their internal tissues, and distended by a morbid accumulation of fluid. So that we have *disease* added to malformation; and dropsy coexisting with the hypertrophied state of these structures. It would be difficult to decide whether the dropsy stood in the relation of cause or effect to the malformation; or whether both were not the effects of some common cause.

PLACENTAL PHTHISIS OR APNŒA,

AS AN INTRA-UTERINE CAUSE OF DEATH AMONG PREMATURE CHILDREN—ITS VARIETIES AND TREATMENT.[1]

(From Edinburgh Monthly Journal of Medical Science, Feb. 1845, p. 119.)

Formerly physicians used the word phthisis as a generic term to signify consumption and organic destruction of various kinds; and the viscus which was the immediate seat of decay and death was added to express the nosological seat and species of the malady. Hence arose the terms phthisis pulmonalis, phthisis mesenterica, phthisis hepatica, &c. The infant in utero is liable to perish—1, in consequence of various diseased states of its own body, as peritonitis, dropsy, &c.; and, 2, in consequence of different morbid conditions of the placenta. Under the designation of placental phthisis, I shall attempt to point out to you, very briefly, the principal diseased states of the placenta that are apt to leadto the intra-uterine death of the child, and at the same time I will try to state their proper principles of treatment.

Let me premise two observations on the subject. First, That in consonance with a very curious and interesting general law in intra-uterine pathology, some of the morbid conditions of the placenta and fœtus are liable to recur in successive pregnancies in the same woman; and, secondly, In actual practice you will find few cases accompanied with greater individual distress and greater family unhappiness, than those in which there is borne by the same mother, two, three, or more premature children, dying regularly in utero, as the seventh or eighth month of utero-gestation recurs.

Occasionally this distressing recurrence of fœtal deaths is the result of disease or malformation in the fœtus itself, and is in a great measure irremediable. Much more frequently, however, such a

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[1] A clinical Lecture extended from the original, especially in that portion which relates to the morbid states of the placenta. See first part of the same lecture in vol. i. p. 557.

:ession of deaths among premature children is the consequence lacental disease or placental phthisis; and is amenable to proper .ciples of treatment.

he principal pathological states of the placenta that are liable to luce the successive deaths of a series of premature children in same mother are the following:

Congestion and Extravasations of Blood into the Structure of the :enta.—In practice it will, I believe, be found that placental ıorrhages or "apoplexies,"—as they have been sometimes termed ›rming in the substance, or on the surfaces of the placenta, ses or collections of extravasated blood in various states of nge, are lesions much more frequently connected with abortions, ı with premature labors. They are found connected with the th of the fœtus more often in the earlier than in the later months .tero-gestation. Sometimes, however, they form, and repeatedly he same mother, the cause of death of the infant from the seventh he ninth month—a succession of limited hemorrhagic extravasa- .s taking place, till at last the placenta is so extensively diseased destroyed as to be unable to maintain any longer the life of the d. In such instances I have usually found the placental struc- › softened and broken up, within and around the more recent extensive extravasations.

I. *Inflammation of the Placenta* and its various consequences— ›atization, Induration, Abscess, &c., constitute a second cause of a-uterine death among premature children. In this morbid ction, the inflammatory action is generally confined to a limited tion of the organ, or to a few lobules, whilst the other parts of organ are left sound and free. In some cases, however, we find ıvading the whole surface of the placenta—an observation which will see confirmed by the state of the organ in many cases such this, which I now show you, where there has been born a ›ndary fœtus, along with one at the full time—the secondary us, as it is called, being merely a fœtus which had been destroyed :arly as probably the fourth or fifth month, by the morbid altera- ı which had occurred in the structure of the placenta, or portion the placenta belonging to it. In inflammatory induration, the rbid deposit and change seems generally to stretch, as shown in › preparation of Mr. Goodsir's, from the maternal surface of the centa towards the fœtal. The decidual membrane covering the erior surface of the placenta is occasionally, as here, much thick- :d by the inflammatory deposit.

II. *Gangrene* of portions of the placenta sometimes, though rarely, produces the recurrent intra-uterine death of the child in the latter months. I have seen several examples of this variety of placental disease. One of the most remarkable was a case which I visited repeatedly some time ago in consultation with Dr. Gibson of Dundee, and where the fœtal heart, as heard by the stethoscope, became irregular and occasionally intermittent for a short time before the birth of the child—an effect which we attributed to the placental disease, as it entirely disappeared when the child assumed an extra-uterine existence. In placental gangrene, the affected parts give forth no gangrenous odor, in consequence of the external air not having reached them; but you will find the disease marked, like Laennec's circumscribed gangrene of the lung, by here and there a dark and mortified portion of placental tissue being enclosed in a limited deliquescent mass of bloody and pasty matter. The placental surface at the site of the gangrenous slough is usually depressed and umbilicated in the centre. Generally, the included sloughing fœtal tuft retains its adhesion to the chorial surface of the placenta, and very exactly represents the mortified branch of pulmonary tissue often seen attached by its arterial structure to the interior of circumscribed gangrenous cavities in the lungs. The placental tissue around the circumscribed gangrenes is usually inflamed; very often, also, it is the site of hemorrhagic extravasation. Perhaps, indeed, the gangrene most frequently originates in the compression and death of placental tufts included in apoplectic extravasations, or isolated and obstructed by local inflammatory action.

IV. *General Œdema* or dropsy of the structure of the placenta occasionally destroys the infant in the latter months of utero-gestation, and is marked by a white, swollen, and serous condition of the organ, without any other appreciable change in its tissues. Sometimes it coexists, but by no means always, with dropsy and anasarca in the fœtus, and in the whole umbilical cord. I have seen it conjoined, probably as an effect, with inflammatory obstruction in the umbilical vessels, and limited dropsy of the cord. Usually it is diffused throughout the whole placenta, and by interfering with its intra-uterine functions, destroys at last, and sometimes very speedily, the life of the child. It must not be confounded with the white, blanched, and merely anæmic state of the placenta, often observable in cases where the child has died of peritonitis or other fœtal diseases, and been retained in utero for some time subsequently; and it is pathologically very different also from the morbid state which I have next to mention, and which can only be fully made out by microscopic examination, viz.:

V. *Stearoid or fatty degeneration of the Placenta.* In the last cen-

Hoboken and Haller mentioned the frequency of appearances
ty matter in the placenta. During the present century, several
ental pathologists, as Stein, D'Outrepont, Wilde, Charante,
rehm, have described cases of fatty degeneration of the pla-
; but the importance and frequency of this peculiar change
ot recognized till the fatty degeneration of other organs came
ly to be studied by the microscope; and it was first brought
the special attention of the profession in this country by Dr.
es of London, in two able papers published in the 34th and
volumes of the Medico-Chirurgical Transactions. It is a
id condition found connected with the death of the fœtus,
in the earlier and later months of utero-gestation. Its exact
logical nature, however, has not been accurately determined
all the conditions in which it is found to occur. Dr. Barnes
to regard this fatty degeneration as, most commonly, a pri-
morbid state in the placental tissues, or rather in the mole-
walls of the fœtal tufts, &c., unpreceded by any other morbid
ge; and some of its varieties are, there is little doubt, of this
cter, particularly where the fatty metamorphosis is general
naccompanied by any other morbid states. But several years
nd before the appearance of Dr. Barnes' essay, my friend and
gue, Dr. Bennett, carefully examined some specimens of both
al and partial fatty degeneration of the placenta with which I
shed him; and in most of these the coexistence of coagulable
h, indurations, &c., showed that the fatty molecules were either
n out as inflammatory exudations, or were formed by inflam-
ry exudations subsequently degenerating into fat particles, and
n in other respects indicated the pre-existence of some degree
cental inflammation. Virchow and Dr. Handfield Jones have
noticed this mode of its production, and Dr. Priestley, who
f late very carefully examined this question in numbers of
, believes that the so-called fatty degeneration is perhaps most
ently a change connected with a low form of placentitis. In
er which I published in 1836, on placental inflammation and
stion, I described the chronic inflammatory induration of the
nta as not unfrequently exhibiting "a somewhat stearoid, larda-
, or fatty-looking structure;"[1] and I suggested also that the
masses like fat and cartilage seen in the placenta by Stein
t originate in effused coagula of blood; and I believe that in
cases we have localized fatty deposits in the placenta, result-
rom degeneration of the blood thrown out in placental conges-
and hemorrhage. Sometimes, further, when the child dies
disease in its own body, and is retained for some time in utero,

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[1] See page 386.

the placental structure presents an appearance of fatty degeneration, as an effect, and not as a cause of the fœtal death. In reference to this morbid change, it is necessary to recollect that the placental structure may appear yellowish-white and fatty in whole or in part, without there being any true fat-globules in it; and to distinguish it accurately, we need the assistance of the microscope.

VI. *Hypertrophy of the Placenta* sometimes leads prematurely to the death of the child in successive pregnancies. When the placenta presents this condition, the organ is greatly enlarged; the divisions between its lobules and maternal surface are very marked and very deep; and the edge of the placenta seems as it were almost to turn to a certain degree over the boundary or circumference of the fœtal surface. One of the preparations on the table is a specimen of this diseased condition, and the patient from whom it was taken had produced six or seven dead-born premature children. Mr. Goodsir has directed his attention particularly to this effect of hypertrophied placenta, and similar observations have been made by some continental accoucheurs. It is difficult to say how the hypertrophy of the placenta destroys the functions of the organ; for in the specimen before you, as in other similar cases, there is no special lesion in the body of the child itself. Sometimes, however, the child presents dropsical effusions, such as anasarca and ascites. Probably the mutual compression and impaction of the different lobules and parts of the hypertrophied placenta upon each other, are such as to diminish and destroy its action as a respiratory organ, and to impede the circulation through its vast collection of vessels, as effectually as this is produced by the obliteration of these vessels and parts by hemorrhage, inflammation, œdema, or other direct disease.

VII. *Other Morbid Conditions of the Placenta* seem sometimes to lead to a succession of dead premature children in the same mother, as extensive cartilaginous and calcareous degeneration, morbid ramollissement, atrophy, &c.; but their action is less marked and decided than those I have already enumerated. Neither these, nor any of the more truly morbid states of the placenta, are apt, as a general rule, to affect and destroy the child, unless they have invaded and obstructed a large portion of the placental structure and vessels. Indeed, slight and limited degrees of stearoid, cartilaginous, and calcareous degeneration are to be seen here and there in many placentæ at the full time—more as a sign of the early senility of this temporary organ than as a mark of disease—the child's health being, to all appearance, unaffected by them. In the essay I have referred to, I have spoken of these fatty and ossific degenerations of vessels as liable, when they occur earlier in placental life, to predispose to vascular rupture and sanguineous extravasations. Let me only farther add, that I have never seen any instance in which the so-called hydatiginous degeneration of the placenta recurred twice in the same patient and thus produced a recurrence of fœtal death; nor have I met with true strumous tubercle so extensively diffused in the placenta as to destroy the child.

The *Diagnosis* of the pathological cause of the death of the fœtus, in one or two successive pregnancies, can only be made out, with any precision, by having an opportunity of examining the body of one of the fœtuses and its placenta. In doing so, we may be enabled to observe which of the causes I have mentioned is the source of the calamity, and to direct our treatment accordingly.

Some years ago, when I was engaged in the investigation of Peritonitis in the Fœtus, I more than once asked myself the question, in what good could such an inquiry result? I felt utterly skeptical as to its being of any benefit, except as satisfying pathological curiosity. But often when we enter on a subject of pathological study, we really know not to what ultimate results it may lead, and therefore never ought to condemn or eschew any pathological investigation, because we do not immediately see any practical advantage to which it may tend. I have latterly become convinced that the study of peritonitis in the fœtus may be made of no small practical utility in the following respect:

In describing peritonitis in the fœtus, I have stated, when discussing the exciting causes of the disease, that in some cases the mother has been exposed to bodily injury, &c., and after mentioning other probable morbific circumstances, I have added, that it appeared to me highly probable, from the investigations I had then made on this point, that a great proportion of those children of syphilitic mothers that die in the latter months of pregnancy, may be shown to have perished under attacks of peritoneal inflammation. Further observations have led me to conclude that the evidences of peritonitis, in several successive children of the same mother, is a pretty certain test of one or other of the parents, especially the mother, being tainted with syphilis.

The practical deduction in the way of treatment from this observation in the way of diagnosis is evident. It is, I believe, in these cases of successive premature labors, where the child perishes of peritonitis, and in these cases only, that mercury and other antisyphilitic modes of treatment are alone useful, though these modes of treatment have been supposed to apply to all instances where there is the unfortunate habit of losing the infant in the last months of utero-gestation.

TREATMENT;—ALKALINE SALTS, ETC.;—INDUCTION OF LABOR.

For the *Treatment* of the cases in which the child dies in consequence of disease, not in its own structures, but in the economy and structure of the *Placenta*, I believe that totally different principles ought to be pursued; and in a large number of instances now, I have had the good fortune to see, in my own practice, and in that of others, these means of treatment followed by the most happy and successful results.

You may easily understand the principles of treatment on which I would advise you to proceed in these last affections, if in the first instance, you recollect that the two great functions which the placenta performs in the fœtal economy are these—1, of nutrition, and 2, of respiration; or probably we should properly say, that this organ is the medium of these two functions between the mother and the infant.

When the placenta becomes diseased it can destroy the infant—provided there is no morbid lesion in the fœtus itself—only by the imperfect manner in which one or both of these functions is performed. Such children, however, as die in connection with diseased placenta, do not appear to perish generally from want of nutrition, because in many instances we find them not more lean and atrophied than healthy children sometimes are at the time of birth; and on opening their bodies you have often abundance of deposit of adipose matter. I believe for my own part that they more frequently perish from the diseased placenta not being able to act sufficiently as a *respiratory medium* between them and the mother, especially when the placental disease and obstruction is, as often happens, extensive and acute, or subacute; and that the infant in consequence dies from the morbid condition of the placenta, in the same manner as we should die if our lungs were densely studded with tubercular deposits, or extensively destroyed by inflammatory action. They die from want of respiration rather than want of nutrition—from placental apnœa rather than from placental marasmus. And when once these infants are born, and are sustained by their own pulmonary respiration, they rapidly thrive and grow, proving that there is in reality no disease in their own bodies.

Now the question is—with such an imperfect placenta—or, in other words, with such imperfect fœtal lungs—what means can we possibly adopt in order to make this diseased placenta serve as a sufficient respiratory organ to the infant for a very few weeks longer.

For the question is in general a matter only of one, two, or more weeks—that is to say, if we could preserve the child's life during hat period from the action of the deleterious influences of which I peak, we should save the child till it was fit to take on an extra-uterine existence.

I have usually—in cases in which, from the history of the previous pregnancies, I knew the tendency to be, as most frequently it is, to ome form of Congestion and Inflammation of the Placenta—ttempted to prevent these morbid actions from going to any considerable extent by small venesections, or leechings from time to ime; particularly at those periods when the woman would have had her catamenia present, provided she were not in the family way; because it is, I believe, at these periods that she runs most danger—there being, during pregnancy, in many females a monthly molimen of blood in these parts, though there be no monthly discharge. It is indeed, at these recurring periods of danger, that rest n the supine position, and freedom from muscular exertion, is specially or alone required. But though we may moderate the hemorrhagic or inflammatory effusions in this way, we can seldom, I believe, entirely prevent them. Hence, our object is to make the diseased placenta as efficient as possible as a respiratory organ; or rather to make the respiratory change in the remaining healthy part as active and intense as possible.

To understand how this may be done, consider for a moment how the fœtus does respire or breathe. Its type of respiration resembles, as I have described it to you at other times, that of fishes. The blood of the fish is sent into the vessels of the gills in order to undergo the respiratory change which is there effected through the oxygen contained in the surrounding water. The blood of the fœtus is sent into the tufts or terminal branches of the fœtal placenta—its gills in other words—in order to be there exposed to the oxygen contained in the maternal blood, by which these tufts are washed in the cavernous structure of the placenta.

The respiration of the human fœtus is like that of a fish, then, with this difference, that the blood in the gills of the fish is arterialized by the water in which these gills are freely immersed, whilst the blood in the placental tufts of the fœtus is arterialized by the maternal blood in which these tufts are freely immersed. We can influence the vitality of the fish by the quantity of oxygen in the water applied to its gills. I believe we may do the same with the fœtus, by changing the oxygenating power of the maternal blood applied to its tufts.

Then comes the question, by what measures could we render the maternal blood as highly an oxygenating medium as possible, in order that, when it is applied to the fœtal placental tufts, it may make up, by the quality or intensity of the respiratory change which it there produces, for that loss of quantity which is a necessary consequence of a portion of these placental tufts being already destroyed by disease.

I have attempted to do this, and in a great number of cases, apparently with perfect success, by keeping the patients constantly on small doses of alkaline salts, such as chlorate of potass, nitrate of potass, bicarbonate of soda, &c., given several times a day, on an empty stomach, exactly as Dr. Stevens, some years ago, proposed to do for the restoration and arterialization of the unarterialized blood in fever patients. I have most frequently employed chlorate of potass in doses of from ten to thirty grains, taken repeatedly during the day, in solution, and upon an empty stomach. Out of every eight atoms of this salt, as many as six atoms consist of oxygen.

You are aware that the addition of alkaline salts to the blood in this way appears to promote greatly, I had almost said to impart, arterial changes and properties, and that in a way which physiologists and chemists have not yet been able satisfactorily to explain. If you cover a coagulum of newly drawn venous blood with a thin layer of water, the surface of the blood continues to retain its black color. If you add alkaline salts to the intervening layer of water, the air will very speedily act through this medium so as to render the clot of a red arterial color. In aquatic respiration, the animal in breathing does not decompose the water, it merely subtracts the oxygen present in it. The same fact, in all probability, holds good in sanguineous respiration—if we may use such a term—and hence the importance of supplying, in the cases we speak of, the maternal blood with a sufficient amount of oxygen.

Patients have repeatedly averred to me, that the use of the salts I have spoken of, has a perceptible influence on the strength of the motions of the fœtus—or, in other words, on its muscular power and vigor for the time being; but the observation is liable to so many fallacies on the part of the parent, that, probably, we should not build much upon it. But if these salts act in the manner which I suppose, on the maternal blood, the fœtus, under their use, is placed in a better and purer atmosphere, to use language applied to extra-uterine life; and in this better atmosphere is capable of living onwards for a few weeks longer than it otherwise would have done. I think it might be a matter of some chemical importance to inquire, what special salts would probably be of most use in rendering the mother's blood as highly an arterializing medium as possible, and if the use of iron in any form would increase its power in this or other respects. Further, would the use of chalybeates, or other means, ever so invigorate the child as to prevent those placental diseases—such as fatty degeneration—which may possibly be connected with want of power in the fœtal economy and circulation? Could any variety of diet or drug render the maternal blood a more nutritious medium for the child, where the placental disease tended to produce intra-uterine death by marasmus or inanition? The subject is quite open for inquiry, and one in regard to which I know not any very accurate existing data.

In all the series of cases which I have adverted to, that is to say, in cases where children of the same mother have died successively from the effects of different diseased states of the placenta, I believe that the *induction of premature labor* about the seventh or eighth month ought to be a principle of treatment prominently held in view, and frequently had recourse to. This remark especially holds good with regard to all cases and causes of recurrent placental disease; and I think that obstetric authors must add, what no one of them, so far as I know, mentions, the diseased states of the placenta to which I have alluded, as indications for the induction of premature labor, both when they have recurred several times in the same mother, and produced death of the child but a few days or weeks previous to its birth, or even in a first pregnancy, when very distinct symptoms of placental hemorrhage and inflammation have occurred after exposure to injury, and, in addition, the stethoscope shows a state of impending danger to the life of the child. Out of three cases of diseased placenta which have been under my care since the commencement of the present year (1845), in two I induced premature labor successfully, as regards both mother and child, one of the patients having previously lost six, and the other three children. I had thoughts of allowing the third to go on to the full period, but fortunately, natural premature labor came on about the eighth month, and a living child was born. The placenta was so destroyed by inflammatory induration in this last case, that I am sure it could not have served the purpose of a lung to the child for a much longer period. Nature here pointed out strongly, and effected by her own efforts, what ought to be done by art in similar instances. Allow me to add, that the necessity for the immediate induction of premature labor is sometimes shown in these cases by the supervention of lowness and depression, more rarely by the occurrence of irregularity and intermittence, in the action of the fœtal heart as heard by the stethoscope. Hence, in watching and treating these cases, auscultation should be constantly used to ascertain the first advent of this sign of danger to the life of the child.

PART VI.

PATHOLOGY OF INFANCY AND CHILDHOOD.

CASES OF DOUBLE CEPHALÆMATOMA.[1]

THEIR TREATMENT.

(From Edinburgh Monthly Journal of Medical Science, April, 1848, p. 764.)

Dr. Simpson showed a child two weeks old, with a well-marked large and defined cephalæmatomatous swelling on each parietal bone, with the hard rim distinct at different points. He had never seen it on both sides except in this case. In this, as in most other cases, the tumors had not been observed till the first washing of the child, having come on, or at least grown greatly, for some hours after birth. The effused blood was already becoming absorbed, and, by leaving the case entirely to nature, a cure would soon be effected. The effusion was between the skull and pericranium.

He had watched various cases of cephalæmatoma during the process of a natural cure; and he several times found that a layer of bone is formed on the inner surface of the separated pericranium, which can sometimes be felt distinctly to crackle under the finger like parchment—and, as the fluid gets absorbed, the two plates of bone gradually approximate and come together.

Dr. S. believed that these tumors were often mistaken and maltreated, by too active measures being employed. He had now had an opportunity of seeing a considerable number of cases of cephalæmatoma, and he had never seen any treatment required except time and patience. The difficulty in their management generally consisted in keeping the friends and others from doing something or other to them, when nothing in reality was required.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, January 12, 1848.

DISEASED STATES OF THE UMBILICUS AFTER BIRTH.[1]

(From Edinburgh Monthly Journal of Medical Science, July, 1847, p. 70.)

1. *Fungating Excrescences of the Umbilicus in Infants.*—In infants, after the umbilical cord has mortified and dropped off, by a kind of natural "dry gangrene," instead of the resulting raw surface contracting and cicatrizing, I have several times seen large granulations appear, and a red, elevated, fungus-like excrescence, resembling the "fungous testis" of surgeons, form at the bottom of the umbilical depression. These umbilical excrescences in general shrink and slough after a time; or they do so on being touched with alum or other astringents, or with nitrate of silver. In one case, which I lately attended with Dr. Finlay of Newhaven, this simple treatment had little or no effect. The excrescence enlarged to the size of a cherry, which it likewise resembled in color. It was apparently insensible to touch; but blood oozed from its red surface under slight handling. It was cauterized several times with nitrate of silver; but this treatment did not cause it to shrink. At last, after several weeks, a ligature was passed round its base, and in a few days it had dropped off. It did not in any degree offer to return.

2. *Secondary Hemorrhage from the Umbilicus.*—Occasionally secondary hemorrhage occurs from the ulcerated mouths of the umbilical vessels, at the site of their natural separation, either before the cord is entirely thrown off, or far more frequently some days after this. The blood wells slowly up, fills the umbilical pit, recurs perseveringly, and often leads ultimately to a fatal termination. I have heard of several instances in which this secondary umbilical hemorrhage occurred in more than one member of the same family. It is frequently found combined constitutionally with jaundice, or with purpura; and locally with deposits and disease in the walls of the umbilical vessels. In the way of treatment all internal medication has failed, and local styptics, caustics, cauteries, and compresses, have proved of little, or indeed of no avail. I have known, however, two cases in which the transfixure of the bleeding part with a needle and including ligature—as in harelip—perfectly succeeded; and it seems to be the only plan of treatment on which any dependence can be placed.

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, April 13, 1847.

ON THE TREATMENT OF ERECTILE NÆVI.

I HAVE seen many different plans tried for the obliteration and removal of the small erectile tumors constituting the usual form of *nævi materni*. Latterly, in my own practice, I have been led to place most reliance upon the two following methods, as being at once the most certain as well as the most expeditious:—

1. The application of a pointed stick of *potassa fusa* to the surface and tissue of the tumor, carefully limiting, at the same time, the effects of the alkali by the free use of *vinegar*. With the potassa fixed in a common caustic-holder, one can easily and satisfactorily destroy the diseased tissue to the required extent and depth in the course of two or three minutes; and the free application of vinegar immediately afterwards, both at once arrests any further destructive action of the caustic, and annuls the sensation of pain and irritation on the part operated on. I have seen a number of both large and small nævi most successfully removed by this plan. But—

2. The *galvanic cautery*, as ingeniously proposed by Mr. Marshall for various purposes in surgery, has appeared to me a valuable means of effectually destroying some nævi when they occupied such positions upon the eyelids, lips, &c., and could not be very readily or safely treated by the potassa. In some cases where the erectile tissue ran deep, I have sometimes passed the platinum wire obliquely under the skin of the nævus, so as to break up and obliterate its interior structure without destroying much of its cutaneous covering; in other more superficial nævi its direct application to the cutaneous surface is sufficient.

In using either the potassa fusa or galvanic cautery, the little patients should be previously anæsthetized.

PROPOSITIONS REGARDING LOCAL PARALYSIS OCCURRING DURING INFANCY.[1]

(From Edinburgh Monthly Journal of Medical Science, January, 1851, p. 92.)

1. INFANTILE paralysis most frequently seems to affect a single limb—as one leg, or more rarely one arm;—sometimes a few fingers only. Occasionally it appears in the form of hemiplegia affecting one whole side; sometimes in the form of paraplegia. I have watched one case in which the paralysis occurred in early infancy,

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[1] Extracted from Proceedings of Edinburgh Obstetric Society, Dec. 11, 1850.

nd now permanently affects both lower extremities, the left upper xtremity, and the left side of the face. The child, now several ears old, is very acute and intelligent.

2. The side of the face, but more particularly the upper and wer extremities, when paralysed in infancy, do not grow in rela- ve proportion with the corresponding healthy parts; so that when e individuals affected reach adult life, the paralysed extremity ppears small, blighted, diminutive, and shorter than natural.

3. The paralysed limb does not appear to want sensation, and its otory muscular power, although greatly diminished, is not entirely bolished. When the local paralysis is seated in the leg—the part ost frequently attacked—the person usually walks awkwardly and nperfectly, throwing out the foot at each step with a flap-like otion, and often with the toes or external surface of the foot some- hat drawn inwards, as the leg is each time extended.

4. The disease generally comes on during the first three years of fe, and especially during the occurrence of that morbidly irritable tate of the nervous system which coexists with teething. I have een an instance in which two children of the same family were ffected within a week of each other.

5. Infantile paralysis generally supervenes very suddenly, some- imes in the course of a single night; and it is often, at the time of he attack, accompanied with little or no constitutional derange- nent; but occasionally it comes on with a fit of convulsions, or ther symptoms of temporary cerebral derangement. Sometimes t supervenes after exposure to cold, and with rheumatic pains in he affected limb. Intestinal irritation in some cases appears to be he exciting cause.

6. The affection is frequently first noticed immediately after fever, pecially after the eruptive fevers; and occasionally it comes on uring the period of convalescence from them. In one case para- legia came on in a child three years old, during the convalescence om scarlatina—the patient going to bed apparently well, and aking paraplegic, and astonished at her own want of power of ovement in both her legs. This patient has now reached puberty, d is so paraplegic as to be unable to stand.

7. When the patients do not recover from the paralysis within a w days or weeks after the attack, under antiphlogistic measures, isters and careful correction of the condition of the intestinal nal and other functions, the paralytic affection almost always oves chronic, and, indeed, permanent. I have seen counter-irri- tion to the spine, galvanism, &c. &c., perseveringly employed in e chronic forms, but without much or any success. Small and ng-continued doses, however, of nux vomica have appeared to

me in several instances to act beneficially in diminishing and even curing the state of paralysis. Keeping all the functions of the body as near the standard of health as may be, friction and bathing of the affected limbs, and inculcating as much muscular exercise of them as possible, seem to be the principal indications of treatment when the disease has already passed into the subacute or chronic type; they restrain the amount of the subsequent muscular atrophy of the limb.

8. The true pathology of the disease is as yet imperfectly known, though the affection is of frequent occurrence. No sufficient number of careful autopsic investigations appear to have been made with the view of ascertaining the state of the brain, spinal cord, nerves, and muscles in the local paralysis of infancy.

ON THE PATHOLOGICAL CONNECTION BETWEEN CHOREA AND RHEUMATISM.

(From Association Medical Journal, February 25, 1853, p. 172.)

1. In a case that I attended some time ago along with Dr. W. H. Douglas, the patient, a young lady of seven or eight years of age, after suffering from severe rheumatism in the lower extremities, presented symptoms of chorea, and ultimately, in the course of the disease, was attacked with subacute pericarditis, which proved fatal. The effusion of coagulable lymph on the pericardium, as ascertained by dissection, was extremely great. 2. Some years ago I saw, with Dr. Robertson, another case in which, first, there occurred rheumatic fever, followed by pericarditis; and, secondly, when this pericarditis was at its height, symptoms of chorea supervened. The patient recovered, but with a heart much diseased by the attack. 3. In one family I witnessed a set of cases similar to those described, a few years ago, by Dr. Begbie.[1] Two children, a sister and brother, were attacked with rheumatism. In the boy, the rheumatism was followed by an affection of the heart, which proved permanent, and was probably inflammatory in its origin. In the sister, the rheumatic attack, on the other hand, was followed by severe and prolonged chorea, complicated with a kind of hemiplegia. She recovered at last, under the use of arsenic.

The investigations which have been lately published by some

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[1] See his interesting paper on the Relation of Rheumatism and Chorea, in the Monthly Journal of Medical Science for 1847, p. 740. The observations in the text were made at the Medico-Chirurgical Society when a paper was read on the subject by Dr. Warburton Begbie.

French pathologists seem to show that the connection between chorea and rheumatism is even more frequent than has been previously supposed, particularly among young children. In the Parisian Hôpital des Enfans, attention having been particularly directed to this subject, more cases were found of chorea and rheumatism combined among the young patients, than of simple uncomplicated rheumatism: at least, such is the assertion of M. Sée. This pathologist further stated, that in the post-mortem examination of individuals dying of chorea, between 35 and 40 per cent. of the bodies were found to present inflammatory effusions upon the surface of internal serous membranes; and pericarditis was one of the most common of these complications.

The old explanation attempted to be given of the connection of chorea with rheumatism, as to its being a nervous or sympathetic connection, &c., is now sustained by few pathologists; and the pathological idea of the relation maintained by Dr. Begbie seems more certainly the correct one, viz., that these two apparently different diseases depend upon some identical or analogous blood-poison. When pathological chemists at last discover what the blood-poison in rheumatism really is, there is every reason to believe that the same blood-poison, or some modification of it, will be found to be also the cause of chorea; however much the two diseases differ symptomologically. The morbific state of the blood existing in rheumatism and chorea predisposes, as late dissections have shown, to produce inflammation of internal serous membranes. The morbific state of the blood in albuminuria has, in a noted degree, the same tendency, as specially shown by Dr. Taylor and others, to excite serous inflammations—as pericarditis, peritonitis, &c.—possibly from the morbid excretory poison existing in the blood, whether urea or not, being metastatically eliminated on these serous surfaces, and consequently irritating and inflaming them. These two morbid poisons are analogous in another respect. While the poisoned state of the blood accompanying or produced by albuminuria has a tendency in some individuals to produce local serous inflammations,—it tends in others, and particularly in pregnant females, to affect the nervous system in the form of convulsions. So the blood-poison in rheumatism, whatever its nature may be, has a tendency, in the same way, to produce internal serous inflammations in some individuals; and in others, particularly in the young, and in the female sex, it tends to affect the nervous system in the form of chorea. Both, thus have a power or predisposition to produce, on the one hand, internal serous inflammatory lesions, and on the other, peculiar affections of the nervous system. Perhaps a closer investigation would prove them also to be analogous in other pathological respects, as in the morbid state of the urinary secretion.

CHLOROFORM IN INFANTILE CONVULSIONS AND OTHER SPASMODIC DISEASES, AND IN PNEUMONIA.

(From Edinburgh Monthly Journal of Medical Science, January, 1852, p. 39.)

"THERE are," observes Dr. Churchill,[1] "few diseases of infants and children which are more formidable or more fatal than convulsions." The great number of deaths from convulsions, especially in infancy, which appears in all our published Mortality Returns, so far bears out the justness of Dr. Churchill's remark. During the five years from 1838 to 1842 included, there occurred, according to the Registrar-General's official returns, 127,276 deaths from convulsions in England and Wales.[2] Of these deaths, amounting to about 25,000 annually, almost all are among children below five years of age; and the greatest proportion of cases and deaths takes place among infants during the first year, or rather during the first months or weeks of life.[3]

Without entering into the question of the nature of the different types or forms of convulsions observable in early life, I shall content myself, at present, with referring to the general opinion of pathologists, that by far the greatest proportion of infantile convulsive attacks are sympathetic or functional merely;—a predisposition to the disease being laid by an undue excitability, or super-polarity, of the cerebro-spinal or rather "true spinal" or reflex system of Dr. Marshall Hall; and the immediate exciting cause of the affection being usually traceable to some morbific irritation of a distant excitant surface or part, as the stomach, bowels, teeth, &c. Hence, when the disease proves fatal under this form, no organic lesions are usually detected. "Dissection," says Dr. Merei, "having incontestably established, that in the great majority of cases of infantile convulsions terminating fatally, there is no cerebral or spinal inflammation, nor even evidence of active vascular congestion."[4]

Consequently, in cases of infantile convulsions, particularly when of a sympathetic, reflex, or eccentric type, after removing all the traceable exciting sources of irritation, and diminishing any excess of vascular action in the nervous centres, physicians have generally proceeded to combat the disease, if it still persisted, with medicinal agents that tended to reduce the super-irritability of the excito-motory system, or otherwise to restore it to its proper and healthy standard of action. To fulfil this indication, preparations of zinc, iron, &c., have been used in the more chronic cases; and in the more acute or sub-acute cases, antispasmodics of very different kinds, as opium, hyoscyamus, musk, &c., have been generally employed. In the following instance, after all the ordinary means of treatment failed, chloroform was used as an antispasmodic with the most marked and satisfactory effect.

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[1] Diseases of Children, p. 97.

[2] Seventh Annual Report of the Registrar-General, p. 63.

[3] "The frequency of convulsions has, in my practice, appeared most considerable in the first month of life; from this period the disease becomes gradually rarer up to the fifth month, and then again more common up to the period when the incisor teeth make their appearance. After this age, the disease again becomes rare."—See observations of Dr. Schœpf Merei, formerly Professor of the Diseases of Children in the University of Pesth, in Monthly Journal for 1850, p. 566.

[4] Monthly Journal for 1850, p. 566. See also Rilliet and Barthez's excellent work (Maladies des Enfans), vol. ii. p. 281; North's Practical Observations on Convulsions of Infants, p. 45; Bouchut's Manuel Pratique des Maladies des Nouveaux-Nés, p. 387, &c. &c.

Case.—The Viscountess —— was confined on the 7th October. The child, a boy, kept quite well till the 17th of the same month, when it was observed by its nurse, two or three times during the day, to have twitchings in the muscles of the face; but they were not so severe as to attract any very special attention. During the two following days, these convulsive twitchings were repeated with rather greater frequency; the hands were observed to be clenched during them, and the thumbs were turned inwards.

On Monday the 20th, the convulsions became far more violent in their character, were more prolonged in their duration, and were repeated with much greater frequency. They continued with little change, and no abatement in their intensity or frequency, for the next fourteen days. Sometimes they affected the right side of the body much more severely than the left. In the meantime, Dr. Scott and I tried a great variety of means for their relief; but all in vain. The bowels were well acted upon with mercurials, magnesia, &c.; and every separate function attempted to be brought as near as possible to the standard of health. A new wet nurse was procured, lest the milk might perchance have been proving, as it sometimes does, the source of irritation. The child was placed in a larger and better ventilated room. Ice and iced water were occasionally applied to the scalp. At one time, when the fits became unusually prolonged, and were not only accompanied, but followed for a time, by much congestion in the vessels of the scalp and face, and an elevated state of the anterior fontanelle, two leeches were applied. Liniments of different kinds were used along the spine. Musk, with alkalies, was given perseveringly for several days, as an antispasmodic; and small doses of opium, turpentine enemata, &c., were exhibited with the same view. All these and other means, however, proved entirely futile. As I have already stated, it was on Monday the 20th October, that the fits first assumed a severe character, and they continued without any amelioration for about fourteen days from that period, recurring sometimes as frequently as ten or twelve times in an hour. At last the child, who had hitherto wonderfully maintained his strength and power of suction, began to show symptoms of debility and sinking; and during the fifteenth and sixteenth days of the attack, the fits became still more violent, and more distressing in their character. They were now accompanied with moans and screams that were very painful to listen to; symptoms of laryngismus and dyspnœa supervened towards the termination of each fit; and in the intervals the respiration, as well as the pulse, continued much quickened.

During these two last days of the disease, the exhaustion became so great, the dyspnœa in the intervals so distressing, and the fits so very violent and constant (seventeen being counted in one hour), that Dr. Scott and I gave up all hopes of the possible survival of the infant. We had exhausted all the usual means of relief. Ultimately, but much more with the view of abating the screaming, laryngismus, and other distressing symptoms under which the little patient was suffering, than with any great hope of permanent relief and cure, I placed the child, on the forenoon of the 5th November, for about an hour under the influence of the inhalation of chloroform. During this hour there was no recurrence of the fits; but in a short time after the withdrawal of the action of the anæsthetic, the convulsions recommenced with their old violence and frequency. The benefit, however, was sufficient to encourage a longer repetition of the remedy; and from four to eight o'clock in the afternoon of the same day, my assistant Dr. Drummond placed and kept the child again under the influence of chloroform—a few inhalations, from time to time, of a very small quantity of the drug sprinkled upon a handkerchief, and held before the face of the infant, being sufficient for this purpose. It was specially applied at any threatening of the recurrence of a fit, and during the four hours in question all convulsions were in this way repressed. When the child was allowed to waken up at eight o'clock, it took the breast greedily, and continued well for upwards of an hour, when the convulsions again began to recur. At last, about twelve o'clock P.M., it was again placed under the inhalation of chloroform, and kept more or less perfectly under its action for upwards of twenty-four continuous hours, with the exception of being allowed to awaken eight or ten times during that period for the purpose of suction and nourishment. During most of this period it was carefully watched by Dr. Drummond, and at last the nurse was intrusted with the duty of adding the few drops of chloroform to the handkerchief, and exhibiting them at any time the child was offering to awaken or become restless.

After this long continuation of the chloroform, the child, on being allowed to waken up, as usual, drank greedily at the nipple, and immediately fell back into a quiet and apparently natural sleep. The chloroform and all other formal medication was in consequence discontinued; and from this time there was subsequently no recurrence whatever of the convulsions. In about ten days the child was removed with the family to the country. I have, within the last two days (December 18, 1851), seen the child as it was passing through Edinburgh. It was strong, plump, and well grown for a child of ten weeks, and was, in fact, revelling in the best of health.

In exhibiting the chloroform to this infant, ten ounces of the drug were expended; but of course a very large proportion of this quantity was lost by evaporation, in consequence of the mode in which it was employed.

I have known the inhalation of chloroform similarly useful in other cases in arresting infantile convulsions; but I am not acquainted with any instance in which the patient was so young as in the above instance. In the adult also, especially in cases of puerperal convulsions, I have now repeatedly seen the inhalation of chloroform as signal and satisfactory in its antispasmodic power over the convulsive fits, as it was in the little patient whose case I have described. Tetanus and epilepsy have been temporarily arrested and controlled by it. And perhaps it will yet be found one of our most certain and beneficial therapeutic means in the functional forms of those different convulsive or spasmodic diseases that are produced either by an undue excitability of the true spinal system, or by distant morbid irritations acting though this, the excito-motory system. Such reflex convulsive or spasmodic affections are, as is well known, particularly common in infancy and childhood. I have seen its use arrest laryngismus, colic, hiccup, &c.; and cases have been detailed to me of its occasional successful use n asthma, spasmodic urethral stricture, &c. But there is one common and too fatal spasmodic disease, almost confined to the period of childhood, in which I have seen anæsthetic inhalations uccessful in arresting and controlling the paroxysms, and where probably a more extended and persevering use in the employment of them would be found to be attended with beneficial effects. I allude to hooping-cough. I have known chloroform inhalations greatly abate the irritability of the cough attendant upon phthisis, &c. But with others, I have scrupled to use chloroform inhalations in hooping-cough, under the fear that they might possibly increase the great predisposition which exists in this affection to pneumonic inflammation, or aggravate that inflammation if it were already present. This *a priori* reason, however, against the use of chloroform inhalations as an antispasmodic in hooping-cough, has been of late set aside by the observations and experience of different German physicians. In a paper, containing some remarks relative to the medical uses of chloroform, published in the Monthly Journal for December, 1847, in addition to its employment as an antispasmodic, anodyne, &c., I suggested the drug acting as a contra-stimulant in some inflammatory diseases, and particularly in those of a painful kind. Latterly, we have had records published of its employment in upwards of 200 cases of pneumonia in German practice. Out of 193 cases of pneumonia treated with chloroform inhalations by Wachern, Baumgärtner, Helbing, and Schmidt, 9 patients died, or the mortality amounted to 4½ per cent. Dr. Varrentrapp has given chloroform in 23 cases of pneumonia in the Frankfort Hospital. One of these 23 patients died.[1] The detailed results in the other 22 cases seem to have been sufficiently satisfactory.[2] At all events, the effects of the chloroform inhalations upon the cough, expectoration, &c., and upon the general course of the disease, would appear to show that we need have no fear of deleterious effects from it, so far as regards the chance or existence of pulmonary inflammation; whatever advantages we may derive from it in relation to its prevention of that inflammatory state by allaying the cough, keeping the lungs in a relative state of quietude, and abating or estraining the succession of characteristic spasmodic attacks. I peak, of course, of the more severe cases of pertussis; for the milder forms of it require care merely, rather than actual treatment.[1]

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[1] It is proper to add, that during the time that these 23 cases of pneumonia were admitted into the Frankfort Hospital, and treated in that institution by chloroform inhalations, three other cases of the same disease presented themselves, where the patients, at the time of application, were already in a hopeless state. Chloroform was not tried with them.

[2] Out of these 23 cases of pneumonia reported by Varrentrapp, in *addition* to chloroform, the first was treated by venesection and antimony; a second case was bled; and two others that were complicated with pleurisy, had calomel exhibited and blisters applied; the remainder were treated with chloroform alone, about sixty drops being placed upon a piece of cotton, the vapor inhaled for ten or fifteen minutes, and the dose repeated every two, three, or four hours. It was not given so rapidly or strongly as to produce unconsciousness. The patients were all adults; the mean period of the disease at their entrance into the hospital was the fourth day; and the chloroform treatment was usually commenced on the following morning. The effects of the chloroform inhalations seemed generally to be—1. The induction of perspiration, sometimes after the first inhalation, in no case later than the third or fourth. 2. Gradual diminution and ultimate disappearance of pain in the thorax or side. 3. Relief of the feeling of thoracic tightness. 4. Daily decrease of the frequency of respiration from thirty-seven per minute (the average on admission) down to the natural standard. 5. In all cases, without an exception, the cough was lessened by the inhalations; the intervals between the coughs shortening, the cough itself being less violent, and the expectoration looser; the sputa gradually losing their red tinge, and diminishing in quantity. 6. The pulse fell rapidly in frequency (down to eighty on an average on the fifth day of treatment), and the fever diminished gradually, in one case suddenly. 7. Good and comfortable sleep ensued on an average on the third or fourth day after the commencement of the chloroform inhalations.—See Henle's Zeitschrift für Rationelle Medicin, and the London Medical Times for October 18, 1851.

SIMULTANEOUS COEXISTENCE AND PROGRESS OF SMALL-POX AND COW-POX,

THEIR MUTUAL INFLUENCE ON EACH OTHER.[2]

(From Edinburgh Monthly Journal of Medical Science, September, 1854, p. 276.)

In two cases, I have seen cow-pox and confluent small-pox co-existing and progressing side by side on the body of the same patient; and other examples of the same kind have been mentioned to me by Dr. Weir, Dr. Girdwood, &c. The first case in which I witnessed the simultaneous occurrence and progress of these two affections, occurred several years ago in a patient attended by my friend Dr. Paterson of Leith.

Case I.—A boy was affected with variola in a confluent form. He had a younger brother unprotected, and with the hope of saving him from an attack of variola, Dr. Paterson vaccinated this brother. But the small-pox eruption began on him three days after vaccination, and gradually assumed a confluent form. The cow-pox appeared almost precisely at the same time as the variolous eruptions, and the two diseases progressed regularly and methodically, seemingly altogether unmodified by each other. The patient died on the ninth day.

Case II.—A patient of Dr. Middleton's, aged five months, was accinated on the 28th December, 1852. Two days afterwards an eruption, which turned out ultimately to be small-pox, began to ppear; and on the subsequent day (the third day from the vaccina-on), the cow-pox vesicle commenced. In the course of eight days the vaccine vesicle had, as usual, attained its perfect size and de-elopment. In the meantime, also, the variolous eruption followed its normal progress, and was confluent on the face and hands, but discrete on the other parts of the body. The variolous pustules were matured by the tenth day. The child died on the fourteenth day after vaccination, or on the twelfth after the appearance of small-pox.

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[1] For albuminuria in connection with infantile convulsions, see Vol. I. p. 725. Since the preceding case was published, various instances of infantile convulsions have been treated successfully by Dr. Simpson and others with chloroform.—See some excellent remarks on its use in hooping-cough, by Dr. Churchill, in the Edinburgh Monthly Journal of Medical Science for August, 1853, p. 180.—(Ed.)

[2] See Proceedings of Edinburgh Obstetric Society, Session xii.

These two cases show, like many others on record, that when small-pox and cow-pox appear upon the body at, or near the same time, they have not that effect in mutually arresting and preventing each other which belongs to them when an attack of one of these diseases precedes for a short time the inoculation of, or exposure to, the other. Indeed, we know from a case observed and described by Dr. Willan, that the two diseases may not only coexist together on the same individual, but even on the same spot of skin, or within the circuit of the same pustule; and each progress regularly on in its own course towards maturation. In this case of Dr. Willan's, the two coexisting and continuous diseased secretions kept so perfectly separate, that at last the vaccine portion of the common pustule was found capable, by inoculation, of communicating cow-pox, and the variolous portion capable of communicating small-pox.

Since the time of Willan, many authors have recorded cases of the simultaneous existence of small-pox and cow-pox. Legendre and Bosquet have, in particular, collected a variety of data on the subject. Here, as elsewhere in pathology, there are no universal laws; but the principal facts or general laws relative to the mutual effects and influence of these two diseases upon each other in the human economy, may be briefly stated as the following:

1. When the small-pox and cow-pox eruptions appear on the skin on the same day, or within one or two days of each other, the two affections usually—as in the instances detailed—pass through their natural courses, unaltered in their forms and progress by each other.

2. If the type of small-pox with which the patient is affected, is originally confluent and virulent, the mere simultaneous coexistence and progression of cow-pox does not, as seen in the two preceding cases, mitigate the severity, or avert the fatality of the variolous disease.

3. When the specific eruption, however, of one of these diseases distinctly forestalls the other as much as four, five, or six days, the first or earlier disease, whether small-pox or cow-pox, does not undergo any change or curtailment in its own natural phenomena or progress; but the second or latter disease is usually more or less distinctly modified in its intensity, and abridged so much in its

course as to arrive at its acme at or near the time of the maturation of the first or prior eruption.

4. In this last respect, the abortive influence of an already existing small-pox or cow-pox eruption upon a supervening eruption of either disease, is similar to the abortive influence of an already existing small-pox upon a newly inoculated small-pox pustule, or of an already existing cow-pox upon a newly inoculated cow-pox vesicle —and in the latter affection constitutes the so-called test-pock of Mr. Bryce.

5. When fully a week or more has elapsed from the appearance of small-pox or of cow-pox, and more particularly when the variolous or vaccine eruption has already run its full course, the inoculation of, or exposure to the one or to the other disease, is followed by no result or eruption whatever—the constitution being now so changed and protected as to be generally proof for the remainder of life against the poison of either small-pox or cow-pox.

6. Exceptions, however, occur not unfrequently to this last great general law;[1] persons being sometimes met with capable of twice taking small-pox;[2] or twice taking cow-pox; or of taking small-pox after cow-pox; or cow-pox after small-pox.

7. When a second attack of either disease thus occurs subsequently in life, this second attack seems usually not liable to follow till years have elapsed; and commonly the eruption is mild and modified in its character. But I have known a person die of a second attack of small-pox, though pock-pitted by the first; and the late Professor Thomson saw a second and well-marked attack of small-pox appear in a student before he had entirely recovered from a previous attack of the same disease.

In the two cases which I have detailed, the children were not vaccinated till after they had been exposed for some time to the small-pox contagion, and till the variola—as was evinced by the date of the subsequent eruption—was already near the height of its stage of incubation in their systems. Hence, the vaccine vesicle, which requires little or no stage of incubation, and the variolous pustule appeared too simultaneously to be of any use in controlling each other. If, however, the eruption of the vaccine vesicle had fortunately preceded by four, five, or more days the eruption of the variolous pustule, the latter would in all probability have been modified and abridged, if not entirely arrested by the previous existence of the former. It becomes, in consequence, a matter of great moment to vaccinate always as speedily as possible any unprotected child or adult who happens to be exposed to the contagion of small-pox—the question of the prevention or modification of the variolous by the vaccine disease being, as a general law, merely a question of relative time and precedence. It is important also to state that numerous recorded facts show that even during the incubating stage of small-pox in the body, artificial vaccination will usually produce a vaccine vesicle, and that this vesicle will protect against or modify the small-pox, provided only the vaccine eruption forestalls the variolous by a sufficient length of time.[1]

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[1] See a great mass of confirmatory literature on this point in Dr. Thomson's Historical Sketch of the Varieties and Secondary Occurrence of Small-pox.—1822.

[2] Gideon Harvey, in his Treatise on Small-pox and Measles (1696), in speaking of the cases of small-pox (p. 71) observes, "It is agreeable with universal experience that three-fourths of those northern regions do once or oftener in their lifetime pass through that sort of purification (small-pox), many once, some twice, and some very few *three* times."—Schweinseber avers that he saw a robust boy suffer *five* attacks of small-pox and survive them all.—(Ephemerid. German, Dec. ii. an. 6.) Borel describes the case of a French woman who died, as he states, under an *eighth* attack of small-pox.—(Hist. et Observ. Medico-Physic, p. 203.)

ON THE RESULTS OF VACCINATION;

AND THE POSSIBLE PROPHYLAXIS, ETC., OF SCARLATINA, MEASLES, AND OTHER DISEASES.

(From Monthly Journal of Medical Science, April, 1853, p. 363.)

THE vast importance of vaccination can only be appreciated when we venture to consider for a moment what would at the present day have been the dreadful state of mortality in our own and in other countries from small-pox, if no such discovery had been made. If small-pox were as fatal now, with our population of twenty millions, as it was during the latter period of the last half century, or before vaccination, it would destroy in Great Britain alone some 80,000 lives year; for with a population of about eight millions, it then yearly roduced a mortality amounting to 30,000 or 40,000 deaths.' Physiians may proudly point to this single discovery alone, as a victory f medicine over disease and death, unequalled in its greatness and nportance by any of the wondrous discoveries that have been made hrough the whole past century in any of the physical arts or ractical sciences. By itself alone vaccination has extended already he average sum of man's life upon earth some three or four years.

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[1] *Present extent of small-pox and its prevention.* At the Meeting of the Edinburgh Medico-Chirurgical Society, February 21, 1849, Dr. Simpson observed, in regard to the prevalence of small-pox, that calculating from the number of deaths by small-pox recorded in England and Wales, probably 8,000 or 10,000 persons died every year of variola in Great Britain; *or about one human being every hour*. He further observed, that he believed that this number of lives would betimes be saved annually if the legislature would adopt a means of insuring vaccination in all. And he argued that it would require no expenditure or new machinery to do so. All that was necessary was to enact, that besides the registration of the *birth* of every child being demanded by law (as was at present in England, and would soon be in Scotland), the registration of the *vaccination* of it should be equally demanded by law within six months, or any other specified time, after its birth. See Monthly Journal for April, 1849, p. 695.—(Ed.)

Nor are the marvellously simple means by which medicine has een able to accomplish this mighty end less remarkable than the nd itself. With an almost invisible speck of matter upon the point f his lancet, the medical man can in a great measure defy one of the most fatal, and at the same time one of the most loathsome, diseases that ever afflicted the human race. For even when it spared life, we must remember how often in former times an attack of small-pox spared not the features and the very eyesight of the scaped patient. And indeed the more deeply any man considers the subject, the more will he venerate the name and discovery of JENNER, who was thus, as it were, to use his own language, intrusted with the mighty and responsible office of shutting one of the great gates of human death.

During the last half century, military science has, it is often verred, advanced greatly and permanently; that is, it has invented uring that period much new and dreadful machinery, and many ew and horrible missiles for the destruction of man and man's orks. During the same period, in the discovery of vaccination lone, medicine has invented a means far more potent to save, than he soldier has invented means potent to destroy. If modern military science can boast of the discovery of wondrous shells and hrapnells, of destructive rockets and rifles, by which the deaths of housands of men may be easily effected—modern medical science an boast of the discovery of a small vesicle, by the multiplied conents of which, the death of thousands and tens of thousands of en may be easily prevented. For truly, the millions of money xpended in the vast military stores of Woolwich and Cherbourg, ack the ability to destroy human life to any such degree as one drop of despised cow-pox matter, with its powers of multiplication n the system, has the ability to save it. The lancet of Jenner has, during the last half century alone, saved in the world more human ives than, during the past or any other century in the history of mankind, gunpowder and the sword were ever yet successful in slaying. How often do we find the art of war described as a "noble and glorious art!" But when we consider in our hearts the relative effects of war and of medicine upon human life and human happiness, the physician may surely arrogate such a title for the art of medicine on infinitely better and higher grounds than the soldier can claim it for the art of war.

It is now some two hundred years since Sydenham wrote and practised in London. Look for a moment what this father of English medicine described as the most fatal diseases in England at that time. Six of the most destructive diseases, or classes of diseases, to human life in that age were, according to Short's mortality bill, plague, ague, dysentery, scurvy, childbirth, and small-pox. These maladies, however, enjoy their fatal pre-eminence no longer. We have banished the plague, which in the olden times, often destroyed in London more lives than all other maladies counted together. We have rooted out and modified the ague, so that it is rarely a fatal affection now, while every year it destroyed thousands formerly. Scurvy has almost disappeared from our mortality bills. Dysentery, though still sometimes a fatal disease, is infinitely less common, and infinitely more under medical treatment, than it was at the time of Sydenham. At that date, or in the middle of the seventeenth century, about 1 in every 40 or 50 women delivered in London died of child-birth or its consequences; but gradually, as medical science has advanced, that mortality has decreased, till now not above 1 in 150 or 200 die. We have in Great Britain about 600,000 deliveries annually, and still above 3000 of the mothers perish in childbirth. If the old mortality, however, of the seventeenth century yet held good, and this department of practice had not greatly progressed and improved, not less than 11,000 or 12,000 maternal lives would now be lost by the present proportion of annual births;—the advancements of modern science thus effecting in this item alone a saving of the lives of 7000 or 8000 mothers every year.[1] And as nall-pox—another of the great sources of human death in the of Sydenham—this fearful and most destructive malady has, have just stated, been almost entirely destroyed by the disy of Jenner.

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[1] As an illustration, look for a moment at the whole class of infantile diseases. In the latter years of the last century, Dr. William Hunter and the other leading English authorities in those days on such affections, believed and taught all infantile diseases to be the results either of fever, and to be treated with anti-febrile means; or the results of weakness, and to be treated with tonics and stimulants. The pathological investigations, however, of the last half century, have proved that the different organs of the child are liable to nearly as numerous and diversified morbid actions as the same organs are in after life; that the same classes or types of diseased action occur in the system of the child as occur in the system of the adult;—that some maladies, particularly of an inflammatory character, as pneumonia, peritonitis, encephalitis, &c., which were supposed to be very rare in infancy, are now known, on the contrary, to be exceedingly common during that fragile period of life —and that the medical management of infantile maladies must be varied as in the adult according to the individual disease present, and cannot be conducted on the belief that all affections in infancy are amenable to one or two modes of general treatment. The hygiene and management of infancy have in an equal degree improved. And the whole changes in these respects have produced great changes in the mortality of the earlier years of life. Formerly, towards the middle of the last century, fifty or sixty out of every hundred children born in London, died before they had reached their fifth year; but the mortality has

ese most formidable and fatal diseases of Sydenham's time in the pre-eminence of their formidable and fatal character no r. And may we not hope that a couple of centuries hence, ery same fact may hold true of some of those diseases that are esent most destructive and deadly in their effects upon our ation? Does not the history of the past suffice to encourage n us a bold belief, that perhaps in half a century or a century , our present most fatal diseases may, by the advancement of nic and medical means, be our most fatal diseases no longer? ie present day we have, according to the registrars' official is for England and Wales, consumption producing above 0 deaths a year; convulsions above 22,000 deaths; pneumonia ronchitis above 30,000 deaths a year; typhus fever and scarlet generally above 20,000 deaths each; and measles, hooping-, rheumatism, diarrhœa, hydrocephalus, &c., destroying human nong us in less, but still in fearful numbers. These several es stand at the present time as the highest on the roll of deive agencies that prevail in British practice. For one, I confess cannot but entertain an ardent belief that medical science et devise measures, prophylactic, perhaps, rather than curative, y the great destruction of human life prevailing amongst us the first, for example, of these affections, phthisis. Perhaps a advanced pathology and chemistry may yet ere long furnish th more enlightened and practical views of pneumonia and inflammatory disorders than we yet possess, and arm us with sure and potent medicinal weapons and resources against

We have, from the experience of the last few years, every to hope that the whole class of zymotic diseases will be y subdued betimes in intensity and violence, when the investiof the physical causes predisposing to them, or even actually ig them, is more fully expiscated.

ides, if by vaccination during infancy, medicine has devised ylactic means to arrest the ravages of small-pox, may it not evise some analogous means also to arrest the ravages of t fever and measles, of hooping-cough, and typhus fever, and

y and steadily diminished, so that now not above thirty or thirty-five in every hunat that early period. At the present time there are above 600,000 children born in Great Britain. According to the above scale of mortality, above 300,000 of these ave perished formerly before they were five years old, now only about 200,000 die he first five years of life: thus showing a saving of human life in this item alone, tent of at least 100,000 human beings a year.

perhaps of the whole class of non-recurrent diseases, by artificially producing these several diseases in a mild and safe form by inoculation and imitative medication or otherwise?[1] And even if we fail to arrest them, we may possibly find out for the varying animal poisons producing these diseases, antidotes as certain as quinine or arsenic are antidotes against the poison of marsh fever.

If, at the present moment, any individual in the profession could happily point out some certain prophylactic means of averting, by antecedent treatment, the liability to those two analogous or identical diseases—puerperal and surgical fever—he would assuredly make a greater and more important discovery than could possibly be attained in any other subject of investigation in the whole range of obstetrical and surgical science. Nor does such a result seem hopelessly unattainable, particularly when we see the occurrence of the disease in question—namely, surgical and puerperal fever—regulated by some special, constitutional, and other circumstances which we may yet be able to detect and remove. Surgical fever, for instance, sometimes shows no tendency to occur, even after the most severe forms of operation; whilst, in other individuals, it will occasionally spring up suddenly and fatally after the most trifling use of the knife or ligature. And if its supervention is thus regulated and modified by various special conditions of the body, why may medical science not yet detect, or at all events be able to control and regulate, these conditions so as to produce artificially that state of the constitution in which it resists most effectually any tendency to the supervention of these fevers.

Let us at least not sit indolently down and argue ourselves into the belief that it is impossible to attain such results. The conquest of small-pox seemed to our forefathers a hundred years ago as impossible as the conquest of these maladies can look to any one now; and yet we all know that the subjugation of small-pox was effected by the genius of one man, and the devotion of one mind to its accomplishment. Some time before Jenner turned his attention to the subject, the learned and accomplished Dr. Mead—the first London physician of his day—wrote of the utter hopelessness of the very idea of battling with and vanquishing such a formidable enemy to human life and happiness as small-pox. He speaks of the possibility of a "specific antidote being found against the con-

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[1] The idea of thus possibly protecting the system against non-recurrent diseases, by imitative medicinal diseases, is one, I think, which should not be lost sight of. I have often seen the internal use of tar capsules produce an eruption, &c., very exactly resembling scarlatina; and for the most part, if not always, individuals whose constitutions had never been affected by that malady. Are those engaged in tar manufactories liable to scarlatina? Which of the various organic principles existing in tar causes this scarlatinoid eruption.

agious poison" of small-pox—that is, an antidote "by which it
ay be so thoroughly destroyed, that though it had been received
nto the body, it may not produce the disease," as an idea as wild
nd chimerical as that of alchemy; and one, in his opinion, out-
ging "the principles and elements of things that are so certain
nd so well established by the permanent laws of nature."[1] These
isheartening opinions of Dr. Mead regarding the hopelessness
f ever gaining a prophylactic for small-pox, were published in
747. Before, however, fifty years had elapsed, Jenner had both
iscovered, and successfully adapted to practice, the great prophy-
ctic measure that has rendered his name imperishable in the
nnals of the human race. And thus the happy results of Jenner's
abors—besides their own intrinsic and inestimable worth—descend
o us stamped with a precious lesson from him, of hope and perse-
erance under alleged impossibilities; for these results preach loudly
o the profession in all time to come, that we should never permit
ny apparent improbabilities to prevent us from going always on-
wards in earnest search for possible means of conquering and curing
iseases even of the most formidable and fatal kind.

EXTERNAL USE OF OIL

IN THE PREVENTION AND TREATMENT OF SCROFULA, PHTHISIS, ETC.

(From Edinburgh Monthly Journal of Medical Science, Oct. 1853, p. 316.)

In Great Britain above 70,000 individuals die every year of pul-
onary consumption and scrofula.[2] In other words, tubercular or
rumous diseases destroy in our island nearly 200 lives every day;
r eight individuals every hour. Of the 70,000 deaths, about 60,000
re the result of pulmonary consumption alone; this fatal malady
arrying off on an average nearly 170 of our population daily; or
xtinguishing among us a human life every ten minutes.

To arrest this frightful mortality, and to prevent, retard, or cure
his—the most destructive variety of human disease—medical
cience has proposed an almost infinite variety of means. All are
eady to lament how comparatively unsuccessful our attempts still

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[1] See his Medical Works, vol. ii. p. 131 (edit. of 1763).

[2] Under the term scrofula I include both the more external forms of tubercular disease nd its more marked internal forms, as Tabes Mesenterica. The calculations in the text for Great Britain are calculated upon the official returns to the Registrar-General of the deaths in England and Wales; for unfortunately no Registration Act has yet been procured for Scotland.

are. I trust, therefore, that any new practical suggestion on the matter, tending, in however slight a degree, to abate the violence and mortality of such a fatal form of malady, will be received by my medical brethren with indulgence at least, whether the data which I have to adduce convince them or not of its probable importance and success in this and in some other morbid states.

A few months ago (December, 1852), when on a professional visit to Galashiels in Roxburghshire, my friend Dr. Macdougall incidentally directed my attention to the healthy state and robust appearance of the operatives at the large woollen manufactories in that town. In the course of conversation he further informed me that these operatives were specially and strikingly exempt from consumption and scrofulous diseases; and that they themselves attributed the immunity which they enjoyed from these affections to the free external application of oil to their bodies which occurred in various parts of the manufacture of woollen fabrics. This latter observation appeared to me particularly interesting, for, as I remarked to Dr. Macdougall at the time, if oil applied *incidentally* to the skin during working-hours in the common course of factory labor was capable of preventing or arresting struma or phthisis, the same means ought to be followed by the same effects, with still greater certainty, if the oil were applied *methodically* to the same surface with the regularity of an artificial medicinal agent.

The casual observation of Dr. Macdougall appeared to me so interesting in itself, and possibly so important in the consequences to which it might lead, that it seemed a matter of moment to ascertain—*first*, If the same relative immunity from phthisical and strumous disease had been observed among the workers at other woollen factories in Scotland; *secondly*, if this immunity were attributable to the external application of oil; and *thirdly*, if the employment of external inunction, when resorted to as a prophylactic or therapeutic means, were capable of acting beneficially upon the body, and could be applied practically in the prevention and treatment of consumption, scrofula, and other affections.

In the following communication I propose to state under several separate heads a brief, and, I fear, very imperfect outline of the results of this investigation.

I. *Evidence of the comparative immunity of Wool-workers from Phthisis and Scrofula.*

The evidence which I have to adduce on this first point—as on one or two others—consists principally of extracts of letters from different medical practitioners residing in the various woollen factory districts of Scotland. The writers have had the best opportunities of studying the state of health of the operatives at the wool-mills, and their concurrent testimony as to the fact of the comparative exemption of wool-workers from consumption and strumous diseases becomes only the stronger, when we consider that the observation has forced itself upon the attention of medical men practising in such different and distant districts, and in a great degree cut off from communication with each other.

Galashiels is one of those districts of Scotland in which there is a considerable number of wool-mills and wool-workers. Regarding the health of the operatives at the mills, Dr. Macdougall writes me as follows:—

"During twenty years in which I have been in practice here, I can remember but few cases of death from amongst the wool-workers. Their immunity from disease is certainly most remarkable. Before the present factory bill was passed, children of seven years of age were sent to the mills for ten hours a day, yet they throve and got healthy while so occupied. I have myself repeatedly recommended parents to send delicate children to the mills as a prophylactic, and always with the most satisfactory results. Consumption, in fact, is unknown here amongst that class. Dr. Weir, who has been in practice here for forty-three years, begs me to mention, that he can fully corroborate these statements."

In a letter on the subject, Dr. Thomson of Hawick alludes to the same point in the following terms:

"I find here the opinion is very general, or rather universal, that the employment is remarkably healthy, the workers being rarely, or almost never, known to suffer from consumption or other chest affection, such as coughs, bronchitis, or asthma. They are usually long-lived, and seldom off work on account of ill-health—certainly not on account of any chest-complaint. By certain parties long connected with our woollen-mills, I am further informed that formerly, when there were no legal restrictions as to the age at which the workers were placed in the factories, it was very frequent for sickly, ill-nourished, ill-thriving children to be put very early to the lighter kinds of work with the express view of acquiring health and a better condition of body—so general was the faith in the healthiness of the employment; and in every case very great physical improvement was the result in the space of a few months. It has been often remarked, that when several members of a family with a tendency to consumption have been put to different employments, those working in the woollen-mills have grown up robust and healthy, while the others employed as tailors or at other trades have fallen under consumption or other disease. Of this I have certainly met with several instances. Many others are on record and fully authenticated."

At Alloa, and in the district around it, are congregated a great number of woollen-mills. In regard to the health of the workers at these manufactories, Dr. Brotherston of Alloa writes me as follows:

"The popular notion in this neighborhood is, that the work carried on in these factories is conducive to health, and especially in those who are affected with scrofula. Frequently, indeed, when the villagers find their children becoming pale and falling off in flesh and appetite, they try to get them employed in these factories, with the view of restoring their health. I have occasion to be much among the people of the locality, and I know scrofula in all its different forms, and at all periods of life, to be very common here. But the young employed at the works are robust and healthy, and remarkably free from it. I enclose you an extract from the London Medical Gazette for 1842, by Mr. Thomson of Tillicoultry, upon whom I called to-day. He is a most intelligent person, and being the local surgeon under the Factory Act, his opinion may, I think, be of importance. You will find his views in the extract which I enclose, and which I cut out of a scrap-book."

To the medical gentleman named in Dr. Brotherston's letter, Dr. Thomson of Tillicoultry, I am much indebted, as will appear in the sequel, for his kind and able aid in different points of the present inquiry. In the interesting communication of his, transmitted to me by Dr. Brotherston, and originally published in the Medical Gazette for 1840, under the title of "The Influence of Woollen Manufactures on Health," Dr. Thomson makes the following remarks relative to the salutary nature of the woollen manufactories upon the health of those employed in them:

"During a residence of several years in a district where the population is much employed in woollen manufactories, no fact connected with our medical topography has impressed me more than the thriving appearance, and great exemption from disease, peculiar to the children in these mills. So remarkable is this fact, that it has become proverbial; and puny and weakly children, in a few weeks after entering these factories, exhibit the most marked improvement in physical appearance."

In a letter to me regarding the health of the people employed in wool-working, Dr. Paterson of the Bridge of Allan, remarks:

"There is but one wool factory here, to which I have been surgeon for some years. While attending to my duties as such, my attention was first drawn to the beneficial effects of the employment in such works. I used to notice children very delicate on entering the mills, become stout and healthy after a short time. It is not exactly a popular notion, that the wool-works are conducive to health, but it is very well known to the workers themselves, so much so, that it is quite proverbial amongst them. The fact is naturally becoming more generally known and believed, as is manifested by parents sending their children who may be delicate, to work in such factories. It certainly does accord with my own experience, that struma, in all its varieties, is rare in such works; and also that a curative effect is induced in those laboring under it, who may be sent to these works. So convinced am I of this, that I have often said to the parents of delicate children, that I wished I could send them to work at the wool-mills. I am not aware whether any of my medical brethren in the district hold similar views with myself on this subject."

Dr. Wilson of Inverness writes me in reference to the woollen manufactories in that district, that—

"It is a popular notion that the workers employed are peculiarly exempt from phthisis and scrofula. The proprietor and manager of the mills inform me, that they have invariably observed delicate-looking and weakly children improve after admission to the works. The manager here has been employed as a woollen manufacturer for thirty years, and cannot recollect a single case of death from consumption among the workers. He has superintended works at Galashiels, Innerleithen, &c., and in all of these places the same opinion prevails as to the exemption of the persons employed from consumption."

Dr. Lyon of Peterculter, who has the medical charge of the Garlogie Mills, near Skene, in Aberdeenshire, in a letter to Dr. Dyce, with which I have been favored, remarks:

"For the last twenty years I have had ample opportunity of knowing the amount of sickness about the establishment, and have never seen a single case of phthisis. The average number of young females is about sixty-five, all of them above thirteen years."

The preceding extracts chiefly refer to the health of the working people engaged at woollen factories situated in our villages, or larger towns, as Hawick, Alloa, &c. Dr. Joseph Bell, one of the Medical Inspectors of the factories of Glasgow, has favored me with the results of his observations upon the health of the workers at the two woollen factories which exist in his district; and his remarks are important, as showing that this form of labor seems attended with the same sanitary results in cities as in the country.

"There is no doubt in my mind," observes Dr. Bell, "that the workers in our woollen factories are more robust, florid, and healthy-looking than those employed in our cotton factories. I have seen several workers enter the wool factories, pale and emaciated, having

been previously employed in cotton mills, become, in the cour
a few months, fat, ruddy, and in every respect contrasting str
with their feeble, sickly appearance when I first examined t
One woman, who labors under chronic bronchitis, informs me
she is obliged to work in the woollen factory during the winte
spring months, as otherwise her cough and dyspnœa becom
tolerable. I have examined two other females who exhibited s
toms of incipient phthisis, but after working a few weeks i
wool-mills, these symptoms disappeared, and their general h
became excellent. So much for my own observations. Wi
gard to the statements of the managers of the wool-mills, they a
that they have never known any employed in those works die
consumption, and that those who have the usual marks of scr
—swelled glands in the neck—when they come to the mill,
soon get cured. . . . . My own impression is, that the work
these wool-mills are less likely to become attacked with scrofu
any of its forms, than the workers in our cotton factories.
events, there does not exist a doubt but that they have a much
healthy and vigorous appearance."

My friend Dr. Dyce, Lecturer on Midwifery in Aberdeen, rem
as the result of his inquiries for me on the present subject in
deen and its neighborhood:

"With reference to the extreme rarity of consumption amon
wool-mill girls—this is a fact which all here have long been fam
with, so much so, that seldom or ever is a consumptive tend
met with amongst the wool-mill girls. Any one accustomed t
the women of the cotton and wool mills, can generally tell
their appearance to what mill they belong. The cotton-mill
are generally, though healthy-looking, pale and bleached, or, a
friend Dr. Cadenhead, who, as Factory Inspector, has ample o
tunities of observing, says—they look tropical, as if they had
in a warm climate; whereas the other, the wool-girls, are rosy
fat. I am indebted to him also, for another very striking fac
corroboration of this, viz., that he has been in the habit for y
past, when in his visits he has noticed any young girls whose l
indicated delicacy, to recommend them to change the mill;
that he has often been surprised to notice the remarkable ch
that a few weeks' work in the wool-mill has produced—from
pale, pasty look, they have become healthy and plump. That
is no new observation, I may mention that so long ago as when
late Sir David Barry was here on a government inspection rel
to the Factory Bill, he remarked this difference in the looks of
women, and he seemed to think highly of the plan of changing
mill in cases of delicacy."

have most kindly received from various other medical gentle-
in other localities, as Kilmarnock, Selkirk, Innerleithen, &c. &c.,
ence analogous to that cited above in relation to the superior
:ary state of the woollen-workers in their respective towns and
icts, and the comparative freedom of these workers from
'ula and phthisis. But it seems quite unnecessary to lengthen
this communication by merely extending and accumulating
mony of the same description and effect as that contained in
)receding extracts. Various mill-owners also have favored me
letters and reports expressing, in equally strong terms, the
thy character of woollen-factory labor, and particularly the ap-
nt freedom of their operatives from attacks of consumption and
'ula. In several of these communications individual cases are
iled, in which weak and scrofulous children were successfully
by their friends into the wool factories to work there for a time,
the professed view of getting their health benefited and re-
:d by this kind of exposure and employment.

ıt while the labor in woollen factories is, as a general rule, at-
ed with the best sanitary results, it is proper to remark that (as
hall see under the next head) all departments of the work are
equally salutary. And even in the most healthy departments
ıe work the exemption is comparative only, and not absolute;
ases of tubercular consumption occasionally occur even among
nost favored class of the operatives, especially in those in whom
ıereditary predisposition to phthisis is very marked. Thus, in
ɜ notes before me regarding the health of the operatives in Mr.
mson's Mills, at Alloa, it is stated that, since the opening of
actory about four or five years ago, among the operatives em-
ed—

Only one death has occurred, and that from consumption, in
case of a boy in whose family the disease was hereditary. A
her of this boy," it is added, "entered the mill in a very delicate
of health, and was so frequently absent from illness, and so
k, that he could scarcely go through the light work given to
. He had great cough, nightly perspirations, deficient appetite,
After some time, however, he began to improve, and gradually
ıme sufficiently strong and robust to undertake heavier work in
djoining mill. He has been four years in the mills, and is now
e healthy, and never absent a day from work. Three of his
hers have died of consumption."

ne of my correspondents, Dr. Anderson of Jedburgh, writes
he has seen two cases of phthisis, and one of tubercular disease
he peritoneum, among the workers at the woollen-mills in that
n. In the last instance, however, that of tubercular peritonitis,

the patient, a young woman, had left off working at the woollen factory "for eighteen months before;" and consequently it is not a case of any importance, either for or against the woollen factory labor. The two cases of phthisis were in men, but Dr. Anderson does not mention in what department of the work they were employed; and, as already hinted, we shall see in the sequel that some departments are much less salutary than others. Dr. Anderson, however, strongly expresses the same opinion as my other correspondents, regarding the very healthy character of those employed in the woollen works in his district, and considers them "relatively free in a remarkable degree from scrofulous or tubercular diseases." In the Jedburgh dispensary books the trades of 6342 of the applicants or patients are entered. Out of this large number, Dr. Anderson finds that only 63 were workers in the woollen factories, though "nearly all of the woollen operatives are," he observes, "of the class that apply to the dispensary for medical aid."

The slight liability of the operatives at the woollen factories to attacks of disease might be ascertained with greater arithmetical precision, if the workers at the different mills were all obliged to form themselves into Friendly Societies for the relief of those of their number that were temporarily incapacitated from work by sickness. I have only, however, heard of one such Friendly Society at those mills, viz., one including all the individuals employed at the wool-spinning mills of Mr. Paton at Alloa; and I have been favored with the returns of this society during the last six years. These returns are sufficiently remarkable in relation to the present inquiry. The average numbers of members forming the society during the six years of its existence has been about 110. But during the whole six years, only 36 members have been so laid aside by attacks of disease as to require temporary aid from the sick fund. This gives an average of less than 6 per cent. of attacks of sickness annually; and even this small number would have been still smaller, had not an epidemic dysentery prevailed in 1849, counting among its number as many as 9 out of the total number of 36 invalids. But in such a Friendly Society, 6 per cent. is an excessively small annual proportion of invalids requiring sick aid. In the Deanston cotton works, a few miles from Alloa, there are 20 per cent. of attacks of disease annually among the members requiring aid from the sick fund; in the Lancashire cotton factories, there are 27 per cent. of such cases of sickness among the males, and 41 per cent. among the females; and in the Glasgow cotton factories, the proportion of sick upon the Friendly Societies also averages about 40 per cent. annually.[1] On a low average, out of every 100 members in Friendly Societies in general, about 25 require aid annually for attacks of sickness of some duration and severity. In the Friendly Society formed by the wool-workers at Mr. Paton's mill, less than six have required such aid yearly. Nor were the attacks of sickness in the 36 invalids severe in character. One of the invalided was on the sick fund for a month. The average duration of sickness in the other 35 did not exceed a week. The usual duration of sickness in such societies averages, in Mr. Farr's returns, from two to three weeks, and in some returns reaches a longer period.

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[1] For these, and other still higher returns, see an article by Mr. Farr, in McCulloch's Statistical Account of the British Empire, vol. ii. p. 555.

On the question of the relative sanitary effect of wool factories upon the health of those employed in them, I have made several attempts, but without much success, to acquire evidence also of another character, viz., the amount of *mortality* among them from disease. To obtain this, however, it would be requisite to have statistical returns showing the absolute number of wool-workers in particular towns or districts, and the annual proportion of deaths among these wool-workers, and the mortality existing in the population in these same towns or districts among the inhabitants not engaged in the woollen factories. The total want, however, in Scotland of any authorized registration of deaths, has proved, in almost all cases, a complete barrier to such investigations. In fact the only trustworthy evidence of the kind which I have obtained has been drawn up for me by the kindness and zeal of Dr. Thomson of Tillicoultry. This gentleman has himself carefully superintended the parish register of deaths at Tillicoultry for the last ten years, and has laudably devoted great attention to the sanitary and other statistics of his locality. The average population of Tillicoultry during the past ten years has been very nearly 4000; the numbers being 3211 in the census of 1841, and 4682 in the census of 1851. Of this population about 1500 have, on the average, been employed at the wool-mills; the exact number of wool operatives in the census of 1851 was 1790. Now the total deaths in Tillicoultry during the last ten years, from 10th June, 1843, to 10th June, 1853, amount to 961, giving an annual mortality among the general population of about 1 in 41. Out of the 961 deaths, only 82 occurred among the wool-workers; or, in other words, the annual mortality among those engaged at the wool-mills did not exceed 1 in 180, whilst the annual mortality among the general population was as high as 1 in 41.

Dr. Thomson has also sent me similar statistics, kept during the last five years, relative to the neighboring parish of Alva. In this parish, out of a population of 3000, about 1200 are engaged in the woollen factories. During the last five years, 320 individuals have died in Alva, giving an annual mortality of 1 in 46. Out of the 320 deaths during the above period, 14 only have occurred among the wool operatives; or, in other words, the annual mortality among the wool operatives has not exceeded 1 in 400, while the annual mortality among the general population of the place has amounted to 1 in 46.

Perhaps when the present subject is investigated by the aid of more accurate and more extensive data than I have been able to procure, it may turn out that the beneficial effects of labor in the woollen factories will be found to be traceable, not so much to the direct prevention of phthisis and scrofula, as to the generally improved health of the operatives, by which these and various other diseases destructive to life are in a remarkable degree averted. In the meantime, taking it for granted, as all the preceding evidence shows, that the work in woollen factories preserves those employed in them in a remarkable degree from attacks of scrofula and consumption, let us proceed to inquire what are the special conditions or circumstances connected with woollen factories, to which we may ascribe this beneficial result.

II. *On the Cause of the Comparative Exemption of Wool-workers from Phthisis and Scrofula, etc.*

The salutary effects of the woollen factories upon the health of those engaged in them, might *a priori* be supposed to depend, and indeed has been attempted to be explained, on different principles, as, 1. Their hygienic state has been averred to be possibly the simple result of the healthy and favorable age of the operatives employed; 2. Their exemption from tubercular and other diseases has been ascribed to the sanitary nature of the factory labor itself. Let us first examine these two supposed explanations before proceeding to show the truth of a remaining *third* proposition, viz., that the relative immunity of wool-workers from disease is the effect of salutary specialities connected with the manufacture of wool, and particularly of the abundant use of oil in the wool-mills.

1. It has been suggested to me by different medical friends, that the comparative exemption of wool-workers from scrofula and phthisis was perhaps owing to the *ages* of the operatives, and especially the large proportion of young among them—rather than to anything directly hygienic in the employment itself. The idea forms a just and fair objection, and one certainly demanding inquiry. But the following circumstances will prove that in itself this suggestion affords no explanation of the relative freedom of the wool-workers from consumption and struma, but the reverse.

The Factory Act strictly prohibits any person from being employed at woollen mills, or other forms of factory labor, under the age of thirteen. In order to obtain a view of the ages of the operatives at the woollen factories, Dr. Thomson and Mr. Paton have noted for me the time of life of all the workers employed at four of the largest mills in Tillicoultry and Alloa. The absolute number of individuals employed in these four mills amounts to exactly 1100; and the following table shows the relative ages of these 1100 persons in quinquennial periods up to thirty years, and in decennial periods after that time—

Ages of 1100 *Operatives engaged in Four Woollen Mills at Tillicoultry and Alloa.*

Under 16 years of age,	163	30 and under 40 years,	185
16 and under 20 years,	282	40 and under 50,	101
20 and under 25,	185	50 and upwards,	54
25 and under 30,	130		

Now if we compare this table of the ages of the woollen operatives with the ages of persons dying of scrofula, and phthisis, it will appear that in itself the time of life of the operatives is such as strongly tends to predispose them to fatal attacks of tubercular disease, rather than to protect them from its ravages. In the latest, and perhaps most valuable, work on Tuberculosis which has yet been published, that of Mr. Ancell, the author gives, in quinquennial periods, a table showing the relative ages of those who die of tubercular disease out of every 1000 individuals who perish under it.[1] Excluding, as not connected with our present comparison, the first two quinquennial periods of life (from 1 to 10 years), it appears that during the remainder of life the greatest number die of tubercular disease from 10 to 15; the next greatest number from 15 to 20; the next from 20 to 25, &c., in the following proportions. Out of every 1000 deaths from tuberculosis, 179 occur among persons from 10 to 15 years of age; 123 among persons from 15 to 20 years of age; 97 from 20 to 25 years of age; 84 from 25 to 30 years of age; and so on in an uninterrupted and rapid decrement from 25 years onwards. When we thus compare the ascertained frequency of deaths from tubercular disease at different ages, with the actual ages of persons employed at the woollen factories, it follows that the general time of life of the operatives is such as gives a decided tendency to tubercular disease, instead of accounting, as has been suggested, for their comparative immunity from phthisis and scrofula.

2. The influence and peculiarities of *factory labor* upon the operatives have been also suggested by some as the probable explanation of the exemption of wool-workers from tubercular and other dis-

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[1] Loc. cit. p. 406.

ease. The effects of factory-labor upon the health of those employed in it have, as is well known, been much inquired into during the last twenty years, and in respect to these effects very opposite opinions have been upheld. It has been maintained by various authorities, that mill-working had a good effect upon the health of those employed, in consequence of the regularity of the hours, the constant, yet never violent, muscular exertion necessary for it, the generally superior pay, and consequent good food and maintenance of those engaged in it, &c. Without, however, entering into these questions, let me merely remark, that though the evidence adduced before the Government Commissioners and elsewhere, regarding the health of the operatives at the cotton factories of England and Scotland, did not perhaps prove that the workers at these mills were particularly and specially liable to tubercular disease, as was at one time supposed; yet it will generally, I believe, be admitted that the same evidence has as indubitably shown, that the nature of the factory or mill-working occupation does not in any degree *exempt* those engaged in it from consumption and scrofula. In other words, the multiplied testimony adduced regarding the health of the workers at the numerous cotton factories of this country, shows that the mere nature of the work at the mill produces no immunity in those employed from consumptive and tubercular affections; and consequently it follows, that if, in any variety of mill-working, such an exemption was found, this exemption could not be ascribed to the mere character of the factory-labor or mill-work itself. And when we find that, while the cotton mill-workers are not free from consumption and struma, the wool mill-workers are comparatively exempt, we must evidently search for the cause of this difference and exemption in some peculiarities connected with the wool-working itself.

3. The great difference and peculiarity in woollen mills, as compared with cotton mills, consists in the fact, that while the hours, the occupation, &c., are much the same in each, in the woollen mills a very large quantity of *oil* is used, and the bodies of the workers are brought in various ways freely in contact with it. It is, I believe, in this one item that the great difference between cotton-working and wool-working consists; and it is to this material, the oil, as freely used in some of the processes of the wool factories, that the operatives themselves universally, and, as I believe, properly, attribute the salutary nature of their occupation.

In corroboration of the truth of this popular belief that the good effects of the woollen factory labors are ascribable to the oil employed, I have to state two points, viz., that—

First, Similar exemption from scrofula and consumption is observed in other classes of workmen whose employment brings them in the same way freely into contact with fats or oils, as tallow-chandlers, oilmen, &c.,[1] and—

Secondly, In the wool factories the degree of exemption among the operatives themselves is by no means equal in all the processes of the manufacture, but is regulated by the more or less "oily" nature of the departments of work in which they are engaged in the mills; so that they in general markedly improve in appearance and health when set to work at the more oily processes; and often as markedly decline after leaving them.

In order to understand this second proposition, it is necessary to remark that in converting wool into different fabrics, the material requires to pass through a great variety of processes. Authors, such as the writer of the essay on "Woollen Manufactures" in the Encyclopædia Britannica, enumerate above twenty different processes or stages in the manufacture. It is only in some of these processes that oil is used, or that the workers at least come much in contact with oil. The wool first undergoes the processes of sorting, cleansing, picking, and sometimes of dyeing before it is mixed with oil at all; so that the operatives engaged in these earlier departments of the manufacture are not in any way directly exposed to any hygienic influences that may be derivable from the use of oil. Before, however, the manufacture can proceed further, the wool requires to be freely and intimately saturated with oil in order to allow it to pass through the further technical processes of willying, scribbling, carding, slubbing, spinning, &c. Afterwards the wool is by a different set of operatives woven into the required fabrics, and subsequently the woollen cloths pass through the remaining processes of scouring, fulling, teazing, pressing, &c. The weavers and those operatives engaged in these latter departments, come in contact with the wool after it is again in a greater or less degree deprived of its imbibed oil. Thus it is only a proportion of workers in wool-mills that are fully exposed to the action of the oil employed. The actual proportion of individuals engaged in these more oily departments of the labor, varies also in different mills, according as the mills embrace all the departments of manufacture; that is, according as they are both spinning and weaving, &c., mills, or spinning mills only. Dr. Thomson calculates that out of 983 workers employed in three of the largest complete mills in Tillicoultry, 256 were engaged in "oily," and 727 in "not oily" departments of the work. In the large Wilton Mill belonging to Messrs. Dickson and Laing, 113 hands are reported to me as employed in the oily departments, and 650 in the non-oily process. I have similar returns from some other mills at Galashiels, &c., so that we may perhaps consider that only about one-third of the operatives in our large woollen factories, where weaving and every other department is carried on, are truly influenced and protected by the oil employed. In some factories, however, the wool is only, as I have stated, carried on as far as the process of spinning; and in these nearly the whole mill-workers are consequently engaged in "oily" occupations.

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[1] Dr. Beddoes believed also that tradesmen, such as butchers, who had to work among *animal* matters, whether of an oily nature or not, would be found free from consumption. Later observations have not confirmed this view. As a general law, it is now well established that those following out-door occupations, such as those of the butcher, in a great measure are much more free from consumption than those following trades conducted within doors. But still later observations appear to show that the butcher by no means enjoys in any remarkable degree the immunity from consumption claimed for him by Dr. Beddoes. Among 4358 patients laboring under phthisis treated as in and out patients at the Brompton Consumption Hospital, from 1842 to 1848, 15 were butchers, or about 1 in every 240 of the patients who applied, belonging to this trade. On the other hand, out of 2179 out-patients (excluding cases of phthisis) relieved at a general hospital, 4 were butchers, or 1 in about every 500 belonged to this trade.—*See Ancell on Tuberculosis, Tables*, p. 497–98.

That the divisions of the wool-workers engaged in the more oily departments of the manufacture are in a marked and special manner exempt from disease, is a point to which several of my correspondents have particularly alluded. Thus, after referring to the relative health of the weavers, stocking-makers, scourers, and others connected with the woollen-trade of Hawick, Dr. Thomson observes:

"It is admitted on all hands that only the mill-workers proper, so to speak, are supposed to enjoy from their trade any special immunity from disease; and these all work among oil in feeding the machinery, slubbing, &c. The other persons composing the working force of a woollen-factory, such as weavers, warehouse people, and the like, though certainly a sufficiently healthy class, are not considered more so than the average of workmen employed in other active engagements, unless the fact be taken into account that most of these persons may previously at one time or another have come under the 'oily' influence as mill-workers."

On the same subject Dr. Thomson of Tillicoultry, in one of his letters to me, observes:—

"My opinion is most decided, that those woollen-workers most engaged in the oily parts of labor exhibit the most marked health and exemption from disease. These persons are called *feeders* and *piecers*, and of these two classes the *feeders*, who are most oily, experience the greatest benefit. I consider also *spinners*, as more concerned in the oil, to have the advantage over weavers, whose materials are *freed* from oil. . . . . The fine appearance," he adds, "of the young workers, their rapid improvement when set to work in oil, their declension when they leave it, the rareness of phthisis and scrofula in them, the notable relief which several patients under mesenteric disease experience at the mills, their improved aspect, and the abeyance of the disease, leave no doubt on my mind that the oil is the salutary agent.

In order to establish the preceding points with still greater certainty, Dr. Thomson has latterly weighed some of the young workers when first beginning the more oily employments, and he has sent me the following note of one of his first observations:—

"Jean P., æt. 16½, a *feeder* in the mill of Messrs. Harrower and Co., Alva. Four months ago she was carefully weighed, and was only 7 stones weight. Two months ago she began to work at the teazing machine,' and now she has got so much stouter as now to weigh 8 stones, 3½ lbs. This girl was most carefully weighed by myself, Mr. Harrower, and the managers, so that no mistake might happen. She had on the same clothes, &c., as formerly."

As an instance of the declension in weight, following the withdrawal of an operative from the more oily departments of the work, Dr. Thomson describes to me a case which I will cite in his own words:—

"Janet R., aged 17½ years, fed the teazing machine before Jean P. (the preceding subject). When doing so, she became so fat as to be a bye-word. She does not know how much flesh she gained, but when she left off two months ago, her weight was exactly ten stones. I weighed her to-day, and find that she has lost seven pounds. She is now working at a different process—not oily."

A careful inquiry upon an extended scale, such as is begun by Dr. Thomson, of the relative weight of a large number of operatives for some time after engaging in, and for some time after leaving off, any of the more oily departments of the woollen manufacture, will probably ultimately lead to some interesting details and results.

III. *The Quantity of Oil used in the Mills in the course of the Woollen Manufacture.*

In the woollen manufactures the wool is besmeared and saturated with oil in order to render its fibre sufficiently pliant and flexible for the purposes of felting, carding, spinning, &c. A large quantity of oil is employed in order to gain these objects. The quantity is regulated by the kind and quality of the wool. I have been assured, in more than one quarter, that it is used in most mills in the proportion of from half a pound to one pound weight of oil daily for every workman engaged. Mr. Paton of Alloa, in computing for me the proportion of oil used in his mill to the wool employed, calculates that, on an average, about one pound of oil is required for every six or eight pounds of wool. At Galashiels, Dr. Macdougall finds that about one pound of oil is commonly computed to be required for every five or six pounds of wool; and during the course of a single year, some of the larger woollen factories use from 3000 to 6000 gallons of oil.

In most of the manufactories the species of oil used is olive oil, or, as it is generally termed, Gallipoli oil, from the district in Italy whence it is procured. In some kinds, however, of woollen work, other oils are employed, as whale or sperm oil. These latter are principally employed in mills where only the coarser woollen fabrics are manufactured. The great expense of the oil has of late induced some mill-owners to try the use of milk and other cheap materials.

IV. *By what Mode or Channels may the Oil enter the System of the Operatives.*

In all those apartments in woollen factories in which the fabric is in its "oily stages," the atmosphere is more or less loaded with oil particles, as ascertained at once by the sense of smell on entering such rooms, and by the oily deposit which covers the machinery, the furniture, and indeed every exposed point and surface. Objects hung up or scattered about such apartments speedily become coated with oil; and those parts of the clothes and bodies of the operatives that are exposed during work to direct contact with the oily wool, as well as those portions that are not so, thus become rapidly greasy and covered with it.

Under such circumstances, we may suppose the oil to enter the bodies of the operatives by one of two channels, namely, either—1, by inhalation through the mucous membrane of the lungs, or, 2, by cutaneous application and absorption.

That inspired gases and vapors readily pass into the circulation through the pulmonary mucous membrane, is granted by all. Experiment also shows that some solid substances, when heated and vaporized, as opium, iodine, and mercury, can be made to enter the system through the same channel. We know also that volatile oils, as turpentine, juniper, copaiba, &c., when breathed, reach the circulation, and are subsequently very speedily found in the urine. It is probable also, that since suspended particles of the olive and other fixed oils used in woollen factories are capable of floating in the atmosphere, and, consequently, of being inspired along with it into the lungs, that they may be absorbed, like volatile substances, by the pulmonary mucous membrane, and in this way produce some of their salutary effects. But direct and indubitable proofs of this mode of entrance of fixed oils into the body are still wanting.

n all likelihood the more important, if not the only channel by
ich the oil gains access to the system, in the case of the woollen
ratives, is by its cutaneous application and absorption. As al-
dy stated, the dresses of the operatives soon become imbibed and
urated with oil, and their hands and arms are constantly smeared
h it during their working hours. Medicated substances thus
lied, with sufficient freedom and friction upon the skin, pass
the system. Even "metallic preparations"—to quote the words
Professor Müller—"rubbed into the skin have the same action
when given internally, only in a less degree. Vegetable matters
," he adds, "if soluble, or already in solution, exert their pecu-
effects through the medium of the skin."[1] Under imbibition
l friction, oil, the agent in whose cutaneous absorption we are
cially interested, readily penetrates through dead epithelial struc-
es, and when the composing particles of the skin have already
ome hardened and condensed after its removal from the body.
us, in the process of leather-making, it is found that large quan-
ies of oil are capable of being rubbed into the tissues of the dead
ins of our domestic animals. In the living human subject, we
n readily gain clinical proof of the facility with which warm oil
n be rubbed into the skin by watching the rapidity with which
e liquid disappears from, and is absorbed from the surface of
ose who use oil-frictions, and particularly in the case of such per-
ns as have followed the practice for a considerable time, and in
om the power of cutaneous absorption is hence increased. Be-
les, we have a further proof of this cutaneous absorption of oil,
the fact that those who use oil-frictions show exactly the same
ecial constitutional effects from this mode of introducing it, as
ose who introduce oil into the system by swallowing it.

. *Would Systematic Oil-inunction, as a Medicinal Measure, prevent or cure Tubercular Disease.*

In the woollen factories the exposure of the operatives, and par-
ularly of the younger part of them, to the inunction and inhala-
on of olive and other oils during the day hours of work, is appa-
ntly a decided and successful means of averting, if not of curing
bercular disease, and of sustaining a high standard of general
alth. But if the free external application of oil during a few
urs of the day has this effect in these operatives, the same good
fect may reasonably be expected to follow if the same free external
plication of oil is methodically made to others, not engaged in
ctory-labor, during an equal length of time, as during the hours
night and sleep. Provided, indeed, our reasoning be not falla-

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[1] Elements of Physiology, vol. i. p. 251.

cious, we ought to find, in a return to the habit of daily or nightly oil-inunction—as practised by the ancients—one important means and principle by which tubercular affections may be sometimes arrested, but far oftener prevented in those delicate families and individuals who are predisposed to this very frequent and very fatal form of human disease. If with them the inunction of the cutaneous surface with oil instead of its ablution with water could be generally introduced as a daily or nightly habit, perhaps phthisis and scrofula would lose much of their dreadful pre-eminence in our mortality bills. In matters medical, as in other practical matters in life, the prevention of evils is always simpler, safer, and surer than their cure. We have every reason to believe that the free hygienic use of oil would prevent the development of tubercle; and after it is developed the free use of it is a great, if not our greatest curative means in those forms of the disease that are curable, as in scrofulous-swelled glands, in scrofulous bones and joints, in scrofulous ophthalmia, &c. Indeed, modern physicians are almost all of opinion that even after that most fatal type of tubercular disease—pulmonary phthisis—has actually established itself in the body, the principal curative measure is the introduction of oil into the system.[1] Cod-liver oil was first used in phthisis by Haenkel in 1833, and when speaking of it in 1849, Dr. Williams of London, the highest authority I could quote on such a subject, declares that in his opinion, and according to his extensive experience, it "is more beneficial in the treatment of pulmonary consumption than any agent, medicinal, dietetic, or regiminal, that has yet been employed." Other oils have, according to the testimony of Thompson, Hall, Duncan, and others, the same beneficial effects on the body when they do not disagree and bring on dyspepsia or diarrhœa. The principal advantage of cod-liver oil perhaps is that when an oil requires to be swallowed, it is less liable to produce stomachic or intestinal disturbance than any of the various fatty or oleaginous substances that have been long prescribed by physicians in tubercular cases. Occasionally, however, cod-liver oil does disagree to such an extent that it cannot longer be borne; to some persons it is too distasteful to swallow above a few days or weeks; and perhaps not many would be induced to use it continuously and constantly as a medicine or as a prophylactic for months or years.[2] The introduction of the requisite oil into the system by cutaneous inunction is so far not liable to these objections, especially if the oil employed be heated, bland, and inodorous. In external oil anointing I have usually employed common olive or salad oil, and sometimes cocoa and neat's-foot oil. Bauer of Tübingen, and others, have used cod-liver oil for external inunction, but its irritating qualities, smell, &c., render its employment very disagreeable.

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[1] For its history, &c., see Dr. Bennett's well-known "Treatise on Cod-Liver Oil," or De Jongh's Essay, "L'Huile de Foie de Morue," p. 1 to 24.

[2] To overcome the scrofulous or rheumatic dyscrasia, the use of cod-liver oil ought (says De Jongh) to be continued without interruption for at least one entire year.—Essay, p. 164. In phthisis it should (observes Professor Schroeder Van der Kolk) "be taken early and continued during several years."—Ibid. p. 220.

I have no intention whatever of entering upon the very debated and very debateable questions of the morbid state of the blood, fluids and solids, in tubercular disease—and of the probable mode or modes in which the introduction of oil into the system rectifies and corrects the tubercular predisposition and action. It is enough for my present purpose to have it allowed—what few will deny—that cod-liver and other oils, when swallowed, have a controlling power over the disease, and in favorable cases show evidence of their beneficial action by gradual and marked improvement in the attendant morbid symptoms, by a gain of strength, and very often by an appreciable increase of weight in the body of the patient.

In tubercular and other cases, these effects are sometimes as distinctly, though perhaps not so frequently, obtained from the external inunction of olive oil, as by the swallowing of cod-liver oil. I have seen a similar amelioration in the constitutional and local symptoms of the malady, and a similar improvement in the general health occur under the one as under the other practice; showing us that, practically, one may, if necessary, be sometimes temporarily substituted for the other; or both employed at once when there is no counter-indication to their combined and more certain action. The restoration of the functions of the skin, and the suppression of hectic perspiration more rapidly and surely follows external inunction. The increase in the weight of the body, which has been so much and justly insisted on as a favorable sign under the internal use of cod-liver oil, is occasionally most marked under the external use of olive oil. In a case in which this increase was specially watched, under external oil-inunction alone, the patient, who was carefully weighed, in forty-two days increased 24 lbs. in weight, a rate nearly as high as any, I believe, ever observed to occur under the employment of cod-liver oil internally. This patient's stomach could not retain cod-liver or other oil in any form that was tried. I have seen a child two years old increase in weight an ounce a day, for eight weeks, under assiduous oil-inunction, its stomach having for some time previously rejected oils, and most other food, when swallowed. And in the external as in the internal use of oil, the increase of weight obtained is often greater than the mere weight of oil introduced into the system. The oil hence evidently leads to increased weight, not only by its own simple addition or assimilation, but apparently by furnishing to the constructive masonry of the body an article, the previous want or defect of which prevented the other materials of nutrition from being duly built up and assimilated. Dr. Walshe and others have published cases in which the swallowing of three or four drachms of oil daily, was followed by an increase in the weight of the body daily, of three or four ounces.

VI. *Diseases and Circumstances in which Oil-rubbing is indicated.*

The preceding remarks apply to oil-inunction as specially useful in scrofulous or tubercular diseases of all kinds and forms. Let me add, that in various affections more or less allied, and frequently complicated with the scrofulous diathesis, it is also often most beneficial, as in rickets, tabes, laryngismus, tinea, impetigo, &c. In inanition, by whatever cause produced, and particularly when dependent on mal-nutrition or mal-assimilation, and combined with a dry or disordered state of the skin—the practice is often most advantageous. In the marasmus of children, I have more than once seen oil-inunction succeed, and apparently save life, when all other means and remedies had utterly failed. When the body is much reduced by morbid eliminations, or by acute or chronic disease—as after the dysentery and diarrhœa of children, oil-inunction sometimes forms the best restorative. In rheumatism and in the chorea of the young, when accompanied by debility, it is often serviceable. The practice itself guards weak constitutions against the effects of changes of temperature and weather; and the feeling of cold and tendency to catarrh and chilliness, attendant upon various debilitated states, is sometimes entirely arrested and averted by oil-inunction. For it is the property of oils—long ago remarked Pliny, "*tepefacere corpus et contra algores munire.*"

Anointing the skin with oil has been declared by Baldwin, Jackson, Macgregor, Green, Forbes, McAdam, and others, to act as a preventive to the contagion of plague. Drs. Craig and Gilders aver that it occasionally prevents also the production of fever from malarious poison. Dr. Macdougall, Dr. Thomson of Tillicoultry, Dr. Thomson of Hawick, and other correspondents, have stated to me, that in epidemics of scarlatina, measles, and cholera, the woollen workers under their care have in general almost entirely escaped—a fact of great practical moment, if confirmed by further observations, inasmuch as in practice it could be easily applied in epidemic seasons, and under exposure, as a simple, if a successful, means of prophylaxis.

Let me add, that the external, like the internal use of oil, is apparently, in general, much more efficacious in the case of children than of adults—the skin of youth having apparently far more absorbing power than is left to it in more advanced life.

VII. *Principal Rules for External Oil-inunction.*

he oil selected ought, as I have said, to be bland and inodorous, olive or salad oil; and the more important directions which re- e to be given by the physician to the patient in reference to the rnal use of it, are probably the following:—

The oil should be applied moderately warm. Its application hus rendered far more agreeable to the feelings of the patient; danger of chills is avoided; and the act of absorption is increased n elevated temperature. 2. A considerable amount and dura- of friction should be used either by the patient or his attendant, y both, in order to rub in the oil as much as possible, and thus note the completeness of its absorption. 3. The oil and friction ld be applied to the whole cutaneous surface of the trunk and emities, but especially to those parts of it where the skin is thin the function of absorption greatest, as the sides, the flexures of limbs, the insides of the thighs, &c. 4. The average quantity il requiring to be used at each inunction, is about a large wine- sful. 5. In cases in which it is an important object to intro- e the oil into the system as freely and rapidly as possible, the ction of it may be practised twice or oftener in twenty-four rs, especially with children; but the best time for a single daily nunction is immediately before retiring to bed, as the imbibi- of any free oil left on the surface may afterwards go on during night; and to save the bedclothes, the patient should sleep in ess of flannel, linen, or other material that stretches beyond the 6. In order to maintain the full absorbing action of the skin onjunction with the practice of oil-inunction, occasional warm ging or bathing of the whole cutaneous surface with tepid er, or with a weak solution of soda in water, should be employed, er immediately before an inunction, or several hours subse- ntly to one. 7. It is to be remembered that the cutaneous ab- tion of oil is usually, though not always, comparatively more and difficult, and hence the practice itself is so far more dis- eable for two or three weeks after the inunction is first begun subsequently; and consequently, that less oil disappears, and e friction is required in the beginning of the practice than after- ds.

III. *Answer to Objections to Oil-inunction on the score of Cleanliness, &c.—Oil-anointing by the Ancients for Luxury, &c.*

he external use of oil is sometimes objected to as a measure in f disagreeable in thought as well as in deed, and particularly its seeming want of cleanliness. Yet the same individuals who oppose on this score the anointing of the skin with inodorous oil, as a means of preserving health and curing disease, will be often found anointing their own hair, &c., with wretched scented oils and pomatums for the mere purposes of ornament and vanity. In this, as in other matters, our prejudices are sometimes nothing but ridiculous inconsistencies. Patients who have not perhaps, by any sponging or bathing, rubbed and cleansed the surface of their bodies and limbs for weeks with water, will occasionally rebel at first against the idea of rubbing and cleansing them freely and fully every night with oil, as something that is, forsooth, uncleanly. The actual principal objection to the practice is its tediousness, and the daily dedication to it of the ten or fifteen minutes that is usually required to perform the inunction fully and perfectly. In relation, however, to the preservation or restoration of human health, from one of the most dreaded and deadly forms of human disease, this surely is an argument of no very great weight. Most of those who have employed warm oil-anointing find that, after a short time, while it promotes cleanliness, sleep, &c., it is a practice that soon becomes the very reverse of unpleasant. As with the employment of the shower or cold bath, &c., I have heard patients declare that they found it as difficult to relinquish the practice of inunction, when at last accustomed to it, as they had found it difficult to adopt in the first instance. Some Asiatic nations at the present day use oil-inunction, as various ancient nations used it, not only as a means of strengthening and refreshing the body, but as a grateful and esteemed luxury. The Bible contains various well-known allusions to the practice among the Jews. The great extent of the habit and luxury of external inunction among the ancient Greeks and Romans, both with and without the bath, is described in all our antiquarian works upon the daily life of these people,[1] and is alluded to by many of the classics, from Hesiod and Homer, down to Terence and Tibullus, Persius and Plautus. Seneca, who in one of his epistles (Ep. liv.) speaks of refreshing his own body by inunction (*corpus unctione recreavi*), declaims, in another epistle (Ep. lxxxvi.) against the practice being repeated, as it was by some, twice or thrice in one day (*parum est sumere unguentum, ni bis die terque renovetur*). "The human body receives," says Pliny, "vigor and strength from every kind of oil (*omni autem oleo, corpus vigorem et robus accipit.*)" "There are two liquids," the same author elsewhere observes, "very grateful to human bodies, namely, wines within and oils without (*duo sunt liquores corporibus humanis gratissimi; intus vini, foris olei*)."[2]

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[1] See the works of Smith, Becker, Ramsay, &c.

[2] Hist. Natural. lib. xxiii. cap. 4, lib. xiv. cap. 22.

PART VII.

ANÆSTHESIA.

SECTION I.

ANÆSTHESIA IN SURGERY.

> "The multiplied experiments to prevent pain in surgical operations, which bears so delightful a testimony to the humanity of their authors, will certainly, in the course of time, be crowned with success."—*Marx's Akesios—Letter to Herman Boerhaave.*

CHAPTER I.

MERE OPINIONS AND PREJUDGMENTS NOT SUFFICIENT TO SETTLE THE QUESTION OF THE PROPRIETY OR IMPROPRIETY OF ANÆSTHETIC AGENTS: ILLUSTRATION FROM THE HISTORY OF VACCINATION.[1]

DURING the latter half of the last century, 30,000 individuals were computed to die annually of small-pox in England.[2] From the official returns of the Registrar-General, it appears, that in England and Wales the number that perish annually of this same disease at the present time is reduced to less than 10,000.[3] In England alone,

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[1] From Edinburgh Monthly Journal of Medical Science, Sept. 1847, p. 145.

[2] Dr. Gregory observes, "The total deaths by small-pox throughout England were estimated at about 45,000 annually."—*Cyclopædia of Medicine*, vol. iv. p. 402. Dr. Haygarth calculated the annual number of deaths from small-pox to amount to 38,000 in 8,000,000 of inhabitants.—See the data of his computation in his "Sketch of a Plan to Exterminate small-pox," 1793, p. 144. In making the various computations regarding vaccination in the text, I have, in order to avoid the possibility of error, kept all the calculations considerably below the ascertained data.

[3] During the five years from 1838 to 1842 inclusive, there died, on an average 8893 individuals yearly of small-pox. In 1842, only 2715 died.—See Sixth Annual Report of the Registrar-General, p. 514. Formerly, 1 in about every 250 of the general population died

therefore, the absolute mortality from small-pox is less by twen
thousand a year than it was half a century ago. If a similar rate
reduction in the number of deaths from small-pox holds good—
we have every reason to believe is the case—in the other kingdo
of Europe, then, out of the 220 millions of people that inhabit t
quarter of the globe, 400,000 or 500,000 fewer die of small-p
than, with a similar population, would have died from this mal
fifty years ago. In other words, according to this rate of comp
tion, there are now preserved from death by small-pox in Engla
during the currency of a single half century, a number of li
greater in amount than the whole existing population of Wa
There are preserved in Europe, during the same period, a num
of lives greater in amount than the whole existing populatio
Great Britain.

For this mighty triumph of medicine over one of the most loat
and dreaded forms of human disease and death, science stands
debted to the inestimable discovery of Dr. Jenner;[1] and ev
medical man is ready to allow, at the present day, that his discov
is not less remarkable in consequence of its gigantic results a
amazing success, than in consequence of the singular simplicity a
safety of the means with which that success is obtained. For
one now dreams of ever expecting any deleterious or danger
consequences to ensue from vaccination; and, indeed, the perfor
ance of it has been mainly or entirely conducted, in some distri
by non-professional individuals—by the priest as well as by
physician—by the nurse as well as by the surgeon.

annually of small-pox; now, only 1 in about every 1700. In England, the registrati
every birth and every death is properly enforced by law. If the registration of the v
nation of each child were enforced as rigorously as the registration of its birth, much
ease, and many thousand human lives would thus undoubtedly be saved annually in G
Britain. Surely it is a subject well worthy of the attention of a benevolent legislature.
see the good effects of such interference in other European states. For, whilst in Eng
(the native country of Jenner), still 1 in every 1700 inhabitants dies annually of small
in Austria, 1 in 4800 dies of this disease; in France, 1 in 11,000; and in Sweden, only
27,000. On the great extent of the number of individuals in society who remain unv
nated, see some excellent remarks by Dr. Stark, in the Edinburgh Medical and Sur
Journal, No. 161.

[1] In answer to those who have affected to doubt entirely the utility of physic and p
cians, medical science may proudly point to the results of vaccination. During the
European wars connected with and following the French Revolution, it has been calc
that five or six millions of human lives were lost. In Europe, vaccination has already
served from death a greater number of human beings than were sacrificed during the c
of these wars. The lancet of Jenner has saved far more human lives than the swo
Napoleon destroyed. On these devastating European wars England lavished million
money, and freely bestowed honors, peerages, and heavy annual pensions upon the
who were most successful in fighting her battles and destroying their fellow-men;
grudgingly rewarded Jenner with thirty thousand pounds for saving thirty thousand of
subjects annually.

Yet at the time of Dr. Jenner's first public announcement of vaccination in 1798, and for many years subsequently, the proposal of bstituting vaccine for variolous inoculation was encountered by rious members of the profession, with incredulity and ridicule, d direct and determined opposition. The measure by which he ught medical science to save annually from death, thousands of man lives in England, and hundreds of thousands throughout rope, was, on its first introduction, bitterly denounced and deied in different quarters, its effects doubted, and its own safety d propriety strongly and strenuously called in question.

Dr. Squirrell earnestly and publicly supplicated his Majesty eorge the Third to suppress "the destructive practice of vaccine oculation throughout his dominions."[1] "It ought," observed rofessor Monro of Edinburgh, "to be prohibited by Act of Parliaent."[2] "The College of Physicians have," exclaimed Dr. Moseley, a duty to perform, and I trust this business will not escape them,"[3] thers, despairing of interference on the part of the King, Parliaent, or Colleges, appealed to the people themselves. "It would," id Dr. Brown, "undoubtedly be downright madness to imagine ey will condescend to encourage it."[4] The Anti-Vaccinarian Soiety called upon the public "to second their efforts in supporting e cause of humanity against cow-pox injuries," and besought their id to suppress "the cruel despotic tyranny of forcing cow-pox isery on the innocent babes of the poor—a gross violation of reliion, morality, law, and humanity."[5]

Frightful, and even fatal consequences were boldly averred to be he direct and immediate results of vaccination.

Deaths from cow-pox inoculation were published in the mortality ills of London.[6] "I have," alleged Dr. Moseley, physician to the helsea Hospital, "seen children die of the cow-pox without losing he sense of torment even in the article of death."[7] Dr. Rowley, hysician to the St. Marylebone Infirmary, professed to publish true ccounts of fifty-nine deaths from "cruel vaccination;" and added, hat "when humanity reflects" on these and (to use his own words) "a great heap of victims diseased for life, and likely to transmit to osterity, for ages, beastly chronic diseases,[8] it is enough to freeze he soul with horror." And "it is," he exclaims, "the duty of honorable men in the medical profession to alarm mankind of the impending danger of vaccination; to warn society of the multifarious evils that await them in the form of this mild catholicon, of a sweetened potion that carries fatal poison in all its destructive particles."[1] He elsewhere eloquently declaims against "affectionate parents being robbed of their serenity, and the minds of tender mothers being wrung with eternal suspense," "whilst a few projectors or visionists are pursuing their deleterious projects on human victims," and perpetrating a "dangerous innovation which so many fatal facts illustrate."[2]

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[1] Observations on the Pernicious Consequence of Cow-pox Inoculation. 2d ed. London, 06, p. vi.

[2] Edinburgh Medical and Surgical Journal, vol. xv. p. 64.

[3] A Treatise on the Lues Bovilla. 2d edition. London, 1805, p. xiv.

[4] A Letter in Reply to the Surgeons of the Vaccine Institution. Edinburgh, 1809, p. 96.

[5] See their Address of 1806 in Blair's Vaccine Contest, p. 56.

[6] Mr. Blair's Pamphlet, p. 95.

[7] Treatise, p. 95.

[8] Cow-pock Inoculation; with the Modes of Treating the Beastly new Diseases produced Cow-pock. 2d edition, 1805, p. 128.

Mr. Lipscomb urgently maintained, in an essay on small-pox inoculation published in 1805, that cow-pox, the "new scourge industriously dispersed to afflict the children of men," is "sometimes fatal of itself, and that the diseases introduced or brought into action by it may be also sometimes fatal, and can never be completely guarded against."[3] One author had seen "numerous instances" of vaccination producing eruptions, remaining "for months and even years afterwards, undermining the constitution, and very frequently terminating in phagedenic or corroding ulcers." And he had likewise witnessed coughs, dyspnœa, hectic marasmus, tedious and difficult to eradicate, &c., result from cow-pox. "Shocking reflection," he adds, "to a humane mind, that a poison should thus be introduced into the human constitution without the plea of necessity, or the support of reason and experience."[4] "Several children," observes Dr. Moseley, "have died from diseases brought on by the cow-pox, where no ulcerations had appeared, and others have lost their nails and ends of their fingers, several months after the inoculation."[5] "My accounts from the country are," he continues, "full of dismal histories of ulcerated arms and mortifications."[6] "Blindness," it was averred, "lameness, and deformity had been the result of employing the vaccine in innumerable instances, and its fatal venom had removed many an infant untimely from the world."[7]

Nay, it was strenuously maintained and believed, that not only were various old maladies, peculiar to man, thus excited into action by the "cow-pox poison," but that different new diseases peculiar to the cow were sometimes communicated to the human constitution by vaccination. "Various beastly diseases," writes Dr. Rowley, "common to cattle, have appeared among the human species since the introduction of cow-pox, cow-pox mange, cow-pox abscess, cow-pox ulcer, cow-pox gangrene, cow-pox mortification, and enormous hideous swellings of the face, resembling the countenance of an ox, with the eyes distorted, and eyelids forced out of their true situation; diseased joints, &c."[1]

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[1] Ibid. p. 14.

[2] Ibid. p. 128.

[3] Inoculation for the Small-pox Vindicated, &c., 1805, p. 40.

[4] Observations addressed to the Public on the Cow-pox, pointing out the dreadful consequences of this new Disease, so recently and rashly introduced into the Human Constitution. By R. Squirrell, M.D., 1805, pp. 16, 17.

[5] Treatise on the Lues Bovilla, p. 118.

[6] Ibid. p. 92.

[7] Moore's History of Vaccination, p. 39.

This was published in 1806, eight years after Dr. Jenner's first essay on vaccination appeared. During the year subsequent to the first public announcement of his discovery, Dr. Moseley suggested the possibility of the "bestial humor" of cow-pox producing "a brutal fever, exciting incongruous impressions on the brain;" and "who knows," says he, "but that the human character may undergo strange mutations from quadrupedan sympathy, and that some modern Pasiphäe may rival the fables of old?" Some after vaccination, were actually supposed to "cough like cows," and "bellow like bulls."[2] And one anti-vaccinist ingeniously suggested that if cow-pox were known to have existed in a family, this fact might debar the members of it from the chances of matrimony. For[3] "it would," he remarks, "be no letter of recommendation, and it would be cruel for the world to know who had labored under the cow-pox mange, evil, ulcer, or any other beastly disease; it might infallibly injure their fortune in life, particularly in matrimonial alliances. Who would marry into any family at the risk of their offspring having filthy beastly diseases?"

Nor were theological reasons, of course, wanting for calling in question the orthodoxy of vaccination, as of other new discoveries and practices.[4] "Small-pox," argues Dr. Rowley, "is a visitation from God, and originates in man; but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the Gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!'"[1] "The projects of these vaccinators seem," it was affirmed, "to bid bold defiance to heaven itself, even to the will of God."[2] "Providence," reasoned another author, "never intended that the vaccine disease should affect the human race, else why had it not, before this time, visited the inhabitants of the globe. The law of God," he continues, "prohibits the practice; the law of man and the law of nature loudly exclaim against it."[3]

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[1] Cow-pox Inoculation, p. 105. See prefixed to the work the colored portraits "of a cow-poxed, ox-faced boy," with two scrofulous abscesses, which were at one time alleged to indicate sprouting horns!—"This boy," observed Dr. Rowley, in a clinical lecture on the case, "is gradually losing the human lineaments, and his countenance is transmuting into the visage of a cow." (Moore's History, p. 46.) He further wrote—"A great number of new complaints, the diseases of beasts, filthy in their very nature and appearance, in the face, eyes, ears, with blindness and deafness, spreading their baneful influence over the whole body, have been not unfrequently the consequence evidently of cow-pox inoculation; either originating from the grease in horses, or the natural diseases of cows," p. 12.

[2] Mr. Ring, in his treatise on cow-pox, mentions "a lady who complained that, since her daughter was inoculated, she coughs like a cow, and has grown hairy all over her body; and Mr. Blair was told, on a late excursion into the country, that the inoculation of the cow-pox was discontinued there, because those who had been inoculated in that manner bellowed like bulls!"—*Blair's Vaccine Contest*, p. 69. [3] Introduction to Rowley's pamphlet, p. vii.

[4] As, for example, small-pox inoculation: see a "Sermon against the dangerous and sinful Practice of Inoculation," preached at St. Andrew's, Holborn, in 1722, by Edmund Massey, M.A. He urges various theological arguments against the "diabolical operation" of inoculation, and at last maintains that, even if it were medically successful, it was not to be courted, for he believes, if mankind should thus "happen to become more healthy, it is a great chance but they would be less righteous."—P. 26. In his admirable "Account of the Inoculation of Small-Pox in Scotland" (1765), Dr. Monro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a

In short, vaccination was opposed and denounced on a variety of grounds. It was alleged to be occasionally fatal in its consequences; to be liable to excite various diseased actions and predispositions; to produce diseases new to the human constitution; to "be impious, unthinking, profane, and irrational;" to be an innovation, neither "established on the basis of reason, nor supported by the foundation of truth."[4] "The vaccine," exclaimed one enemy to cow-pox inoculation, "was the damnedest thing ever proposed; he wished the inventors were all hanged, and he would give his vote for its being done."[5] And strong pictures were hung up to the public eye of the miseries it would infallibly lead to in case of the recurrence of epidemic small-pox. "In many families,"[6] writes an author whom I have already quoted, "there will be none to attend the sick, nurses will quit their patients for their own safety, and servants fly from their masters' houses to shun the pestilence. Then we shall experience a horrid scene of public and private calamity—brought on by a medical experiment, embraced without due consideration, extended by a rash transgression over the bounds of reason; and, after the fullest conviction of its inutility, obstinately continued, by the most degrading relapse of philosophy that ever disgraced a civilized world."

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heinous crime."—P. 5. "Clergymen," observes Dr. Baron, in his Life of Jenner, vol. i. p. 231, "preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things," he adds, "would scarcely obtain credence, were it not that similar arguments and assertions have been employed against vaccination itself."

[1] Rowley's Pamphlet, p. 9.

[2] Vaccine Contest, p. 84.

[3] Preface to the second edition of Dr. Squirrell's Observations, p. 4.

[4] Blair's Essay, p. 83; and Lipscomb's Pamphlet, p. 28.

[5] Moore's Reply to the Anti-Vaccinists, 1806, p. 14.

[6] Preface to Treatise on Lues Bovilla, p. 23.

Such were the chief forms of opposition and argument that were stoutly and vigorously urged against vaccination during the earlier years of its progress. They are the same by which many of the happiest and greatest improvements in our profession have each in turn been assailed at their first promulgation. From time to time in the march of medicine and other allied sciences, some earnest and expanded mind conceives and elaborates a great and novel thought, destined in its practical application to ameliorate the condition and promote the happiness of mankind. But hitherto almost as often as the human intellect has been thus permitted to obtain a new light, or strike out a new discovery, human prejudices and passions have instantly sprung up to deny its truth, or doubt its utility, and thus its first advances are never welcomed as the approach of a friend to humanity and science, but contested and battled as if it were the attack of an enemy.[1] Practical medicine, in its past career, is full of instances illustrative of this remark. Witness the history of the immense and now almost forgotten difficulties accompanying the first introduction of mercury, antimony, and cinchona-bark, into medical practice; or the stern obstinacy with which the ligature of arteries after amputation was long, long rejected, and cauteries and caustics preferred; or the professional and religious prejudices which the propriety of saving human life by inducing premature labor has encountered up to within the last few years. Further, every proposed improvement seems to be met with the same invariable array of objections and arguments. The discovery may be new, but the grounds of opposition to it are not new—they are merely the old forms of doubt, and difficulty, and prejudice, used on former occasions, recalled and reproduced anew. Thus not only in their leading principles and spirit, but in most even of their minute details, identically the same arguments that forty or fifty years ago were urged against the propriety and safety of vaccination, or a hundred years ago against small-pox inoculation,[1] have, within the last few months, been again invoked and used against the employment of etherization. Time has amply proved how futile and inapplicable these arguments were as directed against vaccination. In truth, those forms of reasoning and opposition against the employment of cow-pox that, some forty or fifty years ago, appeared to many members of the profession to be perfectly conclusive and insuperable, now read and appear to us at the present day as in the highest degree illogical and absurd. History has been compared to a mirror, in which we may study the faults of our predecessors, with the view of avoiding the same errors ourselves. The history of cow-pox is certainly calculated to teach us this one lesson, that in relation to the truth of any novel doctrine or practice, such as vaccination or etherization, adverse opinions and prejudgments are, however strongly entertained, or however strongly expressed, not in themselves adequate, as some, at the present time, would seem to believe, to decide the whole matter in dispute, either in one direction or another.[2] And the moral is obvious, hat while minds anxious to promote new and probable inquiries hould not be intimidated and deterred from their pursuit by such rejudgments on the part of others, those who are, on the contrary, nxious to suppress them, should not venture to base their opposiion upon mere impressions and mere opinions only. The ultimate ecision upon such investigations ever comes to be founded, not pon preconceived beliefs or hasty deductions, but upon the careful xamination and evidence of a sufficient body of accurate and wellscertained facts. During the last six months, etherization has been sed to a considerable extent in British surgery; and at the present ime, we are perhaps in a condition to turn and look back upon his past experience with it, in order to endeavor to form, from the xisting facts and cases, a proper judgment upon its merits or lemerits, and especially in order, if possible, to obtain some satisactory light upon that all-important question in relation to its employment, viz., whether its adoption increases or diminishes the usual mortality consequent upon surgical operations. It was with this view that the present communication was undertaken.

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[1] This remark holds true, for instance, with regard to small-pox inoculation, &c. &c. Lord Wharncliffe, in his edition of the Letters and Works of Lady Mary Wortley Montagu, after giving the history of her Ladyship's introducing the practice of small-pox inoculation into England from the East, observes: "What an arduous, what a fearful, and, we may add, what a thankless enterprise it was, nobody is now in the least aware. Those who have heard her applauded for it ever since they were born, may naturally conclude, that when once the experiment had been made and had proved successful, she could have nothing to do but to sit down triumphant, and receive the thanks and blessings of her countrymen. But it was far otherwise. Lady Mary protested that in the four or five years immediately succeeding her arrival at home, she seldom passed a day without repenting of her patriotic undertaking; and she vowed that she never would have attempted it if she had foreseen the vexation, the persecution, and even the obloquy it brought upon her. The clamors raised against the practice, and of course against her, were beyond belief. The faculty all rose in arms to a man, foretelling failure and the most disastrous consequences; the clergy descanted from their pulpits on the impiety of thus seeking to take events out of the hand of Providence; the common people were taught to hoot at her as an unnatural mother who had risked the lives of her own children.

"We now read in grave medical biography, that the discovery was instantly hailed, and the method adopted, by the principal members of that profession. Very likely they left this recorded; for whenever an invention or a project—and the same may be said of persons—has made its way so well by itself as to establish a certain reputation, most people are sure to find out that they always patronized it from the beginning; and a happy gift of forgetfulness enables many to believe their own assertion."—Letters and Works of Lady Mary Wortley Montagu. Edited by her great grandson, Lord Wharncliffe, vol. i. p. 55.

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[1] "The very same objections," writes Mr. Moore, in 1805, "accompanied with the same species of proof, were adduced against it (small-pox inoculation), as are now brought against vaccination."—See his Reply to the Anti-Vaccinists, p. 70.

[2] I have been told, that any comparison between the progress of vaccination and etherization cannot be true in one respect, that vaccination was at once and generally received. The quotations in the text show the contrary; and many of the strongest adverse opinions which I have quoted were published in 1805–6, seven or eight years after Dr. Jenner published his first admirable essay on the subject in 1798. After Dr. Jenner published this essay, he went to London, and resided there for nearly three months; but during this time, "with all his efforts and those of his friends, he was unable in the metropolis to procure one person on whom he could exhibit the vaccine disease. Not one individual would submit to it. After Jenner left London, Mr. Cline made the first experiment in London with cow-pox, by inoculating it as a counter-irritant on the hip of a patient affected with morbus coxarius."—(Baron's Life of Jenner, vol. i. p. 150.) Jenner first tried artificial vaccination in May, 1796. In March, 1800, Mr. (afterwards Sir Matthew) Tierney wrote him from Edinburgh, where he was then a student, stating that "Dr. Gregory, the professor of physic, knew very little about it, and, of course, did not encourage it. Mr. Anderson, a surgeon at Leith, is the only person here who has tried it."—(Ib. p. 376.) As late as 1805, the popular opinion in London was much shaken with regard to the propriety and safety of vaccination. "The influence," says Mr. Blair, "of false rumors and distorted facts operated so strongly in the district of Bloomsbury and St. Giles, as to preclude even a single person from applying for vaccination at that dispensary."—(Pamphlet, p. vi.) Dr. Moseley boasts (p. 13 of his Treatise, second edition), that at that date, 1805, the middle and inferior classes in London had "renounced the delusion," and would not "expose their children to cow-pox." Instances of more marked popular hostility against it were not wanting in the early

But, in the first place, and before engaging with this more difficult part of the inquiry, let me briefly adduce the positive evidence which we possess of the effect of etherization in cancelling and abolishing the sufferings attendant upon surgical operations, and the best means of effecting this desirable object, the *a priori* objections to it, &c.

CHAPTER II.

PROOF OF ANÆSTHETIC AGENTS POSSESSING THE POWER OF ANNULLING THE PAIN ATTENDANT UPON SURGICAL OPERATIONS.[1]

ABUNDANT evidence to convince any unprejudiced mind upon this subject has already been accumulated in our periodical literature; and no one who has felt in his own person, or witnessed in others, the proper and perfect effects of etherization, can reasonably

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history of cow-pox. Mr. Gooch states, that the first people he vaccinated in Hadleigh, Suffolk, 'were absolutely pelted and drove into their houses if they appeared out."—(Baron's Life, vol. i. p. 382.) In the town of St——n, Kincardineshire, a surgeon was lately used in a imilar manner, for venturing to etherize a patient for extraction of a tooth. But still etheriation has made more progress in months than vaccination effected in years; and already, ithin a few short months, a knowledge of it has spread over almost the civilized world. Within these few days, I received a letter of consultation from a lady, asking some direcons for the use of etherization at her approaching confinement, in October next. The tter was from the Far West, and dated "Mount Morris, Illinois, United States."

[1] From Edinburgh Monthly Journal of Medical Science, Sept. 1847, p. 153.

entertain any scepticism upon this point. In regard to it, I shall content myself with adducing the evidence which Dr. Forbes some time ago offered as the result of his inquiries on the subject, in the London hospitals. "For the purpose," he says, "of obtaining information on all the points of this most interesting subject, we personally questioned all the patients in the London hospitals, who, at the period of our visits, still remained in the wards after the ether operations. They were in all *fifty-four*, and the great majority had been the subjects of capital operations. They were unanimous in their expressions of delight and gratitude at having been relieved from their diseases without suffering. In listening to their reports, it was not always easy to remain unmoved under the influence of the conceptions thereby communicated, of the astonishing contrast between the actual physical condition of the mangled body in its apparent tortures on the operating table of a crowded theatre, and the really happy mental state of the patient at the time. The old story of the magician in the Arabian Tales seemed more than realized before us, the ether being like the tub of water, one moment's dip of the head into which produced a life-long vision in the dreamer's mind."[1] Every operator who has used anæsthetics can confirm these statements. Additional evidence is unnecessary.

CHAPTER III.

CONDITIONS FOR INSURING SUCCESSFUL ANÆSTHESIA.[2]

To produce, however, the full and perfect effects of etherization, it is necessary to conduct the process in conformity with certain conditions. These conditions it is not the object of the present communication to consider. But I will take the liberty of mentioning two or three leading points, the importance of which, in relation to the attainment of complete success, has become strongly impressed upon my own mind by a somewhat extensive experience in etherization during the last few months.

First, The patient ought to be left, as far as possible, in a state of absolute quietude and freedom from mental excitement, both during the induction of etherization, and during his recovery from it.[3] All talking and all questioning should be strictly prohibited. In this way any tendency to excitement is eschewed, and the proper effect of the ether inhalation more speedily and certainly induced. And, *secondly*, with the same view, the primary stage of exhilaration should be entirely avoided, or at least reduced to the shortest possible limit, by impregnating the respired air as fully with the ether vapor as the patient can bear, and by allowing it to pass into the lungs both by the mouth and nostrils, so as rapidly and at once to superinduce its complete and anæsthetic effect. Latterly, I have found that for surgical purposes, and when it is not necessary to keep up the etherization above five or ten minutes, by far the best and most perfect inhaler is formed by a large sponge of the common hollow conical shape, perforated artificially with a pretty large aperture at the apex, and placed over the face like a mask, so as to include both the mouth and nose in its concave base. At first, it should be held at a little distance from the face, and afterwards gradually advanced to it, in order to avoid exhibiting the vapor in too powerful and irritating a form for the first few inspirations. Its interior should, immediately before using it, be fully and freely saturated with ether—a very common but certainly a very unpardonable error being to exhibit an imperfect and exciting, instead of a perfect and narcotizing dose of the vapor.[1] Many of the alleged failures and misadventures are doubtless entirely attributable to the neglect of this simple rule; not the principle of etherization, but the mode of putting it in practice, being altogether to blame. But, *thirdly*, whatever means or mode of etherization is adopted, the most important of the conditions required for procuring a satisfac-

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[1] See Dr. Forbes' very able article on Etherization, in the British and Foreign Review for April, 1847, p. 554.

[2] From Edinburgh Monthly Journal of Medical Science, Sept. 1847, p. 154.

[3] The area of an hospital operation theatre is hence, perhaps, not the most favorable place for securing all the advantages of etherization, or rather for shunning all its disadvantages. Lately, in a case in which Professor Miller performed partial amputation of the foot,

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In the Royal Infirmary, I etherized the boy who was the subject of it, in his bed in the wards. He was carried in this state up stairs to the operating theatre—the amputation performed—and the patient brought back again to his bed before he was allowed to awake. He was thus, at one and the same time, entirely spared the moral shock and pain of being transported and carried in before a formidable collection of surgeons and students, and saved from the physical sufferings attendant upon the amputation itself; for he was perfectly unconscious of aught that had occurred, and, when he awoke, he was not aware that he had been operated upon, or had even left his bed. While being carried from the ward to the operating room, the sponge with which he was etherized was kept fixed over his face with a couple of common elastic letter bands. In our surgical hospitals, if a ward immediately adjoining the operating theatre were set aside for operation cases, it would in this way facilitate the process of etherization, and insure more certain and perfect results from it.

[1] When a prolonged effect is required, as in midwifery cases, an instrument is necessary—were it for no other reason than the saving of ether, and the prevention of its diffusion through the apartment. Within the last few days I have seen a pamphlet, dated Boston, May 30, 1847, in which it is stated that for three months previously, all apparatus had been laid aside, and the sponge alone used for etherization, by Dr. Morton of that city—the gentleman to whom, I believe, the profession and mankind are really and truly indebted for first introducing into practice the production of insensibility, by ether inhalation, with the object of annihilating pain in surgical operations.—See Some Account of the Letheon, by Edward Warren, p. 87.

tory and successful result from its employment in surgery, consists in obstinately determining to avoid the commencement of the operation itself, and never venturing to apply the knife, until the patient is under the full influence of the ether vapor, and thoroughly and indubitably soporized by it.

In the operating theatres of the Paris hospitals, the most triumphant successes, in the original trials with ether, were obtained by M. Velpeau, who differed from his fellows in one all-important particular only, namely, in the forbearance with which he waited for the complete insensibility of his patients before venturing to take his operating knife into his hand. Few men have had more practice in etherizing than Dr. Snow of London, who has been in the habit, for some time past, of thus assisting in their operations some of the first surgeons in the metropolis. Speaking of the stage of etherization required, he draws a proper distinction between two degrees of this state, the first, and slighter, in which the patient moves, and winces, and seems to feel pain at the moment, but without afterwards remembering it; and the second and deeper state in which there is no evidence whatever of pain being felt, far less remembered. And he adds, "In full four-fifths of the cases in which he had administered the ether, there was not the least flinch or groan during the cutting of the surgeon's knife. He considered cases of this kind the only truly successful ones, and believed that with proper care every case might be of this nature. When the patient exhibited signs of pain, although he might have no knowledge or recollection of it afterwards, the ether was only partially successful."[1] As a proof that such deep states of etherization are not accompanied with danger, I may mention here, though in the way of anticipation, that out of 39 surgical operations, "nearly all serious ones," in which Dr. Snow has exhibited either at St. George's hospital, 2 only of the patients died, namely, 2 on whom amputation of the thigh was performed after they had been previously reduced to an "extremely weak and emaciated" state.[2] Now, 2 deaths in 39 hospital operations, or 1 in 18, would certainly be regarded as a very satisfactory and favorable result under almost any circumstances, and either with or without ether. Dr. Peacock, in his offcial reports of the Edinburgh Infirmary for 1842 and 1843, has published two tables showing the results of the "principal operations" in that Institution, from amputation and lithotomy down to the operation for harelip.[3] The tables include 150 cases in all;

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[1] Medical Gazette, February 26, 1847, and Braithwaite's Retrospect of Medicine, vol. xv. 1847, p. 409.

[2] Lancet for May 29, 1847, p. 553.

[3] Statistical Tables of the Royal Infirmary of Edinburgh for 1842, p. xix.; and for 1843, p. xviii. In the reports of other years the operations are unfortunately not tabulated on the same plan, and do not show the mortality dependent upon them.

32 of the 150 patients operated upon died, or about 1 in every
Excluding 57 cases of "extirpation of tumors in various parts,"
the 93 individuals on whom other operations were performed,
early 1 in every 3, died.

produce the complete anæsthetic and soporific effects of the
roform, some conditions are necessary to be attended to.[1] With-
ttending to these conditions, you will have failures. 1. The chlo-
rm vapor must always be exhibited as rapidly, and in as full
ngth as possible, if you desire to have its first or exhilarating
e practically done away with, and excluded; and you effect this
giving the vapor so powerfully and speedily as to apathize the
ent at once. If you act otherwise, and give it in small or slow
s, you excite and rouse the patient in the same way as if nitrous
le gas were exhibited. 2. In order that the patient be thus brought
peedily as possible under its full influence, the vapor should
llowed to pass into the air-tubes by both the mouth and nostrils
d hence all compression of the nostrils, &c., is to be avoided.
he vapor of chloroform is about four times heavier than atmo-
eric air. And hence, if the patient is placed on his back during
xhibition, it will, by its mere gravitation, force itself in larger
tities into the air-passages than if he were erect or seated. As
he best instrument for exhibiting the chloroform with these
cations, the simple handkerchief is far preferable to every means
dopted. It is infinitely preferable to any instrument I have
een, some of which merely exhibit it by the mouth, and not by
nostrils, in small and imperfect, instead of full and complete
s; and with instruments so constructed, there is no doubt what-
that failures and exciting effects would ever and anon occur.
des, inhaling instruments frighten patients, whilst the handker-
does not; and mental excitement of all kinds, from whispering
talking around the patient, is to be strictly avoided, if possible.
the quantity required to be applied to the handkerchief, it has
stated, that the average dose of a fluid drachm is generally
ient to affect an adult; but I have latterly seldom measured the
tity used. We must judge by its *effects*, more than by its quan-
The operator gathering his handkerchief into a cup-like shape
s hand, should wet *freely* the bottom of the cup (so to speak),
f the patient is not affected in a minute or so, he should add a
more. It evaporates rapidly; and you must not wet your
kerchief, and then delay for a minute or more in applying it.
ust be applied immediately. Not unfrequently, when the
nt is just becoming insensible, he will withdraw his face, or

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[1] om Edin. Monthly Journal of Med. Science, January, 1848, p. 545, reported from a
ion in the Medico-Chir. Society.

forcibly push aside the handkerchief. If you *then* fail to reapply it to his face, and keep it there, you will be liable to leave him merely excited. But probably two or three inhalations more will now render him quite insensible. The simplest test of its full and perfect effect, is some noise or stertor in the respiration. Cease it as soon as this is fully set in. But reapply it, of course, from time to time, if it is wished to keep up its effects.

Dr. Bennett has spoken of the stertor, or some other symptom, being "serious." Now, this and other terms are, it is believed, calculated to excite unnecessary fear. "Serious" is a relative and conventional term, constantly liable to be altered by increased knowledge and experience. Twenty years ago, travelling at the rate of thirty miles an hour would have been reckoned a very serious matter. Nowadays every one knows it is not so. The tyro looks at first upon the symptoms of an aggravated attack of hysteria as very serious. The physician of more experience knows they are not so. The stertorous breathing, the spasms and almost convulsive symptoms, &c., sometimes produced by chloroform, may appear serious to those who have had little experience in the use of this agent. But every one who has seen much of its effects, knows that there is no danger following, nor is there inconvenience left, after such a show of serious symptoms.

The strength and purity of the chloroform employed are essential elements of success. Professor Gregory has examined about a dozen specimens which he had procured from various shops, here and in Glasgow. Several of them were by no means of the standard strength. A medical friend, two days ago, asked me if I had ever failed obtaining the effects of chloroform upon any person. I replied, never in any one case. My friend added that he had, the night previously, been unable to influence a parturient patient, though he had given her an ounce. On inquiry, I found he had used chloroform from a shop where, according to Dr. Gregory's researches, it was sold under a specific gravity of 1·200, instead of being 1·480.

CHAPTER IV.

THE PREVENTION OF PAIN IN SURGICAL OPERATIONS ALLEGED TO BE UNNECESSARY AND IMPROPER.[1]

THERE is one strange episode which has been frequently reacted in the early history of several of those practical improvements which we all now proudly refer to, and rejoice over, as among the greatest and most undoubted advances made in the past march of medical science. It is this. Some striking discovery happens to be made, or some great improvement suggested. Its worth and importance, betimes, are acknowledged by different members of the profession. Others, however, full of doubts and difficulties, conjure up to their own minds, and to the minds of their brethren, all the usual forms of objection to the new view that has been propounded; and the more ardent among these opponents always fix, and insist among other points, upon this special and singular ground of objection, that—the disease or evil proposed to be remedied is comparatively and truly less obnoxious in its character than was in general previously imagined, that its removal is, consequently, not a matter of much moment, and that the new and artificial measure now suggested for that purpose is, probably, in its action and effects on mankind, really more dangerous and deleterious than the very state or malady which it was proposed to remove by it.

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[1] From the Edinburgh Monthly Journal of Medical Science, Sept. 1847, p. 166.

Take, for instance, as an example of the remark, the strong opposition offered first to variolous inoculation, and afterwards to vaccine inoculation.

It has been repeatedly calculated in regard to Europe, that before the introduction of variolous inoculation, small-pox regularly decimated the human race, one out of every ten deaths that occurred being a death from small-pox; and further, the disease was reputed fatal to at least one out of every six or eight individuals attacked by it. Inoculation was introduced, and its effects were so marked, that of those who submitted to the disease in this form, about one only in every 300 or 400 seems to have perished.[1] Vaccination was discovered, and this preventive affection was found to prove rarely or never *per se* dangerous or fatal to life. But science, in substituting—first, inoculated for natural small-pox; and secondly, cow-pox for inoculated small-pox, was anxiously combated by the double argument, that the old evil was not so great an evil as it was usually represented to be, and that perhaps it was in reality safer and better than the new measure proposed as a substitute for it.

Thus, a hundred and twenty years ago, among the various pamphlets published with the view of contesting and opposing the propriety of the newly introduced Variolous Inoculation, Mr. Howgrave, whose essay on the subject is one of the best of the period, maintains, that "the small-pox in the natural way very rarely affects life where the habit of body and constitution are good;"[1] "that the hazard of inoculation is not only not considerably less, but considerably greater than that of the natural small-pox;"[2] that this "new and strange method,"[3] "more frequently produces accidents than the natural way,"[4] that it is "not only unsafe, but uncertain,"[5] and that, "all persons who will suffer reason to determine their opinion, must be convinced that their (the inoculation) method has no degree of safety in it."[6] Hence we can scarcely wonder when he "affirms that the best method is to trust Providence, and not allow mortal man to inflict diseases upon us at his pleasure."[7] Other contemporary writers against small-pox inoculation, maintained, upon the same grounds, the same views of the impropriety of the practice.[8]

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[1] In reference to the mortality of natural and of inoculated small-pox, Dr. Gregory, Physician to the Small-Pox Hospital of London, observes, "It is commonly stated that one-fourth of those who are attacked by Small-pox in the natural way perish."—Cyclopædia of Medicine, vol. iii. p. 742. And "The average number of deaths (from inoculated small-pox) at the Inoculation Hospital, was only three in a thousand (or about 1 in 330). In the wards appropriated to the casual diseases the deaths were, and continue to be, 3 in 10," or more than 1 in 4.—Ibid. vol. iv. p. 749.

Again, forty or fifty years ago, in opposition to the proposal of Dr. Jenner to introduce vaccination, or, in other words, to substitute the inoculation of cow-pox for the inoculation of small-pox, the same objections to the practice were reproduced. Dr. Moseley stated,[9] for example, several years after vaccination was introduced, that "the mischiefs of the natural small-pox, we all know, are great enough, but they may be prevented by inoculation;" that, "instead of 1 in 300 dying of the small-pox from inoculation, there need not be one in 300,000;" "that the inoculated cow-pox is not a milder or safer disease than the inoculated small-pox;" and that "the necessity of an immediate restoration of small-pox inoculation must strike every person interested in the welfare of society, and the happiness of his fellow-creatures." Cow-pox (averred Mr. Drew and Mr. Forrester) was a "*far more* severe disease than small-pox."[10]

"Out of many thousands, nay millions," (so wrote Dr. Rowley in 1805,[11]) "it has been fully proved scarcely any one died from small-pox inoculation;" "it was safe, mild, and certain; therefore, cow-pox inoculation as a substitute was absolutely unnecessary;" besides, the substitute itself "stands condemned by the experience of veterans in the profession;" "disagreeable events (eruptions, abscesses, gangrenes, chronic diseases, &c.) have in great numbers followed, and can be traced to arise from cow-pox inoculation," while small-pox inoculation was free from them; and "fifty-nine have died, and a great number of victims become diseased for life," in consequence of vaccination; in fact "the senses," says Dr. Rowley, "are appalled, and the pen is tired of recording its dreadful disasters."[1]

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[1] Reasons against the Inoculation of the Small-pox. London, 1724, p. 67.

[2] Ibid. p. 72. [3] Ibid. p. 8. [4] Ibid. p. 61. [5] Ibid. p. 5. [6] Ibid. p. 28. [7] Ibid. p. 63.

[8] See, for instance, the Rev. Mr. Delafaye's Sermon on "Inoculation, an Indefensible Practice," pp. 22, 23; or "A Letter in Reply," by N. Bolaine, Surgeon, p. 17. Sparham's "Reasons against the Practice of Inoculating the Small-pox," p. 27. Dr. Wagstaffe's Letter to Dr. Friend, showing the danger and uncertainty of Inoculating the Small-pox, pp. 48, 49, &c.

[9] Treatise on the Lues Bovilla, 2d edit. 1805, pp. xv. xx. xxiii. 110, &c.

[10] See Lipscomb's Essay, p. 7, and Lipscomb's own similar opinion, p. 39.

[11] On Cow-pox Inoculation, pp 4, 31, 100, 128, &c.

But, in our own day, exactly the same line of argumentation that was thus zealously directed against the adoption of artificial variolation and vaccination, at *their* first introduction into practice, is now as zealously directed against the adoption of etherization. For precisely in the same way some minds, averse to the employment of ether inhalation, anxiously argue that there is not only no call for its employment in surgery, but that its employment, and the result which it produces—namely, immunity from the physical sufferings inflicted by the knife of the surgeon—is probably a direct and positive evil.

The common opinion of mankind and of the profession in regard to the pain attendant upon surgical operations, seems, till of late, to have been unanimous and unchallenged. The human agony and torture following the surgeon's knife, have hitherto been borne with and submitted to merely because, while they seemed absolutely necessary for the preservation of health and life, they were considered at the same time absolutely unavoidable. It is true that differences in the mental and physical constitutions of different men, enabled them to encounter the surgical mangling and mutilation of their limbs and bodies, with different degrees of equanimity and hardihood. And under special morbid states of the nervous system—in the way both of great excitement and great overstraining and collapse—flesh and bone have sometimes been extensively cut and divided without the common accompanying feelings and common manifestations of acute suffering. But the every-day experience of mankind in every age, shows how greatly and truly the reverse of this is usually the fact. And all past human testimony on this point goes fully to corroborate the truth of the sentiment which England's great epic poet puts into the mouth of the wounded Nisroch, on the first occasion in which wounds were ever inflicted, and living beings first "knew pain:"—

> "But Pain is perfect misery, the worst
> Of evils, and excessive, overturns
> All patience."

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[1] Ibid. p. 61. See the works also of other anti-vaccinists for the same line of argument. Thus, observes Dr. Squirrell, the small-pox inoculation, "whatever impediment it might have met with at its introduction, owing, at that time, to a want of experience in the practitioners, is now a mild and harmless disease," and hence "there is no necessity to forsake its practice" for that of vaccination, with "all its difficulties, ambiguities, and malignant effects." "The cow-pox (he argues more at length) produces malignant effects, vitiates the blood, and other juices, and is tedious as well as difficult to cure; the small-pox inoculation produces no ill consequences whatever. The cow-pox produces very ill health in children; the small-pox inoculation improves the health and constitution, and carries off many complaints, which were very uncomfortable both to the parents and children. The cow-pox matter is taken from an animal diseased, and is of specific scrofulous kind, as proved by its effects; the small-pox matter is taken from a healthy subject, and produces no disease whatever, but the one for which it was intended."—Observations on the Cow-pox, pp. 24, 49, 55, &c.

Nor have we any proper test, either of the fortitude with which it wi
be borne, or of the amount of pain actually endured in individu
cases. For whilst the degree of outward manifestation of sufferin
shown by the patient on the rack of the operating table, affor
no perfect evidence of his actual feelings, the greatest torture bein
incapable in some of contorting a muscle, or eliciting a groan, an
the slightest scratch forcing screams and cries from others; neithe
on the other hand, is the degree of equanimity and endurance wi
which the agony of a surgical operation is submitted to, any tr
and direct criterion of the natural moral strength and mental courag
of the patient. Sometimes the sailor and soldier, who would n
feel one moment's dread in facing, under the most desperate ci
cumstances, the sword and fire of the enemy, will quail before th
very thought of having his living flesh deliberately cut and mutilat
by the cold steel of the surgeon.[1] And thus, the mere fear and ho
ror of encountering the pain and agony of a surgical operation, wi
sometimes, by unnerving the strongest mind and boldest hea
bring on, as a consequence, such a depressed state of the syste
as certainly by no means places the patient in a state favorable
securing a happy and successful result. "An extraordinary exci
ment of mind," observes Mr. Travers, "such as is produced
dread, or by the screwing up of the system for the enduranc
painful operations, when it is already much depressed and e
vated by continued suffering, or apprehension of it, someti
proves suddenly fatal."[2]

In the greater operations of surgery, the employment of ethe
tion is not only, thus far, a great blessing to the patient, but it
great boon also to the practitioner. It is a benefit to the oper

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[1] The following illustrative anecdote refers to Lord D——, assuredly one of the b
admirals in the English service, and a man of all others whom his country has long l
upon as the very personification of valor and courage. The injury alluded to was recei
cutting out the Spanish frigate A——, one of the most daring feats attempted in modern
fare. I extract the account from an article on Etherization in the North British Review,
lent in matter, in manner, and in feeling, and written by one of the most distinguished
cal practitioners and authors of the present day. "We remember," says the reviewer,
case of a gallant admiral—one of the bravest hearts that ever beat, in a service where
of every grade are, to a proverb, dauntless—who, in the opening of his distinguished c
had been engaged in cutting out an enemy's frigate. From the gunboat, he climbed u
ship's steep side, and, foremost of his crew, had reached the bulwarks, when recei
stunning blow, he fell backwards into his boat again, striking his back violently on the th
Many years afterwards, a tumor had grown on the injured part, and at length the ad
—gray, and bent in years, found it advisable that this growth should be removed. The
that never feared death in its most appalling form while in the discharge of duty, now sh
from the surgeon's knife; the removal, contemplated with a feeling almost akin to fear,
long deferred; and at length, half stupified by opium though he was, a most unsteady pa
did he prove during the operation."—North British Review for May, 1847, p. 169.

[2] Travers on Constitutional Irritation, vol. i. p. 22.

to those operated upon. For, whilst it relieves the one
ie feelings of agonizing pain, it relieves the other from the
' inflicting such pain upon a fellow-creature. Humanity,
st sense of the term, is the great object of the healing art,
m alike of the surgeon as of the physician. Hitherto the
al duty of the surgeon has compelled him to inflict pre-
ring upon his patients, with a prospective view to their
ate benefit and advantage. And surely there is no one,
uch inured to the sight and shriek of pain, who would not
be able to obtain these advantages for his patients, without
g them to pass through so severe an ordeal as the tortures
rating table. If, as some few operators themselves have
y boasted, their hearts have become so hardened by educa-
ustom as not to be affected by the sufferings of those sub-
their knives, it is still pleasing and refreshing to know
hange in the human feelings, and this artificial violation
the first laws in human nature—namely, sympathy with
ings of others—is by no means necessary to make a man
urgeon.

no surgeon ever operated either more frequently or more
ly than the celebrated Cheselden. In St. Thomas's Hos-
, and exclusively of his practice elsewhere, he performed
upwards of two hundred times, and with results that
r yet been surpassed by any other operator. But he adds,
ge bearing all the simple impressiveness of truth, "If I
eputation in this way, I have earned it dearly; for no one
red more anxiety and sickness before an operation; yet,
time I began to operate, all uneasiness ceased; and, if I
better success than some others, I do not impute it to
wledge, but to the happiness of a mind that was never
disconcerted, and a hand that never trembled during any
"[1] It was under this great master that John Hunter re-
first lessons in surgery; and the biographer of Hunter
at, to quote his words, "Cheselden's manners were exceed-
l and gentle, and, notwithstanding the extensive practice
oyed, he always, before an operation, *felt sick at the thoughts*
he was about to inflict; though, during its performance, his
nd presence of mind never forsook him. Such feelings,"
Ottley, "are in a less marked degree, perhaps, far more
experienced than is generally supposed, by the very best
previously to undertaking operations of importance."[2]
loubt, it is the desire to diminish the actual amount of

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len's Anatomy of the Human Body, 5th edition, p. 333.

Life of John Hunter, in Mr. Palmer's edition of his works, vol. i. p. 9.

pain endured by patients, by curtailing the actual duration of it, that has led many of our best surgeons, with praiseworthy earnestness, to attempt to invent new and more rapid modes for performing particular operations,—a field in which no small degree of success has consequently been met with.

A new era, however, arrives in chirurgical science, and a measure is, at last, brought to light, through the influence of which surgeons may perform operations, and patients submit to them, even when of a prolonged nature, without the necessity of pain. It is found that the excruciating tortures, and writhings, and shrieks of patients on the operating table, may be saved; and yet the required operations be as well and perfectly executed as before. Scarcely, however, is this glad and glorious discovery announced and acted upon, than another new, and, if possible, still stranger discovery, is broached and anxiously promulgated; namely, that in cutting the living flesh of man, the surgeon's knife does not, after all, produce any very remarkable or very important amount of pain, and that immunity from this pain during operations would be, perhaps, an evil rather than a good to humanity—a calamity rather than a blessing.

At a meeting of the South London Medical Society, held in April last, Dr. Gull read a paper on the injurious effects of ether inhalation, and ended his communication with queries as to the "*desirability* of removing pain," &c.[1] Mr. Bransby Cooper, Surgeon to Guy's Hospital, afterwards affirmed it as his opinion, "that pain was a premonitory condition, no doubt fitting parts, the subject of lesion, to reparatory action, and, therefore, he (Mr. Cooper) should feel averse to the prevention of it."[2] "Pain," argues Mr. Nunn, surgeon to the Colchester and Essex Hospital, in some observations against ether inhalation,—"pain (toothache?) is, doubtless, our great safeguard under ordinary circumstances; but for it we should be hourly falling into danger; and I am," he continues, "inclined to believe that pain should be considered as a healthy indication, and as an essential concomitant with surgical operations, and that it is amply compensated by the effects it produces on the system, as the natural incentive to reparative action."[3] Arguing in a similar but still more bitter strain against etherization, Dr. Pickford affirms, that "pain during operations is, in the majority of cases, even desirable; and its prevention or annihilation is, for the most part, hazardous to the patient."[4] Upon one of the first communications being given in to the French Academy of Sciences upon etherization, M. Magendie, the distinguished physiologist, maintained[1] that "pain has always its usefulness;" he doubted if there was a true advantage "in suppressing pain, by rendering patients insensible, during an operation;" and argued, that "it was a trivial matter to suffer (*c'est peu de chose de souffrir*); and a discovery whose object was to prevent pain was of a slight (*mediocre*) interest only."

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[1] See Report of the Meeting in the London Medical Gazette for April 30, 1847, p. 777.

[2] Ibid. [3] London Medical Gazette for March 5, p. 415.

[4] On the Injurious Effects of the Inhalation of Ether, in the Edinburgh Medical and Surgical Journal for July, 1847, p. 258.

It would be as idle as it would be useless to confute by mere argument the preceding allegations, in regard to the supposed necessity and advantages of pain in surgical operations. For, in fact, the whole question amounts to this: Mankind are perfectly agreed, that the cutting and mutilation of the living human body is painful, however loudly surgeons may preach to the contrary; and medical men have hitherto assented, without one contradictory voice, to the self-evident aphorism of Galen, that pain is useless to the pained (*dolor dolentibus inutilis est*). If we find then, as we do now, a few men entertaining and expressing opinions on these points so very different from the general ideas and general experience of mankind, these opinions can scarcely be looked upon as aught else than indications of a strange degree of eccentricity of thought upon one special subject. And if the same line of thought that they apply to pain were extended, as it should be if true in principle, to other diseases or effects of disease, the untenable and irrational character of it would lead to conclusions that might, perhaps, surprise and startle even those minds that at present employ it against etherization. For if physical pain and agony be a blessing and benefit, and not an evil and a calamity to be eschewed, then all other morbid symptoms and affections should equally, upon the very same grounds, be included under the same category—their presence endured and courted, and their removal forbidden and avoided; diseases and death are parts of the great economy and general police of nature, and the labors of the physician and surgeon to counteract their agency, should be denounced and decried as unnecessary and improper.

Let us view the subject, however, calmly, and as a question in pathology. And that pain is, *per se*, and especially when in excess, a condition which is not only trying and difficult for any exertion of human fortitude to bear, but at the same time directly injurious and damaging in its action upon the constitution, and sometimes very fatal in its effects, is a practical truth that many of our best surgeons and soundest pathologists have long amply acknowledged and attested, and that, too, without any prejudgment in regard to it, or, at all events, without any view to such an extraordinary proposition as the now alleged impolicy and impropriety of abolishing it.

[1] Gazette Médicale de Paris, 6th Feb. 1847, pp. 112, 113.

Speaking of the pains of wounds, the great father of French surgery, Ambrose Paré pithily tells us, that it "ought to be assuaged; because," adds he, "nothing so much dejects the powers of the patient (*prosterne et abatte les vertus du malade*)."[1] "Mere pain," observed the late Dr. Gooch, "*can* destroy life."[2] "Pain," according to Mr. Travers, "when amounting to a certain degree of intensity and duration, is of itself destructive."[3] "Pain in excess," he again remarks, "exhausts the principle of life; so that either its continuance without intermission, or the superaddition of the slightest shock subsequent to its endurance for a certain period, is fatal. In operations protracted by unforeseen difficulties, as in cases of lithotomy, in which the stone is of such magnitude as to require crushing, the patient has begun to die upon the table. The same happens in parturition, &c., protracted by mechanical impediment," &c.[4] Again, "Bodily exhaustion from continued pain, spasm, and other causes, not unfrequently proves suddenly fatal. I have often observed a sudden deliquium at the conclusion of an operation for recent injury, arising evidently from the exhaustion of the nervous system in the effort to support it."[5] "Great sensibility or excessive pain attendant on an injury (or operation) has," remarks Professor Burns, "two effects. First, it exhausts both the system and the part: and, secondly, it acts as an exciting cause of inflammation, a disease apt enough to take place from the injury itself."[6] "We have many facts," observes Professor Alison, "to prove that various violent and overpowering sensations, intense pain, &c., when acting in the utmost intensity, affect the circulating system just as a concussion does, and sometimes with fatal effect—especially when they take place in a state of unusual weakness or exhaustion."[7] Dr. Ranking has lately published a striking instance of the fatally depressing effects of extreme pain. It occurred in a case in which a ligature was applied for the cure of an erectile tumor of the breast. "The patient," he relates, "a healthy female, bore the initiatory steps of the operation without a murmur, without failure of pulse, and without change of countenance. The instant the ligature was tightened, which it was with the full force of two surgeons, she gave a yell of agony, the pulse became imperceptible, the countenance became ghastly pale, and in eighteen hours she was a corpse."[1]

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[1] Paré's Works, Johnston's Translation, p. 329.

[2] Merriman's Synopsis of Midwifery, 1826, p. 239.

[3] An Inquiry concerning that disturbed state of the Vital Functions, usually denominated Constitutional Irritation, 1826, p. 65.

[4] Ibid. vol. i. p. 76. The statistical data published by Dr. Collins in his excellent Report of the Dublin Lying in Hospital, proves how true Mr. Travers' remark holds as a general principle with regard to the effect of pain in protracted parturition. In fact, the maternal mortality attendant upon parturition, regularly increases in a ratio progressive with the increased duration of the woman's sufferings. Thus, according to calculations which I have made from Dr. Collins' data, while in the women whose sufferings were terminated within two hours, only one in about 320 of the mothers died; where the labor varied in duration from two to six hours, one in 145 died; in those in whom it continued from seven to twelve hours, one in 80 died; when it endured from twelve to thirty-six hours, one in 23 died; and out of those whose sufferings were prolonged beyond thirty-six hours, one in every 6 perished. See vol. i. of this work, p. 468.

[5] Loc. cit. p. 24.

[6] The Principles of Surgery, by John Burns, M.D., vol. i. p. 502.

[7] Outlines of Pathology and Practice of Medicine, p. 13. See also pp. 203 and 317, on peritonitis proving directly fatal, "by virtue, probably, of the intense and peculiar sensation (of pain) it excites, acting as a powerful sedative on the heart." On pain as a cause of exhaustion of nervous power, see also Dr. Holland's Medical Notes and Reflections, p. 618.

But it is, I believe, needless to accumulate superfluous proof on a point on which the observations and feelings of the profession and of mankind are sufficiently agreed—namely, that bodily pain, particularly such excess of pain as, with all its concomitant fears and sickening horrors, accompanies the larger operations in surgery, is, with very few, if indeed any exceptions, morally and physically a mighty and unqualified evil. And, surely, any means by which its abolition could possibly be accomplished, with perfect security and safety, deserves to be joyfully and gratefully welcomed by medical science, as one of the most inestimable boons which man could confer upon his suffering fellow-mortals. Few now deny, and, no doubt, ere long fewer and fewer will venture to doubt, that with a generality as certain and constant, or indeed more so, than we see exemplified in the action of most other therapeutic agents upon the human constitution, etherization *does* possess the desired and entire effect of abolishing and annulling the pain following the scalpel and saw of the surgeon. But, as I have already stated, a question of the most important moment remains behind. Many surgeons, who cannot gainsay the indisputable effects of ether-inhalation in cancelling the pains of surgical operations, still ardently maintain that this admitted amount of present good can only be purchased and obtained by the patient, at the hazard or certainty of a greater and disproportionate amount of future evil. In other words, they allege and maintain that the condition of etherization is one which, from its marked powerful effects upon the economy, must produce, in some cases, immediately dangerous results, which must affect the system, so as to impede and interfere with the condition of wounds and the recovery of patients from them, and hence that it will render operations more perilous in their consequences, and more fatal in their ultimate results.

The correctness or incorrectness of this last feasible and assuredly most formidable objection to etherization in surgery, is a problem that no mere reasoning or mere opinion could ever certainly and satisfactorily solve. The evidence of simple prejudgment and argument could never perfectly settle it, however plausible and ingenious the grounds of the prejudgment and argument might be. It is one of those allegations, the accuracy or inaccuracy of which is a matter that can be fully and finally determined by one method only,—namely, by an appeal to the evidence of facts, and to the evidence of facts alone. For the purpose of assisting in the decision of this question, I have, through the great kindness of my professional brethren, collected the results of above three hundred amputations of the thigh, leg, arm, and fore-arm, performed within the last six months upon patients in an etherized state, in the civil hospitals of England, Scotland, Ireland, and France. The statistical analysis of these three hundred amputations with ether, and the comparison of *their* results with the results of various similar collections of the same amputations without ether, in the same and in other similar hospitals, will, I believe, enable us to arrive at some more definite ideas and deductions than we are yet in possession of, in regard to the debated question of the danger or safety of etherization in the operations of surgery. But let us first inquire if this statistical method is the proper method of investigating such a subject.

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[1] Abstract of the Medical Sciences, by Dr. Ranking, vol. v. 1847, p. 383.

CHAPTER V.

VALUE AND NECESSITY OF THE NUMERICAL METHOD OF INVESTIGATION AS APPLIED TO SURGERY.[1]

"La possibilité de l'application de la statistique à la médecine, est une verité tout aussi bien demontrée que la realité de la circulation."—Dezeimeris, Dictionnaire de Médecine, vol. xxviii. p. 550.

THE vast practical importance of the doctrine of statistics, and its power of elucidating, simplifying, and deciding many and various inquiries in surgical and medical science, is now becoming daily more and more acknowledged by the members of the profession. The doctrine itself has been long, not only acknowledged, but acted upon by governments and by the public at large. The political laws and expensive machinery pertaining to the registration of the deaths and diseases of the inhabitants of England, and of other kingdoms of Europe, are founded upon the soundness of the doctrine. In our numerous life assurances and annuity companies, millions of money are unhesitatingly staked upon the truth of it. And the principle upon which the usefulness and stability of the whole doctrine of medical statistics rests, is a very simple one. It amounts to

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[1] From Edinburgh Monthly Journal of Medical Science, Nov. 1847, p. 13.

iis: Among facts, data, or unities of a variable chance—such
i the probabilities of death within a given time, or the probabi-
ties of attacks of particular diseases within a given time, or the
obabilities of averting death in particular diseases by particular
ethods of treatment, or operation—there is ever a mighty uncer-
inty as to the results, if we consider only single cases, or a small
id limited number of instances; but our results approach more and
ore to certainty, in proportion as we deduce these results from a
eater and more extended number of instances—from a larger and
ultiplied series of facts. There is always great uncertainty and
stability in regard to the results of single or isolated cases; but a
oper aggregation and conjunction of cases affords results which
e comparatively certain and stable.

In the present investigation into the effects of etherization upon
e mortality attendant on surgical operations, I have followed the
atistical method of inquiry. But as the doctrine itself of statistics, as
pplied to such questions, is still, I fear, very imperfectly understood
y the profession in this country, I shall here take the liberty of
remising a few observations upon this mode of inquiry.

I have already stated, that the great and leading principle upon
hich all statistical inquiry is grounded, consists in the fact, that in
nities or entities of a doubtful chance, while the result, or event,
n *individual* instances, is ever variable and uncertain, the result, or
vent, when calculated from, or upon *masses* of instances, becomes
omparatively certain and invariable. I shall show the truth of this
bstract remark, in the form of illustrations, of a few of the fundamental principles or propositions upon which the doctrine of medical
tatistics is founded, and this more especially with a view to the
earings and important advantages of the statistical or numerical
nethod of inquiry as applied to questions in surgery.

FIRST PROPOSITION.

The absolute number of deaths from all causes, in a given time, in a given population, is always nearly the same.

The probability of life or death to individuals within the limits of
given period, is proverbially uncertain. Nothing is more uncer-
tain, for instance, than the number of individuals that will die in
he currency of a single year in any particular family, street, or
illage in England and Wales. But nothing could be more cer-
tin than, *cæteris paribus*, the number that will die during the cur-
ency of a single year in the whole of England and Wales. Esti-
nating, for instance, as we do in *all* modes of reasoning and
hilosophizing, from the experience of the past what will be the
xperience of the future under similar circumstances, we may state

beforehand as certain, that in 1845 (the results of which have not yet been published) the total number of deaths in England and Wales amounted to about 350,000. For the returns of the Registrar-General for England and Wales have now been collected and published for seven years—viz., from 1838 to 1844 inclusive, and the total numbers that died during each of these years were fixed and determinate, to the extent shown in the following table:

No. 1.—*Table of Absolute number of Deaths in England and Wales and of the percentage of Deaths among the whole population during the seven successive years from* 1838 *to* 1844 *inclusive.*

Year.	Absolute number of Deaths.	Percentage of Deaths among the Population.
1838	342,547	2·2 in 100
1839	338,979	2·1 in 100
1840	359,634	2·2 in 100
1841	343,847	2·1 in 100
1842	349,519	2·1 in 100
1843	346,446	2·1 in 100
1844	356,950	2·1 in 100

In reference to the preceding table, we must bear in recollection one point, that in this, as in other statistical inquiries, there is always a range of oscillation, and limits of possible error; but, as Gavarret has well demonstrated, the extent of these oscillations and limits of possible error are themselves easily ascertainable, and capable of being reduced to mathematical calculation and correction.

SECOND PROPOSITION.

The absolute number of deaths from individual diseases and specific causes in a given time, in a given population, is always nearly the same.

For if it be true, as shown under the first proposition, that the exact number dying annually in England is nearly the same, it is equally true and demonstrable that the particular causes or forms of disease producing these deaths recur in successive years in the same number and proportion. What is true regarding the whole, is true in regard to its parts. For the purpose of illustrating this secondary fact, I shall take from the Registrar-General's reports nine returns, three referring to medical, three to surgical, and three to obstetric affections and complications; and I shall add one pertaining to the department of medical jurisprudence. Each of them shows the comparative certainty of large numbers. For while, for example, no man could predict who or what number of a small community would die annually of croup, or tetanus, or ovarian dropsy, yet the absolute number dying each year of these and other affections throughout England, when calculated on a large scale, mes annually, in all except epidemic and zymotic diseases, to be arly the same, as the following table sufficiently demonstrates. fact, their numbers are, if possible, more determinate than the mbers of the total deaths; because, while the absolute mortality a kingdom is liable to be varied by variations of a temporary and nsient nature in the existing epidemic and endemic influences, ., those individual diseases and causes of death, the etiology of ich is more fixed, are more stable in their results:

No. II.—*Table of Absolute number of Deaths annually in England and Wales from twelve different Diseases or Causes of Death.*

Causes of Death.	No. Dying in 1838.	No. Dying in 1839.	No. Dying in 1840.	No. Dying in 1841.	No. Dying in 1842.
Croup, ...	4463	4192	4336	4177	4457
Jaundice, ...	841	800	875	864	952
Apoplexy, ...	5630	5293	5451	5581	5361
Hernia, ...	507	474	480	475	529
Tetanus,.	129	122	142	118	118
Carbuncle, ...	35	38	33	28	40
Childbirth, ...	2811	2915	2989	3007	2687
Malformations, . .	166	214	211	206	217
Ovarian Dropsy, . .	45	34	43	44	52
Violent Deaths, . .	11,727	11,632	11,594	11,100	11,092

The regularity with which the same disease thus destroys in successive years, nearly the same number of individuals, may appear markable to those who have not given attention to the study of dical statistics, and who have, consequently, not marked the fixed d determinate nature of the results which this means of investition always elicits, when it is enabled to work upon a sufficiently ge basis of facts, or a sufficiently large series of data. But this nstancy appears, if possible, still more singular, when we return such a subject as that included under the last column in the table ., "violent deaths." Under this head are included deaths by chanical injuries, by chemical injuries, by asphyxia, &c., and, if returns were more specific, it would no doubt be found that the mber of violent deaths from *each* separate division of causes was nually nearly the same. Even causes originating in passions of human mind, and leading to violent death by murder are, *cæteparibus*, repeated in nearly the same number in each successive ar. The moral man is subject to laws as fixed as the physical n. Some years ago, Quetelet showed, from the comparison of the nual number of deaths in Paris, and the annual number of crimes mmitted throughout France, that the statistics of human crime as fixed as the statistics of human mortality; that each age paid as uniform and constant a tribute to the jail as it paid to the tomb; that the numbers of any specific crime in successive years was, like the numbers of deaths from any specific disease in successive years, always nearly the same; ay, that the very instruments by which the same crime (as murder) was perpetrated in different years, were always in nearly the same proportion.[1]

THIRD PROPOSITION.

The absolute number of those that recover should, cæteris paribus, *be as fixed as the number of those that die, from individual diseases in a given population.*

The preceding table (No. II.) shows how many died of the several affections included under it, during a succession of years. If our statistics were more specific and detailed, we ought to be able to tell also how many recovered each year from attacks of each of these affections, as well as how many died from them; and if we could thus count the number of recoveries as well as the number of deaths by striking the proportion between them, we should obtain the average mortality of each disease. The deaths, for instance, from croup, amount on an average to 4325 each year. But if at the same time we knew the total average number of cases of croup that occurred every year (say, for the sake of illustration, that they amounted to 13,000 in all), then the mortality of the disease would amount to nearly 1 in 3; or out of every three patients attacked with croup, two would recover and one die.

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[1] "If all human actions could be registered," says Quetelet, "it might be supposed that their numbers would vary from year to year as widely as human caprice. But this is not what we in reality observe, at least for that class of actions of which we have succeeded in obtaining a registry. I shall quote but a single example; but it merits the attention of all philosophic minds. In everything which relates to crimes, the same numbers are reproduced; so constantly, that it becomes impossible to misapprehend it—even in respect to those crimes which seem perfectly beyond human foresight, such as murders committed in general at the close of quarrels, arising without a motive, and under other circumstances to all appearance fortuitous or accidental. Nevertheless experience proves that murders are committed annually not only pretty nearly to the same extent, but even that the instruments employed are in the same proportions."—Treatise on Man, p. 6. The following table, abridged from Quetelet, may enforce still more the truth of his observations.

Table of the Annual Total Number of Murders, and Instruments of Murder, in France, collected from the Reports of Criminal Justice, from 1826 *to* 1831.

Modes of Murder.	In 1826.	In 1827.	In 1828.	In 1829.	In 1830.	In 1831.
Total Number of Murders, .	241	234	227	231	205	204
By Gun and Pistol, ...	56	64	60	61	57	44
By Knife, ...	39	40	34	46	44	24
By Stones, ...	20	20	21	21	11	9
By Kicks and Blows, ...	28	12	21	23	17	25
By Cudgel, Cane, &c., ...	23	28	31	24	12	21
By Stabs, Cuts, and Bruises, .	35	40	42	45	46	49
By other means, ...	40	30	18	11	20	39

The difference in 1830 and 1831 from the preceding four years was no doubt owing, in a great degree, to the Revolution of 1830 and its immediate effects.

The Registrar's reports, however, do not furnish us with returns f the recoveries as well as of the deaths in any special disease or mplication, with one exception. Under the division of births he ives the number of children born each year, and under the term ildbirth, in the division of deaths, he gives the number of mothers ho perished each year under parturition or its consequences. eing thus furnished not only with the annual total number of deaths at took place from parturition, but also with the annual total mber of cases of parturition that occurred, we have the data in is instance for calculating the proportion of recoveries to the pro- rtion of deaths in childbirth. And the following table presents e results for the only four years of which the full data have been t published.

No. III.—*Table of Proportion of Deaths in Childbed in England and Wales, from* 1839 *to* 1842.

Years.	No. of Children Born.	No. of Mothers dying in Childbed.	Proportion of Maternal Deaths in Childbed.
1839	492,574	2915	1 in 169
1840	502,303	2989	1 in 168
1841	512,158	3007	1 in 170
1842	517,739	2687	1 in 192[1]

FOURTH PROPOSITION.

atistics enable us to prove that the general mortality, the mortality in particular departments of practice, and the mortality from individual diseases, are capable of being altered by altering the attendant circumstances.

Under the three preceding propositions, in showing the attendant esults to remain from year to year the same, I have supposed the ttendant circumstances to remain also the same. But if we change he conditions in which the community or the individuals composing t are placed, we change also the results. The great power which rt possesses, is the power of altering these attendant conditions and ircumstances by improved hygienic measures, improved medical ractice, improved modes of operating, &c. And the effects of this lteration might be interminably questioned and doubted, provided e had not the power of proving it by simple statistical evidence. shall adduce some examples.

Improve, in regard to salubrity, &c., the circumstances in which a community is living, and you increase the value of life in that community, or diminish its mortality. Thus, in 1786, the yearly rate of mortality for the whole of England and Wales was 1 in 42; or, in other words, 1 in every 42 of the inhabitants died annually. In 1801, this mortality was found to be 1 in 47. In 1831, it had diminished to 1 in 58; showing a difference of 38 per cent. in the short period of half a century.[1]

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[1] In relation to the etiology and pathology of puerperal fever—the most common cause death in childbed—it is not uninteresting to remark, that in 1842, when the deaths fol- wing parturition were so few, the number of deaths from erysipelas was also much minished in number. This fact may, I think, be adduced as a reason additional to the any lately urged in proof of an identity in causation and character between erysipelas and erperal fever.

Again, if we wished to prove that our practice, in relation to any particular department of the profession, was more successful now than formerly, or more successful under one mode of treatment than under another, our only certain form of evidence consists in a similar appeal to statistics. Mere assertions and opinions avail little in such a question. Figures and statistics can alone properly decide and determine it. And here, as elsewhere, they offer a kind of evidence, which is not less remarkable for its intelligibility and simplicity, than for its precision and certainty. I shall again take the case of childbirth as an example. The following table, calculated from the mortality bills of London, and given in a more extended form by Dr. Merriman,[2] demonstrates statistically, what could not be satisfactorily proved by any other form of evidence, that the practice of midwifery has become more and more improved in London, and inferentially in the whole kingdom, during the last two hundred years; the ratio of deaths in childbirth having gradually become less in number, and that to such a degree, that the proportion of parturient mothers lost, during the last years of the seventeenth century, was nearly double the number of those lost during the first years of the nineteenth century:

No. IV.—*Table of average number of Mothers dying in Childbed in London from* 1660 *to* 1820.

Years.	Proportion of Mothers lost.
For 20 years ending in 1680, ...	1 in every 44 delivered.
For 20 years ending in 1700, ...	1 in " 56 "
For 20 years ending in 1720, ...	1 in " 69 "
For 20 years ending in 1740, ...	1 in " 71 "
For 20 years ending in 1760, ...	1 in " 77 "
For 20 years ending in 1780, ...	1 in " 82 "
For 20 years ending in 1800, ...	1 in " 110 "
For 20 years ending in 1820, ...	1 in " 107 "

Further, supposing we desired to prove, in regard to any one complication or disease, that some particular mode of treatment or of operation was more successful than another, we can resort to no other definite mode of decision than statistics; and no other known mode of investigation could yield the same simple and satisfactory results. In illustration, let me adduce an instance from obstetric surgery. The normal conjugate diameter of the brim of the pelvis is four inches. Sometimes, however, it is morbidly contracted. When, as occasionally happens, this bony canal is so much diminished in ize that its opposite walls are not more than from two and a half to three inches distant from each other, the mutilation and destruction of the child's head by craniotomy was formerly supposed, by British accoucheurs, to be the only proper, or indeed possible, mode f delivery. Latterly, after great and strenuous opposition, a revoution in practice has taken place in such cases; and, at the present ay, the artificial induction of premature labor at the seventh month the established rule of treatment. The child's life has thus cerinly been saved, in many instances, from otherwise inevitable estruction. But is the life of the mother not placed, as has been ometimes alleged, in greater jeopardy by it? Statistical evidence nswers the question by showing, that while craniotomy is fatal to he mother in about one in every five cases, the induction of premaure labor is not fatal to her life in more than about one in every fifty ases. Nor would it be possible to place such a question beyond he possibility of doubt except by statistics, built upon a proper and ufficient basis of data. Mere impressions and arguments would ot solve the problem. Dr. Osborne declared, for instance, that craniotomy was rarely fatal; and Madame Lachapelle described the induction of premature labor as rarely safe. Statistics have amply belied both opinions.

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[1] Dublin Review, vol. ii. p. 97.

[2] Synopsis of Difficult Parturition, p. 343.

FIFTH PROPOSITION.

Statistics offer a test by which the impressions of unrecorded and limited experience are corrected; and they furnish a mode of investigation capable of resolving many existing practical problems in surgery.

It is only since statistics began to be applied to surgical investigations, that surgeons themselves seem to have become aware of he excessive rate of mortality accompanying most of their capital perations. As long as the mere impressions of cases upon the memory were depended upon, and the individual cases or facts hemselves not noted or counted, the most erroneous opinions preailed regarding the rate of mortality following upon surgical pracice. All conclusions drawn from the memory are, observes Malaigne, "horribly fallacious (*horriblement infidèles*), and it is," he dds, "to their employment, that we owe the astonishing delusions lmost generally professed regarding the real danger or fatality of mputations."[1]

In his System of Surgery, Mr. Benjamin Bell, one of the most steemed surgical practitioners and writers of his day, in some general remarks upon amputation, observes, "In the present improved state of the operation, I do not imagine that one death will happen in twenty cases, even including the general run of hospital practice; and in private practice, where due attention can be more certainly bestowed upon the various circumstances of the operation, the proportion of deaths will be much less."[1] But in 1844, Mr. Inman collected the statistics of 3586 cases of "amputations generally, including secondary, primary, for accident or disease;" most from hospital, but some from private practice, and he found that out of these 3586 cases, there died 1146, or one in every $3\frac{1}{10}$.[2] In a very valuable communication, Mr. Fenwick has published a collated table of 4937 amputations. Out of these 4937 cases 1565 died, or the operation was fatal in the proportion of 1 in every $3\frac{1}{15}$ submitted to it.[3] The late statistical investigations of Phillips, Lawrie, Malgaigne, Gendrin, &c., have all fully borne out the same view with regard to the great mortality attendant upon amputations.

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[1] Archives Générales de Médecine for April, 1842, p. 391.

Nor are opinions formed from a single accurately observed case, or a very limited number of data, to be depended on as the ultimate probable measure of the value or fatality of an operation. An impression from an individual case often leads us to form a wrong estimate of the average danger or average safety of an operation, while adequate statistics at once show us the truth. Mr. Pott[4] saw amputation at the hip-joint performed in one case, and from that case drew the general deduction, that in this operation the want of success would be uniform. Mr. Syme, after performing amputation at the hip-joint in one case, from that case drew the opposite deduction. "I firmly believe,"[5] says he, "that if the operation be done properly, and above all, quickly, its success will be general, if not uniform." In his excellent System of Surgery, Professor Fergusson of London, depending upon his impression of cases, but not actually counting them, observes in regard to amputation at the hip-joint, "Although no reasonable practitioner would ever attempt it except as a last resource, it is somewhat satisfactory to know, that with all the disadvantages under which it has been performed, particularly in military practice, and notwithstanding the fearful shock that must of necessity attend such an extensive mutilation, the success of the operation has probably been such that 1 patient out of every 3 on whom it has been performed has been saved."[6] But when we turn from such conclusions to the simple evidence of numerical facts, all of these three opinions are seen to be erroneous; and in addition, the mind at once obtains a precise and definite idea of the degree of danger attendant on the operation when we simply appeal to statistics and find, as Mr. Sands Cox has shown,[1] that out of 84 cases in which amputation at the hip-joint has now been performed, 26 were successful, and 58 unsuccessful; or out of every 10 operated upon, 7 died and 3 recovered.

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[1] A System of Surgery, by Benjamin Bell, vol. vii. p. 254 of 7th edition.

[2] Lancet for 5th October, 1844, p. 39.

[3] Monthly Journal of Medical Science for October, 1847, p. 238.

[4] Chirurgical Works, vol. iii. pp. 217, 218.

[5] Edinburgh Medical and Surgical Journal, vol. xxi. p. 27.

[6] Practical Surgery, p. 362. The rate of mortality which Mr. Fergusson here attributes to amputation at the hip-joint, is in fact the rate of mortality belonging to all the minor ampu-

Now the certainty and correctness of the knowledge which we obtain in this and similar instances from merely and simply *counting* up a hundred accurately recorded cases, is infinitely superior to a hundred separate opinions and arguments upon the matter. A hundred writers upon such a subject as the probable degree of fatality accompanying amputation at the hip-joint, would no doubt give us every variety and confliction of opinion on the subject. A hundred cases of it correctly noted and counted, would give us a result not admitting of any variety or confliction of opinion, except it were objected that the numbers on which the calculation was founded were too small for a perfect conclusion; and this objection can always be met by collating additional data in order to extend the basis of our calculation, and thus remove and free it from this chance of possible error.

Upon my own mind, the strongest conviction is impressed, that the numerical or statistical method of inquiry is yet ere long destined to advance and promote surgical science, by revolutionizing some departments of surgery, by rectifying a number of its existing errors, by clearing away many of its doubts and difficulties, and by settling and determining for it definitely, various of those practical questions upon which the opinions of the best operators are constantly and ever changing. In the introductory remarks to his last work on surgery,[2] the late Sir Charles Bell remarked, "Men's opinions go to extremes; they vibrate like the pendulum." But the application of statistics to surgery will betimes impart greater precision, and accuracy, and stability to its opinions; for it forms, I believe, the simple, and, at the same time, the only possible means of deciding numerous doubtful and disputed questions in the practice of it. It has, for example, been much and long debated whether the circular or the flap method of amputation is the safer and the better mode of operating. The most opposite sentiments are still expressed, and the most opposite practices still prevail in reference to this point. Some sur-

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ations of the limb, taken as a whole, as has been shown in the preceding paragraph. The actual fatality of amputation at the hip-joint is above 2 in 3, instead of being 1 in 3.

[1] British and Foreign Medical Review, July, 1846, p. 112.

[2] Institutes of Surgery, p. 22.

geons and some surgical schools earnestly maintain the superiority of the one method, and others as strongly uphold the greater safety and greater propriety of the opposite plan. Even the same mind, with every anxiety to arrive at nothing but the truth, may repeatedly change, and at different times hold different opinions upon the same matter. But the question at issue between the flap and circular methods of amputation is principally this;—which operation least endangers the patient's life? Now this question is one which could be satisfactorily settled by statistical investigation, and no doubt will ultimately be so. Probably one or two thousand amputations of the limbs are performed every year by the hospital surgeons of the United Kingdom. If our object were to ascertain whether amputation of the thigh by the flap or circular methods were the safer as regarded the *life* of the patient, and our hospital surgeons were only to note carefully, and collect the results of this operation for a year or two, so that we should have the statistical returns of both operations and their results upon a sufficient number of unselected cases, we should thus become furnished with data, the mere counting up of which would show us, infinitely better than any argument, whether the two modes of amputation differed at all in their relative degree of fatality; and if they did differ, which was the most dangerous of the two; and what was the degree of the comparative excess of danger of the one over the other. By the same form of statistical inquiry upon the same or other cases, and by analyzing or decomposing each separate case into as many parts as it contained distinct objects, we could further ascertain and determine all the various minor points, such as—which operation required the shortest period of convalescence—which ultimately secured the best form of stump, &c. &c.

And here I would beg to add one observation relative to the probable future importance and bearing of statistics upon surgery. It will be found that, in most of the past literature of their profession, surgeons have almost invariably contented themselves with recording their own deductions from their own cases, without recording the cases themselves. They have left us their inferences, but have not left us the grounds and bases of these inferences. They have generally given us, not their individual cases or individual facts, but the opinions which they themselves thought fit to draw from these facts.[1] The result has been, that, in numerous instances, inferences of the most erroneous and contradictory kinds have been drawn, in consequence merely of the elementary facts observed and

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[1] In the past records of midwifery, on the other hand, we have all their important *individual* facts and cases left on record for us, in the works of Mauriceau, Portal, Giffard, Smellie, &c. &c.

neralized upon, being far too few for the establishment of a cor-
ct deduction. I have already offered an instance of this in the
ro opposite opinions expressed by Mr. Pott and Mr. Syme regard-
g amputation at the hip-joint, from single cases observed by each,
contrasted with the actual and ascertained degree of danger con-
cted with that operation. A sufficient series of individual facts,
llected from the practices of several different surgeons, may thus
int out a deduction quite at variance with the so-called experience
d opinion of the individual authors themselves. Few surgeons
llow that strangulated hernia, when properly operated upon, is
ery fatal in its results. "The operation," says Mr. Pott, "if applied
in time, very seldom fails; so seldom, that I believe I might
enture to say, *not* 1 in 50 dies of it if timely and judiciously exe-
ated."[1] But, out of 77 cases recorded in Sir Astley Cooper's
ork on hernia, 36 died; out of 183 operations for this disease,
ollected by Malgaigne, 114 proved fatal; out of 545 collected by
r. Inman, 260 died; or 1 in every 2 perished instead of 1 in 50.
gain, amputation of the thigh is fatal in nearly the same propor-
ion; or about 1 in every 2 or 3 who are subject to it dies. Out of
87 cases of this operation collected by Mr. Phillips,[2] 435 of the
atients or 1 in every $2\frac{2}{10}$ perished. Yet speaking of the degree of
anger accompanying amputation of the thigh, as apparently in-
rred from his own observations upon the point, Mr. Ormerod, in
late work on surgery, observes that, "considering the severity of
e operation, and extent of the injury done to an individual by the
moval of a limb above the knee, the success of the amputation
ere is very great; the patients are often placed under bad circum-
ances for operation, and their health very much reduced, yet death
om failure, after the removal of a limb for disease, is *very rare*
deed."[3] I repeat, that if, instead of dealing in vague and value-
ss generalizations and opinions of this stamp, surgical authors
ould only carefully note and record all their individual opera-
ons and results, with a view that betimes a sufficient collection
data might be thus gradually gathered together, in order to
ttle particular questions of surgical science or practice, then their
llated facts would, in determining such questions, be indescriba-
ly more valuable than their individual opinions. For the facts and
stimony of surgery would thus become *cumulative* and increasingly
nclusive upon any points on the investigation of which it was
eemed proper to direct the power of its evidence, instead of being

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[1] Chirurgical Works, vol. ii. p. 180.

[2] Medical Gazette for 1844, p. 805.

[3] Clinical Collections and Observations in Surgery, p. 135. London, 1846.

lost and frittered away, as at present, on the formation of a host of isolated opinions, which are too often not less perplexing from their contrariety than from the confidence and dogmatism with which they are severally advanced. It is interesting to reflect how much, in all of these respects, might soon be accomplished by proper and systematical annual reports from the great public surgical hospitals throughout the country, and at the same time it is distressing to consider what masses of valuable information are yearly lost from the mere want of such reports.

SIXTH PROPOSITION.

Statistical evidence alone enables us to ascertain correctly the effects of various minor conditions upon the Fatality of Operations—such as the influence of the age, sex, &c., of the patient; the special success of different operators, &c.

The results of surgical operations are, like the results of diseases, varied by age, sex, constitution, idiosyncrasy, &c. On the influence of these and other minor points, some surgeons may have been led to form and express opinions more or less correct; but it is only by employing the numerical or statistical method of examination that a perfect degree of accuracy of judgment can be possibly attained on such matters. Without statistics, all opinions on these points would have remained doubtful and undetermined; by statistics their influence can be at once discovered and measured, and that, too, by a kind of evidence which is at once simple and convincing. I shall adduce one or two points as an example of the whole.

Let us take the influence of *age* upon the results of the operation of lithotomy. Various late authors have published the ages of their patients, and reports, which might be reduced to show the influence of age upon this operation. I shall content myself with tabulating, for this purpose, the earliest of the kind ever published, viz., those of Cheselden. He has left records of the ages and results of lithotomy, in 213 cases operated upon by him at St. Thomas's Hospital.[1] Out of these, only 20 patients died, or the small number of 1 in $10\frac{13}{20}$. But I shall throw all the 213 cases into a tabular form; and it will be at once seen, from this view of Cheselden's recorded data, that the danger of lithotomy increases in a ratio progressive with the age of the patient.

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[1] Cheselden's Anatomy, p. 332. From the admirable researches of Mr. Edmonds and Mr. Farre, we know that the mortality of disease *in general*, and the mortality of individual diseases (as small-pox, &c. &c.) increases from puberty upwards, in a regular geometrical progression, and that the rate of increase is about three per cent. every year, or more nearly 34 per cent. every ten years. I believe that an adequate collection of data will very probably show that this same "constant" mortality regulates the degree of liability to death in lithotomy, amputation, and other surgical operations.

No. V.—*Table showing the influence of the Age of the Patient upon the Mortality of Lithotomy.*

Ages of the Patients.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Percentage of Deaths.
Under 10 years, . .	105	3	1 in 35	3 in 100
From 11 to 20 years,	62	4	1 in $15\frac{1}{2}$	6 in 100
From 21 to 40 years,	22	5	1 in $4\frac{2}{5}$	22 in 100
From 41 to 80 years,	24	8	1 in 3	33 in 100

Let us take another illustration from lithotomy of the capability of statistics, proving one more of these minor points, such as are alluded to in the general proposition. Without statistics, it would be difficult or impossible to demonstrate the influence of the mere *size* of the stone extracted, upon the results of the operation for its extraction. But by statistics it can be readily proved that the mortality of lithotomy rises higher and higher in proportion as the stone increases in size; and hence, in all probability, in proportion as the operation increases in severity and difficulty. The following table, calculated from the Norwich data furnished by Mr. Crosse, in his valuable work, affords the required numerical evidence for this generalization.

No. VI.—*Table of the Mortality of Lithotomy, calculated according to the different Weights of the Stone extracted.*

Weight of Stone.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Percentage of Mortality.
2 oz. and under, . .	648	65	1 in $9\frac{63}{65}$	10 in 100
From 2 to 4 oz., . .	46	23	1 in 2	50 in 100
From 4 to 7 oz., . .	9	5	1 in $1\frac{4}{5}$	55 in 100

Statistics in surgery have been objected to on the ground, that in combining cases, in order to arrive at a general result, we do not take cognizance of the superiority of the practices of individual operators. "We find," argues Dr. Bennett, "operations by different surgeons, and various experiences, all mingled together to produce one sum total.[2] The most skilful metropolitan surgeon is put on a par with the country practitioner, and the experience of long practice is of no more value than that of the tyro. It is well known that, even in one person's practice, he operates differently at different periods of time. Mr. Syme had well illustrated this with regard to lithotomy, and told us that, since his alteration of the method of its performance, his success had been much greater

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[1] Crosse, Treatise on Urinary Calculus, Appendix II.

[2] Of course this is necessary when we wish to ascertain the *general* average success of an operation in the hands of *all* surgeons, and not its *special* average success in the hands of any *individual* operator or operators.

than formerly. Yet, according to the reasoning of Dr. Simpson, all the operations must be added together, and those performed during the inexperience of youth and the senility of advanced life, must, as with Mr. Martineau, be put on the same level with the cases that form the boast of mature age, and the most perfect powers of mind and body."[1]

Now, in this as in other points, I believe that the statistical method of inquiry forms the means, and the *only* means, of enabling us to prove the very items which it is alleged that statistics lose sight of and conceal. Take, for instance, the identical examples adduced. The present *general* average mortality of lithotomy, as performed by *all* operators on subjects of all ages, is, according to Dr. Willis, about 1 in 8.[2] Out of 5900 cases collected by Mr. Inman, 765 patients died, or 1 in $7\frac{3}{4}$. Out of 14 cases operated upon by Mr. Syme, and recorded in his Surgical Reports in the Edinburgh Medical and Surgical Journal, vol. xxxiii. to vol. xxxix., 5 died, or 1 in $2\frac{4}{5}$. Since adopting his present plan of lithotomy, however, he had performed 17 operations in the hospital, of which two only have proved fatal, or 1 in $8\frac{1}{2}$. Now, this difference could not be educed or stated with accuracy in any other way than by figures, or by the statistical method; for by it alone can we determine the *special* averages of different operators, or of the same operator at different times. But "take care," observed Sir Astley Cooper, "how you draw any deduction from particular cases. I and many others have for a length of time met with extraordinary success in operating for the stone, when 4 or 5 unsuccessful cases in succession have come, which have generally brought down the result to the amount I mentioned, viz., that 2 in 15 die."[3] Mr. Martineau's practice afforded a curious illustration of the necessity of this caution.

In the 11th volume of the "Medico-Chirurgical Transactions of London," Mr. Martineau published an account of 74 cases in which he had performed the operation of lithotomy in the Norwich Hospital from the year 1804 to 1840.[4] Only 2 of these 74 died, or 1 in 37. We learn further, however, from a paper of Dr. Yelloly,[5] that Mr. Martineau operated in the same hospital on 73 additional cases, 147 in all. Out of these 73 additional cases, 15 died, or 1 in $4\frac{13}{15}$. And I repeat, that it is statistics only which could properly and fully prove to us this great *special* difference in the success of Mr. Martineau's practice at different periods. At the same time, however, the same case proves to us further, that if we wished to obtain not this *special* average of practice at a *selected* time, but the *general* average of all his practice at all times, it would amount to nearly the general average of most other operators. For out of his whole 147 cases, 17 died, or 1 in 8, which we have seen to be nearly the common degree of success in lithotomy, according to the investigations of Drs. Willis and Inman. The special average success of some operators has been greater than this. We have already seen that Cheselden, out of 216 recorded hospital cases, only lost 20, or 1 in 10½. The special average success of other operators has been less. Out of 356 Parisian cases collected by Dupuytren, 61 died, or 1 in 6. Such differences, I repeat, could never be proved and substantiated, unless by statistics.

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[1] Monthly Journal of Medical Science, October, 1847, p. 307.

[2] "The average mortality from lithotomy, on all hands, appears at present to be about one in eight."—Dr. Willis's Urinary Diseases, 1838, p. 347.

[3] Lectures on Surgery, p. 321.

[4] He records 84 cases in all, with 2 deaths; but 10 of these 84 cases had occurred in private practice.

[5] Philosophical Transactions for 1829, p. 63. "The whole number of Dr. Rigby's operations (in the Norwich Hospital) was 106, with 15 deaths; and of Mr. Martineau's 147, with 17 deaths."

SEVENTH PROPOSITION.

Statistics afford us in general the only true and ultimate "measure of value" of any proposed alternative operation, or of any new practice in surgery.

Every well-informed writer has naturally and almost instinctively recourse to this form of proof, when originating a new, or reviving a neglected operation. For example, in his able treatise on "Excision of Diseased Joints," Mr. Syme, in recalling the attention of surgeons to the operation of Park and Moreau, and in showing the advantages and safety of excision of the elbow joint, as compared with the alternative operation of amputation of the arm, most properly uses the following statistical argument: "I have," he remarks, "cut out 14 elbow-joints, and the operation has been performed in Edinburgh three times by other practitioners; of all these 17 cases, only 2 have terminated fatally; and in one of them the patient would, I believe, have died from any operation whatever, while, in the other, the disease was found so extensive as to render the excision almost impracticable. I believe the result of 17 amputations in similarly unfavorable constitutions would not be so satisfactory."[1]

Anatomical and pathological or other considerations may suggest to us the propriety or impropriety of any newly proposed condition or mode of operating; but an appeal to statistics is the only means of ultimately and definitely deciding upon its merits or demerits. For instance, surgeons were long afraid to place a ligature upon the carotid artery, fearing the difficulties of the operation, and the probabilities of danger to, and derangements in, the cerebral circulation. Statistics, however, show that this reasoning was so far unsound. Ligature of the carotid has now been performed above two hundred times.[1] Out of that number it has proved fatal in the proportion of about 1 in every 4 patients operated on; and consequently it has become an established operation in surgery,—many capital operations being more mortal than this.

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[1] Treatise on the Excision of Diseased Joints, p. 26.

After seeing the comparative safety with which ligature of the carotid and other large arteries was performed, surgeons, reasoning on these results, believed it would be justifiable to tie the arteria innominata. The results, however, have belied the *a priori* reasoning. Ligature of the arteria innominata has now been performed, according to Dr. Norris, in eleven cases. All the eleven patients died.[2] In three other cases, occurring in the practice of Post, Key, and Porter, the operation was commenced, but not completed. Two of these three patients died. In the third (Mr. Porter's), the idea of tying the artery was abandoned after it was exposed; the wound was closed up, and the patient recovered. And, doubtlessly, these statistical results will, ere long, compel surgeons to acknowledge this operation to be one which it is unjustifiable in them to practise.

Every newly proposed practice in surgery thus offers, as it were, a new problem for statistical solution. In the instance of etherization a small number of cases might be sufficient to satisfy any unprejudiced mind, that the etherized state was one in which the patient was saved from the pain of the surgeon's knife; but a small number of cases could not prove—any more than abstract reasoning could prove—whether etherization were or were not a safe practice as regarded the probabilities of the ultimate recovery of the patient after severe operations; whether, that is to say, it increased, diminished, or altered in any respect these probabilities. And hence the origin of the present communication. But before attempting to show the solution of this problem by statistics, let me premise in

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[1] I have collected the following table from an elaborate essay of Dr. Norris of Philadelphia on Ligature of the Carotid, &c., published in the 27th Number of the American Journal of the Medical Sciences:

Table of the Statistical Results of 203 Cases of Ligature of the Carotid Artery.

Cause for Operation.	No. of Cases.	No. of Deaths.
Aneurisms, ...	88	16
Wounds, &c., ...	30	15
Extirpation of Tumors, ...	18	6
Cerebral Affections, ...	6	0
Erectile and other Tumors, &c., ...	42	13
Brasdor's Operation, ...	15	4
Total, ...	203	54

[2] The operations in these eleven cases were performed by Mott, Graefe, Hall, Dupuytren, Norman, Bland, Lizars, Hutin, Arendt, Liston, and Kuhl.

another proposition an answer to the chief objections which have been urged against the method of inquiry that I have adopted.

EIGHTH PROPOSITION.

The objections of late years offered against the application of statistics to practical inquiries in surgery and medicine, seem altogether founded upon a misconception of the objects and principles of statistical investigation.

The application of the statistical or numerical method of inquiry to the solution and determination of questions in medical and surgical science, is of comparatively late date.[1] Like most other innovations, its introduction has been more or less strongly opposed;[2] and the principal objections which have from time to time been urged against the employment of it, have been the following:

1st Objection.—The numerical or statistical method consists of a calculation of probabilities. There is no doubt whatever of the truth of this allegation. But if it formed a valid objection against the application of statistics to medicine and surgery, it would form equally a valid objection against almost all other modes by which the human mind struggles to acquire increased knowledge, either in medicine or in any other department whatever of science and art. For, as the great French mathematician Laplace observes,—and I could not quote a greater and higher authority on such a point,—"To speak strictly, almost all our knowledge is but probable; and among the small number of things which we can know with certitude, in the mathematical sciences themselves, the *means* to arrive at truth are founded upon probabilities; so that the *entire* system of human knowledge is subjected to the theory of probabilities."[3]

2d Objection.—The numerical method calculates together as similar, facts which are not sufficiently similar to be a basis of calculation of probabilities. M. Double and others who have propounded this objection, have affirmed, that no two cases in medicine or surgery are entirely or exactly the same, and hence that they cannot be counted together as the same. But if this strange and illogical averment were true to the extent which its supporters maintain, and if every single case to which the physician was called, and every single operation which the surgeon performed, were an individuality and unity so dissimilar from all other previous cases of disease or operation which he had witnessed, as to be incapable of being grouped or classed in any way with them—then we could not possibly have any general facts, principles, or rules to guide us in the practical exercises of our profession. And, if medicine and surgery had no such general laws or principles, there would be necessarily an end to their existence, either as sciences or arts. Grant, however, that there are specific diseased states in medicine, and specific operations in surgery, with some general facts or rules applicable and peculiar to each disease, and each operation; then, it is further evident, that each of these general facts must have been originally founded upon a basis or deduction of particular facts—that the fundamental or particular facts must be always *more* or *fewer*—consequently capable of being counted—and, consequently, coming within the range of the numerical method of inquiry.[1]

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[1] See some historical notes regarding it in a paper by Mr. Marshall, in the Edinburgh Medical and Surgical Journal, No. 116.

[2] Our semi-civilized brethren of China, with their fixed hatred of all improvements and innovations, seem to have a particular dislike to statistics, and upon grounds amusingly similar to those of some members of the medical profession in Europe. "Moreover (says Mr. Fortune), they [the Chinese] cannot appreciate statistical inquiries; they always fancy we have some secret motive for making them; or that the subject cannot be of the slightest importance either to ourselves or others."—Three Years' Wanderings in China, p. 3.

[3] Essai Philosophique sur les Probabilités, p. 1, &c.

But it has been further objected, allowing that individual cases of disease may be arranged into groups or species, still the individual cases composing these groups are often so inaccurately observed as not to form a sufficiently true basis for statistical comparison and inquiry. This objection, however, applies to all other modes of medical investigation as well as to the numerical. There is precisely this very same difficulty to overcome, in whatever way or by whatever method we attempt to study and generalize upon diseases. There is this difference, however, the statistical method compels and exacts *more* care, and caution, and correctness in our study, and in our record of cases than other plans of generalizing; and certainly, *this* forms an argument in favor of the adoption of statistics, rather than an argument in favor of the rejection of them. For it is an acknowledged truth, in medical as in all the other sciences, that the greatest attainable degree of accuracy in our fundamental or elementary facts is necessary, that we may reach the greatest attainable degree of accuracy, and consequently of utility, in the general practical conclusions or laws which we venture to deduce from these facts. From time to time we are obliged, in *every* known form of medical reasoning and generalization, to revise our fundamental acts and change or modify our conclusions as our knowledge of pathoogy, diagnosis, &c., increases. The same holds true of the numerical method. And at present, the principal obstacle against applying tatistics, more fully than has been done, to some departments of he physician's study, confessedly consists in our occasional inability o make a perfect and undoubted diagnosis of some *internal* disases, and hence in the liability of our comparing and calculating toether cases that are not specifically similar. In statistics, however, as pplied to surgery and surgical questions, the same obstacle does not meet us, at least in the same degree. Supposing we wish, for instance, s will subsequently be my object in the present investigation, to alculate what proportion of persons die under particular amputaions, we only require perfect accuracy on three facts, each and all of which could certainly be noted, and by a little care, noted with perfect accuracy, and without much, if indeed any, chance of error. For they amount to these points in each case, viz.—1. Was ampuation performed? 2. In what part of the limb was it performed? and 3. After its performance, Did the patient live or did he die? In this and many other points of surgery to which the numerical method of inquiry is capable of being directed, all the necessary elementary matter could assuredly, with any common attention and accuracy, be readily collected without much probability or possibility of error. Of course, it is unnecessary to add that in this, as in all other modes of philosophizing, our observations and deductions must be pursued with stern and strict honesty, and with a view to the attainment of truth and truth only; and that, for this purpose, *all* the pertaining individual facts or cases must be always given, and always counted. There must be no omission; no concealment; no selection of any kind.

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[1] If the reasoning of M. Double were admitted, 'it would,' as Gavarret properly observes, "altogether strike down medicine from the position which it ought to occupy in the temple of human knowledge. What language," he adds, "can a physician address to his pupils, who will not see, anywhere, but individualities? On what ground can he recommend them such or such treatment for their patients, since they ought never to meet, in their practice, anything comparable with what their master has seen? According to this inadmissible hypothesis, medical experience would be a word without meaning; the student, who has never yet seen a patient, would necessarily know as much as the most perfect physician. For if the career of the latter and his predecessors is consumed in the sterile observation of a succession of therapeutic individualities, the healing art cannot but be composed of a series of isolated attempts, without a common tie, and from which it would be impossible to draw any general conclusion, or any precept for the future."—Principes Généraux de Statistique, p. 42.

3d *Objection.—The statistical method of inquiry is different from and opposed to the inductive method.* No opinion could be more erroneous. The numerical method is assuredly not opposed to that strict observation of individual facts, and that strict generalization of them, which constitute the double basis and essence of the inductive method; but the very reverse. 1. It demands in the same way the strict observation of individual facts; but it demands that the observation and record of them be made, if possible, with still greater care and accuracy than heretofore. 2. It educes in the same way the general principles or laws of practice from the comparison and analysis of these observed facts; but it enforces more rigorous accuracy than heretofore in the deduction of these principles or laws, in proportion as figures are more certain than memory, and actual enumerations more certain than general impressions. Its required mode of observation and mode of generalization are the same as in the common inductive method, only more rigid and hence, more rigorously correct. Its object is not to supplant but to supplement our former methods of inquiry—not to make them useless, but to make them more useful by making them more accurate. It is an instrument which enables us to draw our deductions, not only with greater simplicity, but also with greater truth and precision. "No man (says Bacon), be he ever so cunning or practised, can make a straight line or perfect circle by steadiness of hand, but this may be easily done by help of a rule or compass."[1] And those who maintain that the numerical method is different from and opposed to experience and induction, might as rationally argue, that when we try to ascertain the general fact of the rapidity of a patient's pulse, we employ two different methods when we attempt to attain the desired information *without*, and *with* the aid of a stop-watch. We here practise two methods that are not different or opposed to each other. They are logically the same in all respects. But the one method is relatively more accurate, precise, and determinate than the other. And in this as in other applications of the numerical or statistical method, the language we employ becomes at the same time simple and decisive. For if we say, for instance, that the beats amount to 100 a minute, our language is far more clear and simple than if we mentioned that the pulse was "*quick*," or "*rapid*," or "*frequent*," &c. We state a definite and intelligible fact, instead of using some comparatively indefinite and uncertain term which the very temperament of every speaker and hearer may interpret differently. The great aim and object of the statistical or numerical method of inquiry, as applied to surgery and medicine, is the determination of their general facts, or general laws, with the utmost attainable degree of accuracy; but, in truth, figures are not only the strictest and most correct way of *educing* their general facts; they form also the shortest and most correct way of *stating* or *expressing* them after they are educed.

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[1] See Bacon's Advancement of Learning, in Montague's edition of his Works, vol. ii p. 182.

CHAPTER VI.

ES ANÆSTHESIA INCREASE OR DECREASE THE MORTALITY ATTENDANT UPON SURGICAL OPERATIONS?[1]

> "Why dost thou whet thy knife so earnestly?
> . . . . . . Shylock must be merciful.
> On what compulsion *must* I? Tell me that."
>
> *Shakspeare's Merchant of Venice.*

IN two papers on Anæsthesia in Surgery, published in the Monthly
urnal of Medical Science for September and November, 1847, I
k occasion to discuss various points connected with the subject,
d more particularly dwelt upon the necessity of having recourse
the evidence of a large collection of statistics as the only proper
d legitimate method of determining the fact, whether the pre-
us superinduction of artificial anæsthesia increased, decreased, or
ered in any way the mortality attendant upon surgical operations.
ring the intervening period, various circumstances and engage-
ents have intervened to delay the publication of the following
quiry, the results of which were laid at length before the Medico-
irurgical Society of Edinburgh, in July last. From that time
to November, I continued to receive additional returns, all of
ich have been embodied in the Tables, pp. 510, 511, and 512.

Shortly after anæsthesia began to be employed in Surgery, its
eged beneficial or baneful effects were keenly discussed among the
embers of the profession; and principally, or entirely, upon the
sults of individual or isolated cases. Some eagerly and stoutly
ubted, *in toto*, the possibility of making operations painless; and
any who admitted its possibility, denied altogether its propriety,
the alleged ground of its increasing the general subsequent
ngers of the patient, including a variety of alleged morbid states
d lesions, and adding, on the whole, to the fatality of operative
rgery.

Amidst the many conflicting and contradictory assertions that
ere uttered on these points, I became convinced that there was
ly one method of arriving at the truth, viz., by instituting a sta-
tical investigation upon as large a scale as possible into the results
f the practice, and thus ascertaining whether, out of an extensive
ries of operations performed with and without anæsthesia, the mor-
lity was greater or was less when the patients were operated on
a narcotized and anæsthetic state, than when they were operated
in a waking and æsthetic state.

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[1] From Edinburgh Monthly Journal of Medical Science, April, 1848, p. 697.

The first difficulty to be encountered in such an inquiry was
difficulty of obtaining a proper field and standard for the propo
comparison. But *first* of all, it was evident that the comparis
whatever it might be, could only be properly instituted betw
patients operated on in public hospitals, with and without an
thesia. For we had nowhere published, nor did it seem poss
to obtain, any adequate comparative returns of the results of
rations from the surgical practice of private practitioners. Besi
hospital returns were preferable in this respect, that there existe
the whole, everywhere, undoubtedly a far greater uniformity betw
the hygienic and other collateral circumstances of patients oper
on in hospital than in private practice. *Secondly*, however, it
further evident, that in seeking and fixing upon a criterion
which we could compare the statistical results of surgical op
tions formerly performed without anæsthesia, with those now
formed upon anæsthetized patients, it was improper and imposs
to institute the comparison between *all* operations and report
operations in hospitals; for the severity and danger of the op
tions performed in, and reported from, different hospitals, diffe
immensely in their nature, and consequently in their results.
order, therefore, to obtain the primary requisite for a correct sta
tical inquiry—of having data of a *similar* kind and character
the proposed testing and comparison—it was necessary to se
and contrast the result of some one operation without ether, w
the results of the same one operation with ether. With this v
I selected the larger amputations of the limbs as the fittest field
which to conduct the proposed investigation; and I restric
myself to hospital amputations of the thigh, leg, arm, and f
arm, on account of their being everywhere performed in alm
the same manner, for the same causes, under the same circumstan
and on the same class of subjects; and because there already exis
extensive published researches, by Phillips, Lawrie, and Malgaig
into their absolute mortality, when performed under ordinary circu
stances and without anæsthesia, to aid us in satisfactorily determ
ing the nature of the results of the new practice of operating u
patients in an anæsthetic state.

Having thus fixed upon the mode of inquiry, I proceeded to ap
for returns from all the surgical hospitals of Great Britain and I
land that I could hear of, as likely to have employed anæsthesia
amputations. And I feel it quite impossible to return thanks
any adequate terms, for the very great politeness and kindness w
which my inquiries were answered on all hands.[1] In some hospit

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[1] In my letter of application, I stated, that " the effects, whether favorable or unfavora
of anæsthesia upon the ultimate recoveries of patients from surgical operations is su

thesia had not been tried, and I was consequently furnished with
ata; in others in which it was used, my correspondents were
at issue about its propriety; many were doubtful; some ex-
ed themselves strongly against it, and others strongly for it.
[was principally anxious to obtain the total results, believing
they would decide the question far more certainly than any indi-
al experience or individual opinion could. In Table No. I.
pp. 510 and 511), these results are given in a detailed form,
the names of the gentlemen who kindly reported each return
e.[1]

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of much doubt and uncertainty. We have as yet had no proper collection of data to
in whether the mortality of operations has been increased or not by patients being
under the influence of ether at the period of their performance. In order to deter-
s far as possible this important point, I have been induced to undertake the statistical
igation of the results of the larger amputations in cases where anæsthesia was employed
time of operation. Amputations have been selected for this purpose in preference to
operations, because they are, under all common circumstances, nearly and everywhere
and because the general average mortality accompanying most of the greater ampu-
is already known from the inquiries of Phillips, Lawrie, and others, and thus a ready
rd of comparison is afforded us. You would, therefore, oblige me by filling up the
ing table with any results, however few in number, of amputations in which ether
sed in your hospital. I especially wish to know *all* the deaths as well as all the
ries in these operations; and by thus collating, on the whole, a large body of statistical
I hope to be able to arrive at the same general results."

Copy of Form of Table sent.—" Results of Amputations performed upon Patients in an Etherised State in the —— Hospital."

Seat of Amputation.	Primary or for Injury.		Secondary or for Disease.	
	Total No. of Cases.	Total No. of Deaths.	Total No. of Cases.	Total No. of Deaths.
Amputation of Thigh, . . .				
Amputation of Leg, ...				
Amputation of Arm, ...				
Amputation of Fore-arm, . .				
Total, ...				

ay be proper to remark, that in answer to the returns, I had the results of twenty-four
tations of the fore-arm sent me, which are not included in the subsequent remarks in
t. Out of these twenty-four amputations ten were primary, with one death, and
en secondary, with two deaths. I have omitted them in the text, in consequence of
g that Mr. Phillips, in his standard of amputations, confines his returns to those of the
leg, and arm, and does not include those of the fore-arm.

No. 49 of the Table, the name of the hospital is not mentioned, as my correspondent
unately omitted to date his return. The Paris hospital returns of twenty-two cases
l0) are distributed according to the standard of Malgaigne; Dr. Burguières, in a note
, having stated that he was unable to give the exact number of these amputations
were respectively primary and secondary.

No. I.—*Table showing, in detail, the number of Individual Amputations and*

No.	Name of Hospital.	Name of Reporter.	Amputation of Thigh.			
			Primary.		Secondary.	
			Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary, .	Dr. Keith and Dr. Macintosh,	....	....	2	1
2	Bedford General Infirmary, . .	Mr. Hurst, . . .	....	....	3	0
3	Birmingham General Infirmary,	Mr. Amphlett, . .	....	....	1	0
4	Birmingham Queen's Hospital, .	Dr. Wright, . . .	....	....	1	0
5	Bristol, General Hospital, .	Dr. Lansdowne and Mr. Mason,	....	....	3	0
6	Bristol Infirmary, ...	Mr. Morgan, . .	....	....	1	1
7	Belfast Hospital,.	Mr. Moore, . . .	1	1	1	1
8	Cumberland Infirmary, . . .	Mr. Page, . . .	....	....	2	1
9	Chester Infirmary, . . .	Mr. Harrison, . .	....	....	....	....
10	Cork South Infirmary,. . . .	Dr. Tanner,. . .	....	....	3	0
11	Dundee Infirmary, ...	Dr. Monro, . . .	....	....	....	....
12	Dumfries Infirmary, ...	Mr. Borthwick, .	1	1	....	....
13	Derbyshire General Infirmary, .	Mr. Fox,. . . .	....	....	1	0
14	Devon and Exeter Hospital, .	Mr. James, . . .	....	....	2	0
15	Dublin Mercer Hospital, . . .	Dr. Jamieson, . .	....	....	1	1
16	Dublin Richmond Surg. Hosp.,	Dr. Hamilton, . .	....	....	3	1
17	Edinburgh Infirmary, ...	Mr. Miller, . . .	1	1	2	0
18	Edinburgh Infirmary, ...	Dr. Duncan, . .	2	2	4	1
19	Elgin Infirmary, ...	Dr. Paul, . . .	....	....	....	....
20	Glasgow Hospital, ...	Dr. Orr and Dr. Steele, . . .	....	....	8	4
21	Hereford Infirmary, ...	Mr. Wandby, . .	....	....	3	0
22	Hull General Infirmary, . . .	Mr. Craven, . .	1	0	1	1
23	Hants, Royal South Infirmary, .	Dr. Bullar, . . .	....	....	....	....
24	Ipswich Hospital	Dr. Durrant, . .	....	....	3	0
25	Leeds Infirmary	Mr. Hey,. . . .	....	....	....	....
26	London Hospital, ...	Mr. Curling, . .	1	0	4	0
27	Lond., St. Bartholomew's Hos.,	Mr. Haig, . . .	2	1	6	3
28	London, Univ. College Hosp.,	Mr. Liston and Mr. Cadge, .	2	1	5	0
29	London, King's College Hosp., .	Mr. Fergusson,. .	....	....	1	0
30	London, St. George's Hospital, .	Dr. Snow, . . .	....	....	12	2
31	London, Middlesex Hospital, .	Mr. Shaw, . . .	....	....	5	0
32	London, Charing Cross Hosp., .	Mr. Avery, . . .	1	0	2	1
33	London, Westminster Hospital,	Dr. Bird, ...	....	....	4	0
34	Leicester Infirmary, ...	Mr. Paget, . . .	2	0	....	....
35	Liverpool Northern Hospital, .	Dr. Bainbrigge, .	....	....	3	2
36	Liverpool Southern Hospital, .	Mr. Morris, . . .	....	....	1	0
37	Manchester Royal Infirmary, .	Dr. Reid, . . .	....	....	2	1
38	Newcastle Infirmary, ...	Mr. Greenhow, .	....	....	....	....
39	Nottingham General Hospital, .	Mr. Wright,. . .	....	....	3	0
40	Paris Hospitals, ...	Dr. Burguières, .	3	2	7	2
41	Perth Infirmary, ...	Dr. M'Farlane, .	4	2	1	0
42	Sussex County Hospital, . . .	Mr. Parson,. . .	1	0	3	0
43	Staffordshire, North Infirmary, .	Mr. Turner,. . .	....	....	2	0
44	Sheffield General Infirmary, .	Mr. Jackson, . .	....	....	3	0
45	Salisbury Infirmary, ...	Mr. Young, . . .	....	....	4	2
46	Stockport Infirmary, ...	Mr. Rayner, . .	....	....	....	....
47	Winchester County Hospital, .	Mr. Wickham,. .	....	....	3	0
48	Worcester Infirmary, ...	Mr. Sheppard,. .	1	1	4	0
49	. . . .	Mr. Stocker, . .	1	0	1	0
	Total, ...	. . . .	24	12	121	25

ults, in different Hospitals, upon 302 *Patients under Etherization.*

PUTATION OF LEG.			AMPUTATION OF ARM.				TOTAL CASES.			
ary.	Secondary.		Primary.		Secondary.		Primary.		Secondary.	
Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
....	5	0	....	....	....	....	....	....	7	1
....	....	....	....	....	1	0	....	....	4	0
0	1	0	....	....	2	0	2	0	4	0
0	3	0	....	....	....	....	1	0	4	0
....	3	0	....	...	....	....	....	....	6	0
....	....	....	....	....	....	....	....	....	1	1
....	1	0	....	....	1	0	1	1	3	1
....	2	0	1	0	....	....	1	0	4	1
....	1	0	....	....	....	....	....	....	1	0
0	2	1	1	0	....	....	2	0	5	1
....	1	0	1	0	....	....	1	0	1	0
....	....	....	....	....	....	....	1	1	....	....
0	....	....	....	....	....	....	1	0	1	0
....	1	0	....	....	....	....	....	....	3	0
....	....	....	....	....	....	....	....	....	1	1
....	....	....	....	....	1	1	....	....	4	2
....	1	1	....	....	....	....	1	1	3	1
0	3	0	....	....	....	....	3	2	7	1
....	....	....	....	....	1	0	....	....	1	0
1	5	2	2	0	4	3	7	1	17	9
....	....	....	....	....	....	....	....	....	3	0
1	2	0	1	0	1	0	5	1	4	1
....	2	0	1	0	1	1	1	0	3	1
....	....	....	....	....	....	....	....	....	3	0
0	1	0	....	....	....	....	1	0	1	0
2	1	0	....	....	....	....	4	2	5	0
1	4	1	2	1	1	0	6	3	11	4
....	5	1	....	....	1	1	2	1	11	2
....	1	0	....	....	....	....	....	....	2	0
....	7	0	....	....	1	0	....	....	20	3
0	1	1	....	....	1	0	1	0	7	1
....	....	....	....	....	1	0	1	0	3	1
....	1	0	....	....	1	0	....	....	6	0
....	1	0	....	....	....	....	2	0	1	0
....	4	3	....	....	2	1	....	....	9	6
0	....	....	1	0	1	0	2	0	2	0
....	2	1	....	....	....	....	....	....	4	2
2	....	....	1	0	....	....	3	2	....	....
0	1	0	1	1	....	....	2	1	4	0
1	5	2	2	1	2	1	8	4	14	5
....	....	....	....	....	....	....	4	2	1	0
1	1	0	1	0	....	....	5	1	4	0
....	2	0	....	....	....	....	....	....	4	0
....	2	0	....	....	2	0	....	....	7	0
....	3	0	....	....	....	....	....	....	7	2
....	2	0	....	....	....	....	....	....	2	0
....	....	....	....	....	2	0	0	....	5	0
....	4	0	2	1	....	....	3	2	8	0
0	....	....	....	....	....	....	2	0	1	0
9	81	13	17	4	27	8	73	25	229	47

GENERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM, WITHOUT ANÆSTHESIA.

Before attempting to determine whether the results in these anæsthetic amputations (Table No. I.) are, or are not favorable to the adoption of Anæsthesia in Surgery, let me in the first place state the results of the previous investigations that have been published by Phillips, Lawrie, and Malgaigne, relating to the mortality of these same amputations, when the same operations were performed without anæsthesia. In the year 1837, Mr. Benjamin Phillips brought before the Royal Medico-Chirurgical Society of London, a communication[1] on the results of the amputation of the thigh, leg, and arm, in different countries. From the collection of cases which he laid before the Society, Mr. Phillips concluded that the general mortality of these larger amputations amounted to 23 deaths in the 100 operations. The correctness, however, of his conclusions was called in question by the publishing committee of the Society, on the idea that the alleged mortality was too great, and he was recommended to investigate the subject more fully before proceeding to publish his observations. Further inquiry served only to satisfy him that his previous results were understated rather than overstated.

Subsequently, in 1844, Mr. Phillips published a table of a still more extensive series of cases.[2] This collection, however, includes the results of private as well as of hospital practice, "They are," says Mr. Phillips, "the whole, so far as I know, of the cases of amputation recorded in the periodical literature of this and other countries during the present century. I by no means," Mr. Phillips adds, "think that the results furnished by such data will fairly represent the mortality. I believe it will be *understated*, because successful cases are more likely than unsuccessful ones to find their way into print."

The table (No. II.), extracted from Mr. Phillips's second paper, shows in a summary way the results which he obtained from these sources.

No. II.—*Table of the Mortality of* 1369 *Cases of Amputation of the Thigh, Leg, and Arm*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh, ...	245	176	72	415	87	21
Leg, ...	204	88	43	231	61	27
Arm, ...	164	49	29	110	26	24
Total, . . .	613	313	51	756	171	23

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[1] Observations on the Results of Amputation in different Countries. Medical Gazette, vol. xxii. 1837–38, p. 457.

[2] Medical Gazette, vol. xxxiii. 1843–44, p. 804.

n the year 1840, Dr. Lawrie of Glasgow published an excellent er[1] on the results of amputations, with tables showing the rate nortality from amputation in the Glasgow Hospital, from the iod of its foundation in 1794 down to 1839. Dr. Lawrie's in- ies yielded an average mortality greater than that of Mr. Phillips, ıg as high as 36 per cent. The following table, made from data Ir. Lawrie's paper, contains the results of amputation of the h, leg, and arm, in the Glasgow Hospital:

o. III.—*Table of the Mortality of* 242 *Amputations of the Thigh, Leg, and Arm, in the Glasgow Hospital, from* 1794 *to* 1839.

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh, ...	35	27	77	92	19	20
Leg, ...	27	18	66	35	12	34
Arm, ...	36	18	50	17	3	17
Total, . . .	98	63	64	144	34	23

n 1842, a valuable series of papers on the statistics of amputation published by Professor Malgaigne in the *Archives Générales de decine*, his data being derived from the reports of the Parisian pitals. In these papers, Malgaigne enters largely upon the sub- t of the mortality of amputations. The following table, compiled m data in his returns,[2] exhibits a mortality still higher than that the Glasgow Hospital.

No. IV.—*Table of the Mortality of* 484 *Amputations of the Thigh, Leg, and Arm, in the Parisian Hospitals, from* 1836 *to* 1841.

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh, ...	48	34	70	153	92	60
Leg, ...	80	51	63	112	55	49
Arm, ...	30	17	56	61	24	39
Total, . . .	158	102	64	326	171	52

hese three tables of large collections of cases by Phillips, Lawrie, Malgaigne, may be properly considered as giving a correct idea he general mortality of these amputations in hospital practice, and

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[1] On the Results of Amputations. Medical Gazette, vol. xxvii. 1841, p. 394.

[2] Archives Générales de Médecine, vol. lviii. 1842, p. 40.

No. V.—*Table showing, in detail, the results of* 618 *Amputations, in* 30 *different British Hospitals, immediately before the introduction of Anæsthesia.*

No.	Name of Hospital.	Date of the Observations.	Name of Reporter.	Amputation of Thigh.				Amputation of Leg.				Amputation of Arm.				TOTAL CASES.			
				Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.		Primary.		Secondary.	
				Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
1	Aberdeen Royal Infirmary,	1841–1846,	Dr. Macintosh,	5	3	7	2	5	2	15	4	5	0	5	0	15	5	27	6
2	Bristol, St. Peter's Hospital,	1844–1846,	Dr. Rogers,	....	....	3	0	....	....	....	....	....	....	....	....	....	....	3	0
3	Bristol Infirmary,	1845–1846,	Mr. Morgan,	1	0	17	2	2	0	12	1	3	0	1	0	6	0	30	3
4	Bristol General Hospital,	1846,	Mr. Mason,	....	....	2	1	....	....	....	....	....	....	....	....	....	....	2	1
5	Berks Royal Hospital,	1840–1845,	Mr. May,	7	5	5	0	3	0	....	....	3	0	1	0	13	5	6	0
6	Cork Northern Infirmary,	1845–1846,	Mr. Bullen,	1	0	2	1	....	....	3	0	....	....	....	....	1	0	5	1
7	Cumberland Infirmary,	1845–1846,	Mr. Page,	....	....	4	1	....	....	2	0	2	0	....	....	2	0	6	1
8	Dumfries Infirmary,	1842–1846,	Mr. Borthwick,	....	....	6	1	....	....	2	0	....	....	1	1	....	....	9	2
9	Derbyshire General Infirmary,	1845–1846,	Mr. Fox,	....	....	5	2	....	....	6	0	....	....	....	....	....	....	11	2
10	Dundee Infirmary,	1844–1846,	Dr. Monro,	1	0	2	0	1	1	4	0	3	0	....	....	5	1	6	0
11	Edinburgh Royal Infirmary,	1840–1845,	Dr. Peacock,	8	8	35	13	4	3	12	2	5	4	2	1	17	15	49	16
12	Elgin Infirmary,	1844–1846,	Dr. Paul,	....	....	8	2	1	1	1	0	....	....	....	....	1	1	9	2
13	Glasgow Hospital,	1840–1844,	Dr. King,	11	10	14	6	14	5	6	1	7	4	....	....	32	19	20	7
14	Glasgow Hospital,	1844–1846,	Dr. Orr,	10	3	22	8	17	5	13	4	12	3	11	5	39	11	46	17
15	Hull General Infirmary,	1846,	Mr. Craven,	....	....	2	0	3	0	....	....	2	0	....	....	5	0	2	0
16	Inverness Infirmary,	1845–1846,	Dr. Manford,	1	1	....	....	....	....	....	....	1	0	....	....	2	1	....	....
17	Ipswich Infirmary,	1845–1846,	Dr. Durrant,	....	....	6	0	1	0	....	....	3	0	....	....	4	0	6	0
18	London St. Bartholomew's Hos.,	1846,	Mr. Haig,	2	1	7	2	3	0	4	1	3	0	3	0	8	1	14	3
19	London University College Hos.,	1841–1846,	Mr. Cadge,	5	3	16	7	1	1	15	2	1	0	7	1	7	4	38	10
20	Leicester Infirmary,	1845–1846,	Mr. Paget,	2	0	3	1	1	0	4	0	3	0	2	0	6	0	9	1
21	Liverpool Northern Hospital,	1845–1846,	Dr. Bainbrigge,	5	3	8	4	4	1	6	2	2	0	....	....	11	4	14	6
22	Liverpool Southern Hospital,	1846,	Mr. Morris,	1	1	....	....	2	1	....	....	....	....	1	0	3	2	1	0
23	Newcastle Infirmary,	1840–1846,	Mr. Benson,	7	3	10	2	8	2	10	3	10	2	2	0	25	7	22	5
24	Perth Hospital,	–1847,	Dr. M'Farlane,	....	....	13	5	....	....	7	2	....	....	2	1	....	....	22	8
25	Sussex County Hospital,	1844–1846,	Mr. Parson,	2	2	2	2	2	1	3	0	3	1	1	0	7	4	6	2
26	Salisbury Infirmary,	1845–1846,	Mr. Young,	2	0	3	0	3	2	3	1	1	0	1	0	6	2	7	1
27	Stockport Infirmary,	1845–1846,	Mr. Rayner,	2	2	2	0	....	....	1	0	2	0	....	....	4	2	3	0
28	Sheffield Infirmary,	1845–1846,	Mr. Jackson,	....	....	1	0	3	0	....	....	....	....	....	....	3	0	1	0
29	Worcester Infirmary,	–1846,	Mr. Sheppard,	....	....	5	0	1	0	3	0	4	2	1	0	5	2	9	0
30	York County Hospital,	1845–1846,	Mr. Hey,	....	....	1	0	1	1	3	0	2	1	1	1	3	2	5	1
	Total,			73	45	211	62	80	26	135	23	77	17	42	10	230	88	388	95

y be used with justice as subjects of comparison with any series cases similar to them in the whole series of circumstances, except t one whose influence upon the results is to be decided. After, vever, I began to collect the results and mortality of the same putations upon patients in an anæsthetized state from various tish and other hospitals, it was objected to the inquiry that it uld be unsatisfactory in two respects, viz., that the amputations npared were possibly performed in different classes of hospitals, l at dates so different that I did not consider in my investigation changes and improvements which might possibly have been induced into the very methods of operating.

n order, then, at once to enlarge the basis of data for compari- , and to obtain a series of cases still more exactly similar to the lection of anæsthetic amputations which I was making, I pro- ed from various British hospitals, through the kindness of different respondents, and from published data, returns of the latest am- tations that had been performed in them immediately previous the introduction of anæsthesia. These returns are given in detail page 514. All of the operations have been performed within the ht years, from 1839 to 1846 inclusive. By having this collection cases as an additional standard, I hoped to avoid all cavil on the ound of any supposed difference in the time, and other collateral rcumstances, in which the compared operations were performed. The data in the preceding table, No. V. (p. 514), when condensed to the tabular form, afford the results in the following table, o. VI.

No. VI.—*Table of the Mortality of* 618 *Amputations of the Thigh, Leg, and Arm, without Anæsthesia, performed during the last few years in* 30 *British Hospitals.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh, ...	73	45	63	211	62	29
Leg, ...	80	26	32	135	23	17
Arm, ...	77	17	22	42	10	24
Total, . . .	230	88	38	388	95	24

NERAL MORTALITY OF AMPUTATIONS OF THE THIGH, LEG, AND ARM, UPON PATIENTS IN AN ANÆSTHETIC STATE.

In the preceding lengthened Table, No. I. (pp. 510–511), I have en from forty-nine different hospitals the detailed reports of 302 putations of the thigh, leg, and arm. When these 302 amputations are reduced into a tabular form, similar to those which I have used for stating the data of similar amputations without anæsthesia, they present the following results :

No. VII.—*Table of the Mortality of* 302 *Amputations of the Thigh, Leg, and Arm, under Anæsthesia.*

Seat of Amputation.	PRIMARY.			SECONDARY.		
	No. of Cases.	No. of Deaths.	Percentage of Deaths.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Thigh, ...	24	12	50	121	25	20
Leg, ...	32	9	28	81	13	16
Arm, ...	17	4	23	27	8	29
Total, . . .	73	25	34	229	46	20

I shall now proceed to contrast these results with the results of the same operations in the same class of hospitals, and when performed upon patients not in an anæsthetic state.

Before doing so, however, let me observe in passing that the data I have adduced in Tables Nos. I. and V. (pp. 510–511, 514), have been objected to on the ground that they are collected from too many different hospitals, and too many different sources. But, on the contrary, I believe all our highest statistical authorities will hold that this very circumstance renders them more instead of less trustworthy. Professor Chomel of Paris, after pointing out the first requisite for a successful statistical comparison of therapeutic or other results, viz., a sufficient similarity between the number of collated cases—adds, as the second condition, "that the data be numerous, collected at different times, in different places, and if possible, by several observers. It is easily seen," he adds, "that the results of a number of facts too limited, collected in a short space of time, in a single place, and by a single observer, however exact as regards that individual series of data, may yet be very different from or even the reverse of conclusions drawn from a larger series, and one collected under various circumstances."[1]

COMPARISON OF THE MORTALITY FOLLOWING THE LARGER AMPUTATIONS OF THE LIMBS—1, WITHOUT, AND 2, WITH ANÆSTHESIA.

The major amputations of the limbs, including those of the thigh, leg, and arm, are generally fatal in hospital practice in the proportion of about 1 in every 2 or 3 operated upon. In the Parisian hospitals, the fatality according to Malgaigne, amounts to upwards

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[1] Bulletin de l'Acad. Roy. de Médecine. Séance du Mai 2, 1837.

'1 in 2. In Glasgow, it is 2½. In British hospitals, I found that ıder these amputations 1 in 3½ died. The same operations, per- rmed in the same hospitals, and upon the same class of patients, an anæsthetic state, presents a mortality of 23 in 100, or 1 in 4 only. ae following table shows the amount of the individual cases, and e percentage of deaths in different collections, with the corre- onding proportion of deaths in those operated on in an anæsthetic ate.

No. VIII.—*Table of the Mortality of Amputation of the Thigh, Leg, and Arm.*

Reporter.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Parisian Hospitals—Malgaigne, . .	484	273	57 in 100
Glasgow Hospital—Lawrie, . . .	242	97	40 in 100
General Collection—Phillips, . .	1369	487	35 in 100
British Hospitals—Simpson, . . .	618	183	29 in 100
Upon Patients in an Etherized State,	302	71	23 in 100

The evidence which the preceding table affords in favor of the eater safety of amputation with anæsthesia than without it, is suf- iently strong and striking. While 23 in 100 died under the am- itations named, when the operations were performed upon patients an anæsthetic state, 29 in every 100 died under the same amputa- ons in the same hospitals when the patients were not anæsthetized; -in the Glasgow hospital as many as 40 in 100 died; and in Paris, many as 57 per cent. In other words, out of every 100 persons ibmitted to amputations of the thigh, leg, or arm, the lives of six ere by the employment of anæsthesia, saved above the average umber of the same operations in British hospitals;—17 lives in ch 100 were saved, if we take the Glasgow returns as a standard f comparison; the average mortality was, under anæsthesia, less by 4 in every 100 cases than that which was found by Malgaigne to ccompany the same operations in the Parisian hospitals.

But probably, to most minds, this comparison would be rendered ore clear and simple, if we took not a class of operations, but a ngle operation as a standard and medium of comparison. For is purpose, let us select amputation of the thigh as the *individual* eration regarding which we possess the largest series of observa- ons.[1]

One objection may be urged against the comparison of the results of a single operation, th or without anæsthesia, that I am now about to institute, on the ground, viz.—that the mber of cases (145) is too limited to afford a result that is perfectly decisive. I am per- tly willing to admit the justness of this remark in a statistical point of view, and to hold s part (and indeed the whole of the present inquiry) as, so far, the commencement and cleus merely of a more full and lengthened investigation by other hands. At the same ie, I have, during the course of the inquiry, had the conviction impressed upon me, that ure results will more and more confirm those that I have here stated in the text, and be ll more in favor of etherization; for no small number of the operations reported to me

COMPARISON OF THE MORTALITY FOLLOWING AMPUTATIONS OF THE THIGH; 1, WITHOUT, AND 2, WITH ANÆSTHESIA.

There are few or none of the operations deemed justifiable in surgery, that are more fearfully fatal in their results than amputation of the thigh. "The stern evidence," says Mr. Syme, "of hospital statistics shows, that the average frequency of death is not less than from 60 to 70 per cent.,"[1] or above one in every two operated on die. Out of 987 cases of amputation of the thigh collated by Mr. Phillips, 435 proved fatal, or 44 in every 100 were lost.[2] "On referring," observes Mr. Curling, "to a table of amputations in the hospitals of London performed from 1837 to 1843, collected with care by a private society to which I have the honor of belonging (the Medical Society of Observations), I find 134 cases of amputation of the thigh and leg, of which 55 were fatal, giving a mortality of 41 per cent."[3] Out of 201 amputations of the thigh performed in the Parisian hospitals, and reported by Malgaigne, 126 ended fatally. In the Edinburgh Infirmary, 21 died out of 43. Dr. Lawrie found the mortality attendant upon this operation in the Glasgow Hospital to amount to 46 deaths in 127 cases. In the collection of cases from thirty different British hospitals which I have published in Table No. V. (p. 514), 284 cases of amputation of the thigh are reported; 107 out of the 284 operations proved fatal. On the contrary, I have collated 145 cases in which the same operation has been performed during the past year in British hospitals upon patients in an anæsthetic state. Out of these 145 cases of amputation of the thigh only 37 proved fatal. Or, in other words, the fatality was not greater than one in every four operated on when the patients were previously anæsthetized. It was as high as one in every two or three operated upon when the patients were not previously anæsthetized. The following table presents these results in a more clear form:

No. IX.—*Table of the Mortality of Amputation of the Thigh.*

Name of Reporter.	No. of Cases.	No. of Deaths.	Percentage of Deaths.
Parisian Hospitals—Malgaigne, . .	201	126	62 in 100
Edinburgh Hospital—Peacock, . .	43	21	49 in 100
General Collection—Phillips, . .	987	435	44 in 100
Glasgow Hospital—Lawrie, . . .	127	46	36 in 100
British Hospitals—Simpson, . . .	284	107	38 in 100
Upon Patients in an Etherized State,	145	37	25 in 100

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were, in the first periods of the new practice, doubtlessly performed upon patients in whom the anæsthesia was by no means entire and complete, in consequence of imperfection in the forms of apparatus, in their management, in the dose given, &c.; and, I believe, that as the profession becomes more accomplished and certain in the use of such measures, the resulting effects will become proportionally happier and more favorable.

[1] Monthly Journal for May, 1845, p. 337.

[2] Medical Gazette for 1844, p. 805.

[3] Address to the Hunterian Society of London, 1848, p. 31.

The preceding figures speak in a language much more emphatic than any mere words that I could employ in favor of anæsthesia, not only as a means of preserving surgical patients from pain, but as a means also of preserving them from death. Between even the lowest mortality in the table without anæsthesia, 36 in 100, and the rate of mortality with it, 25 in 100, there is the difference of 11 per cent. That is to say, according to this standard, out of every 100 patients submitted to amputation of the thigh without anæsthesia, 11 more would die from the operation than if the same 100 patients were submitted to the same operation in a state of anæsthesia. And if the condition of anæsthesia effects thus a saving of 11 lives in every 100 amputations of the thigh, then out of every 1000 such operations the lives of 110 patients would be preserved by the use of antipathic means.

If we compare these results with the standard of Mr. Phillips, the contrast is still more startling. Out of 987 amputations of the thigh collected by him, 435 proved fatal, or 44 in the 100. Out of 145 amputations of the thigh under anæsthesia, 37 proved fatal, or 25 in 100. According to this comparison, the amount of persons saved from death in amputation of the thigh by the patients being rendered anæsthetic during the operation, amounts to 19 lives in every 100 operations performed.

In conclusion, let me add, that when anæsthesia first began to be employed in surgical operations, it was eagerly argued that its adoption produced a greater tendency to primary and secondary hemorrhage, to imperfect union of the wounds, to pneumonia, &c. If my space had permitted it was my intention to show, from the analysis of the three hundred cases of amputation reported to me, that these various allegations were foundationless and imaginary[1]—that such consequences were not so frequent after amputations with anæsthesia as after amputations previously performed without it—that as the casualties were reduced in number, so were also the attendant accidents and complications.[2] But I believe such proof to be at the present day superfluous, as few or none now maintain such opinions. When writing to me as early as in June last on this subject, the late lamented Mr. Liston stated what all the subsequent experience of our ablest surgeons here and in London has confirmed. "The æther," says he, "produces no bad effect, as far as I can see. There is no change in the blood, nor in the vessels, or muscles. The recoveries are, at least, quite as good as before it was employed." An excellent surgical pathologist (Mr. Curling, surgeon to the London Hospital)[1] has more recently afforded still stronger testimony to the same effect. "I have carefully watched," says he, "the progress of cases, after operations of various kinds performed upon patients in a state of anæsthesia, and I can with confidence declare that, so far as my present experience has reached, the constitutional symptoms have been milder, and the cases have proceeded more satisfactorily, than after operations in which no means had been taken to prevent pain. Several of my surgical friends can fully confirm this statement."

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[1] Some of my correspondents, who expressed the strongest opinions in regard to the reality of these supposed evil consequences, have, I know, now abandoned such opinions as utterly untenable.

[2] In my communication to the Medico-Chirurgical Society, in June last, I went over this ground at some length.

I have also avoided entering into the theoretical question—How does anæsthesia render severe operations less fatal and dangerous in their consequences? I have already shown[2] that the endurance of severe pain is in itself depressing and destructive; and apparently the anæsthetic state saves the patient from this suffering and its effects, as well as saves him, in some degree, from the shock of the operation and its consequences. When writing, in 1839, on the subject of pain and shock, and on certain states connected with or produced by wounds or injuries, Professor Burns of Glasgow offered some remarks bearing directly on the present subject, and which are more valuable as they were written without any theory, and without any prospect of such a state as he speaks of being capable of being artificially induced. I shall quote them in his own words: "The mere lopping off of the member, by the immediate abridgment of the quantity of living body, the instant loss of so large a portion, which was formerly acting along with the system, is productive of serious evil to it, from the sympathy which universally prevails. But if the *nervous system become in part torpid, so as to prevent this sympathy*, or to be incapable of maintaining it, the loss of a member, or what is, in one respect, the same, the loss of its connection with the system, and its failure in power, and action, and sensibility, may not have the same bad effect."[3]

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[1] Address to the Hunterian Society of London, 1848, p. 23.

[2] Monthly Journal for September, 1847, p. 164; p. 484 of present volume.

[3] Principles of Surgery, vol. i. p. 493.

SECTION II.

ANÆSTHESIA IN MIDWIFERY.

> . . . "Not poppy, nor mandragora,
> Nor all the drowsy syrups of the world,
> Shall ever medicine thee to such sweet sleep."
> SHAKSPEARE.

CHAPTER I.

ON THE INHALATION OF SULPHURIC ETHER IN THE PRACTICE OF MIDWIFERY.

ABUNDANT evidence has of late been adduced, and is daily accumulating, in proof of the inhalation of sulphuric ether being capable, in the generality of individuals, of producing a more or less perfect degree of insensibility to the pains of the most severe surgical operations. But whilst this agent has been used extensively, and by numerous hands, in the practice of surgery, I am not aware that any one has hitherto ventured to test its applicability to the practice of midwifery. I am induced, therefore, to hope that the few following hurried and imperfect notes relative to its employment in obstetric cases, may not, at the present time, prove uninteresting to the profession.

Within the last month I have had opportunities of using the inhalation of ether in the operation of turning, in cases of the employment of the long and of the short forceps, as well as in several instances in which the labor was of a natural type, and consequently required no special form of artificial aid.

The first case in which I employed the ether vapor, occurred on the 19th of January. Some details of the result have been already published.[2] The pelvis of the mother was greatly contracted in its conjugate diameter from the projection forwards and downwards of the promontory of the sacrum; the lumbar portion of the spine was distorted; and she walked very lamely. The present was her second confinement. Her first labor had been long and difficult; she began to suffer on a Monday, and after a protracted trial of the

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[1] From Edin. Monthly Journal of Medical Science, March, 1847, p. 721.

[2] Ibid. Feb. 1847, p. 639.

long forceps, was at last delivered by craniotomy late on the subsequent Thursday night. Even after the cranium had been fully broken down, a considerable time and much traction had been required to drag the diminished and mutilated head of the infant through the contracted brim of the pelvis; and she was long in recovering. Contrary to the urgent advice of her medical attendant, Mr. Figg, he was not made aware of her present or second pregnancy till she had arrived at nearly the end of the ninth month. It was thus too late to have recourse to the induction of premature labor, which had been strongly pressed upon her as the only means of saving her child, should she again fall in the family way. The pains of her second labor commenced in the forenoon of the 19th. I saw her, with Mr. Figg, at five o'clock in the afternoon, and again at seven. The os uteri was pretty well dilated, the liquor amnii not evacuated, the presenting head very high, mobile, and difficult to touch; and a pulsating loop of the umbilical cord was felt floating below it in the unruptured bag of membranes. From five to nine o'clock the pains seemed only to push the circle of the os uteri further downwards, without increasing its dilatation, or making the head in any degree enter into the pelvic brim. Assisted by Dr. Zeigler, Dr. Keith, and Mr. Figg, I shortly after nine o'clock made the patient inhale the ether vapor. As she afterwards informed us, she almost immediately came under the anodyne influence of the ether; but in consequence of doubts upon this point its use was continued for nearly twenty minutes before I proceeded to turn the infant, as I had previously predetermined to do. A knee was easily seized, and the child's extremities and trunk readily drawn down; but extreme exertion was required in order to extract the head. At length it passed the contracted brim, with the anterior part of its right parietal bone deeply indented by pressure against the projecting promontory of the sacrum, and the whole cranium flattened and compressed laterally. The infant gasped several times, but full respiration could not be established. The transverse or biparietal measurement of its head, at the site of the indentation, was, in its compressed state, not more than 2½ inches. Hence we judged the conjugate diameter of the pelvic brim not to exceed this. The infant was large, and rather above the usual size. It weighed 8 lbs. On afterwards examining the head and removing the scalp, no fracture could be found at the seat of indentation. The thin parietal bone had merely bent inwards.

On questioning the patient after her delivery, she declared that she was quite unconscious of pain during the whole period of the turning and extracting of the infant, or indeed from the first minute or two after she first commenced to breathe the ether. The inhalation was discontinued towards the latter part of the operation, and her first recollection on awaking were "hearing," but not "feeling," the head of the infant "jerk" from her (to use her own expressions), and subsequently she became more roused by the noise caused in the preparation of a bath for the child. She quickly regained full consciousness, and talked with gratitude and wonderment of her delivery, and her insensibility to the pains of it. Next day I found her very well in all respects. I looked in upon her on the 24th, the fifth day after delivery, and was astonished to find her up and dressed, and she informed me that on the previous day she had walked out of her room to visit her mother. Mr. Figg informs me that her further convalescence had been uninterruptedly good and rapid.

I have previously alluded to two cases of delivery by the forceps, in which the patients were under the action of ether at the time of the operation. The woman in the first of these cases was brought into the Royal Maternity Hospital, in strong labor, early on the morning of the 3d February. It was her second confinement. At her first accouchement, seven years before, she had been delivered by instruments, in Ireland, and had been informed by the attendant practitioner, that artificial delivery would be similarly required at her future labors. I saw her between ten and eleven o'clock A.M. The os uteri was well dilated, the membranes ruptured, and the pains extremely strong and frequent; but the large head of the child seemed not to enter fully into the brim, and was little affected by the powerful uterine contractions under which the patient was suffering. By three o'clock her pulse had risen to above 125 beats a minute, and it appeared to the medical officers present, that it would be improper to allow the ineffectual and exhausting efforts of the patient to be longer continued. She was then, at my request, brought under the influence of ether. Dr. Moir, with great skill, applied the long forceps upon the head of the child. He subsequently was obliged to use strong traction during the pains that followed, and becoming temporarily fatigued with his efforts, I supplied his place. After the head fully passed the brim, the forceps were laid aside, and one or two uterine contractions finished the delivery. The child was large and strong, and cried vigorously soon after it was expelled. During the whole of this severe operation the patient appeared quiet and passive. The cries of her child speedily roused her from her anæsthetized state, and she subsequently assured Dr. Moir that she had felt comparatively little or no pain during the whole operation and delivery.

On the evening of the 12th February, I saw another forceps case, with my friend Dr. Graham Weir. The patient was advanced in life, and it was her first confinement. The waters had escaped early, and the anterior lip of the uterus had subsequently become forced down in a very swelled and œdematous state before the head of the infant. After this obstruction was overcome, the child's head speedily descended upon the floor of the pelvis; but it was there impeded in its further progress by the narrow transverse diameter of the outlet. Under the compression of the converging tuberosities of the ischia, the bones of the fœtal cranium soon began to overlap; but at last, no further progress being made, the patient becoming exhausted by a continuous labor of about twenty-four hours, and the soft parts being evidently well relaxed and prepared, Dr. Weir applied the short forceps, and extracted a living infant. For a considerable time before this operation was adopted, I exhibited the vapor of ether to the patient; under it she speedily became quite narcotized. Its action was kept up, and the pains appeared to be so strong as almost to warrant the idea that nature would yet be sufficient; but ultimately instrumental delivery was, as I have already stated, had recourse to. The mother did not fully recover from her state of anæsthesia for ten or fifteen minutes after delivery, and then stated that she was quite unaware of anything that had been done, and of what had occurred. Dr. Weir informs me that this patient was up on the fourth day after delivery, and felt by that time so perfectly well, that she was with difficulty persuaded by the nurse to abstain from walking about the house as usual.

As far as they go, the preceding cases point out one important result:—in all of them, the uterine contractions continued as regular in their occurrence and duration after the state of anæsthesia had been induced, as before the inhalation was begun. The emotion of fear has appeared to me to suspend, in one or two nervous patients, the recurrence of the first pains, after the apparatus was adjusted and its employment commenced, but this effect speedily passed off; and as yet I have seen no instance in which the pains were sensibly diminished in intensity or frequency after the ether had fairly begun to act. Indeed, in some cases they have appeared to me to have become increased as the consciousness of the patient became diminished. This has more particularly occurred with one or two patients, who breathed ether, combined with tincture of ergot, or containing a solution of its oil. A woman was brought into the Maternity Hospital on the 28th January, after being in labor for 30 or 40 hours. It was her second child. Subsequently to her entering the hospital, at seven P.M., scarcely any decided uterine contraction could be said to take place. The os uteri was well opened, but the head was still high in the pelvis; and when I saw her at four A.M., of the following morning, nine hours after her entrance into the hospital, little or no advance whatever had been made, and the case was becoming an anxious one. She was then made to inhale equal parts of sulphuric ether and tincture of ergot. In the course of a few minutes a series of extremely powerful uterine contractions supervened, and the child was born within a quarter of an hour of the commencement of inhalation. The mother subsequently declared that she recollected nothing at all of her delivery, except the removal of the after-birth. In this case, was the re-excitement of strong pains the result of the action of the sulphuric ether, or of the ergot, or of both? Or was it a simple but very strange coincidence? More facts than I yet possess are necessary to decide such a question; but I have seen some cases which lead me to believe that other therapeutic agents besides those I have named may be readily introduced into the system by means of pulmonary inhalation.[1]

A more extensive and careful series of investigations than I have yet been able to institute, may perhaps show that in some constitutions, and under some circumstances or *degrees* of intensity, the process of etherization may possibly interfere with the uterine contractility, particularly in the earlier stages of the labor. At the same time, various analogies would lead us to expect that, as I have hitherto found, the action of the uterus would go on uninterruptedly, when the psychical influence of the mind and purely cerebral functions were suspended, as in the more complete states of anæsthesia. At all events, if we may judge from the analogous experiments of Vollkmann, Bidder, and Kölliker, upon the simple contractions and rhythmic reflex actions of the heart, intestines, &c., the motory nervous powers of the uterus belong to the ganglionic and to the spinal systems, and are not in any necessary dependence upon the brain or mind. Indeed, Ollivier and Nasse have published cases of perfect paraplegia, notwithstanding which the act of parturition in the human female proceeded regularly in its course, and without conscious pain. In the one case (Ollivier's), the cord was compressed and destroyed from the first to the fourth dorsal vertebra by a collection of acephalocysts;[1] and, in the other instance (Nasse's), complete paralysis had followed a fracture of the third and fourth cervical vertebræ.[2] Of course such lesions necessarily prevented the brain exerting any influence upon the uterus, or its contractions.

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[1] Dr. Richard Pearson, who, in 1795, was, I believe, the first person that recommended the inhalation of sulphuric ether as a therapeutic agent (see his Account of the Nature and Properties of different kinds of Airs, p. 24), suggested also the use of it impregnated with opium, squill, cicuta, &c.; and he speaks of the effect of "an emetic given in this manner." He employed the simple sulphuric ether vapor in some cases of phthisis, asthma, hooping-cough, croup, and catarrh, recommending it to be inhaled (after being rectified and washed), from a cup—through an inverted funnel—or, with children, by "wetting a handkerchief with it, and holding it near the nose and mouth." See Medical Facts and Observations, for 1797, vol. vii. p. 96. In the 13th volume of the Dictionnaire des Sciences Médicales (1816), p. 385, Nysten has described a particular apparatus, like some of our modern forms, for the inhalation of sulphuric ether. See also vol. xvii. p. 134.—Vaporizable substances, when introduced into the system in this manner, probably pass undigested and unchanged into the circulation, and "seem (observes Wagner) to make their way into the blood through the unbroken vascular membrane [of the bronchial cells] with the *same* certainty and ease as when they are injected *directly* into the veins." (Elements of Physiology, 1842, p. 443.) Will this not explain both the rapidity and intensity of their actions when thus used?

Long ago, in discussing this subject, Haller adduced the authority of Harvey, Smellie, Lamotte, &c., to prove that uterine contractions and labor may go on with the mother, "ignarâ, stupidâ et sopitâ, et immobili, et apoplecticâ, et epilepticâ, et convulsionibus agitatâ,[3] et ad summum debili."[4] Deneux mentions a fact still more in point, because in it the analogy with the operation of anæsthetics is still stronger, or indeed identical. "A woman," says he,[5] "was brought to the Hotel Dieu at Amiens in a comatose state, in consequence of her taking spirituous liquors since the commencement of labor. She was delivered in the natural manner in this state; the sleep continued for some time after delivery. The woman, on awaking much surprised at finding her delivery completed, congratulated herself on having made so happy a discovery, and declared she would make use of it if she had again occasion."[6]

In obstetric, as in surgical practice, the degree of insensibility produced by anæsthesia, and its accompanying phenomena, differ much in different instances. In some, a state of total apathy and insensibility seems to be produced; others move about and complain more or less loudly during the uterine contractions, though afterwards, when restored to their state of common consciousness, they have no recollection of any suffering whatever, or, indeed, of anything that had occurred during the inhalation and action of the ether; others, again, remain quite aware and conscious of what is going on around them, and watch the recurrence of the uterine contractions, but feel indifferent to their effects, and not in any degree distressed by their presence; and in another class, again, the attendant suffering is merely more or less diminished and obtunded, without being perfectly cancelled and annulled.

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[1] Traité de la Moelle Epiniere, p. 784.

[2] Untersuchungen zur Physiologie, &c. Dr. Cheyne reports a case of fatal hemorrhagic apoplexy and hemiplegia, in which, without any apparent pains, "the uterus (observes Dr. Kellie) appears as an involuntary muscle, to have acted in the most perfect manner in expelling the fœtus and secundines," the day before death. The child was born alive.—Cases of Apoplexy and Lethargy, pp. 91 and 161.

[3] "During the continuance of puerperal convulsions, uterine action is *not* suspended, although no signs of pain are manifested by the woman, if she remain comatose."—Dr. F. Ramsbotham's Obstetric Medicine (1844), p. 455.

[4] Elementa Physiologiæ, tom. viii. p. 420.

[5] Recueil Periodique de la Societe de Médecine, April, 1818.

[6] The celebrated case of the Countess de St. Geran is sufficiently remarkable in relation to the present subject. See full and long details of it in Gayot's Causes Célèbres, tom. i. p. 142 to 266. After the Countess had been nine hours in labor with her first child, the midwife in attendance exhibited to her a potion (*breuvage*), which rendered her insensible till the following morning. When the Countess then awoke to consciousness, she found herself bathed in blood, the abdominal tumor fallen, and all the signs of recent delivery present; but the child born during her state of insensibility had been removed, and its existence was even denied to her. It was years afterwards proved, to the satisfaction of the French law courts, that the Countess had been delivered of a male child, during an induced lethargic condition, and that the infant had been surreptitiously conveyed away to a distance, and brought up as the son of a poor man. The child's claims were, after much litigation, fully acknowledged; he was restored to his parents, and ultimately succeeded to his father's title. What Nepenthean "breuvage" could possibly produce the alleged effect?

On the evening of the 13th inst., in two cases that rapidly followed each other, I witnessed, in the above respect, two very different conditions induced by the use of the ether. The patients each of whom had borne several children previously, were both placed under the influence of it just as the os uteri became fully opened, and in neither did the full expulsion of the infant through the pelvic passages require above twelve or fifteen minutes. My first patient, the wife of a clergyman, subsequently stated, that she knew all that was said and done about her, was aware of the pains being present, but felt no distress from any of them till the supervention of the last strong contraction, which drove the head out of the vulva, and the feeling then seemed to partake of the character of strong pressure, rather than of actual pain. Subsequently she told me, she could only look back with regret to the apparently unnecessary suffering she had endured in the birth of her former infants. The second patient, a lady of a timid temperament, and very apprehensive about the result of her present confinement, was induced with difficulty to inhale the ether vapor; but it speedily affected her when once she did begin. In two or three minutes she pushed the apparatus from her mouth, talked excitedly to a female relative present, but was immediately induced to recommence the inhalation; and subsequently, according to her own statement, "wakened out of a dream, and unexpectedly found her child born." Like many others, she thought hours instead of minutes had elapsed, from the commencement of the inhalation to the period of the complete restoration of consciousness. Making apparently an effort of memory, she afterwards inquired if she had not once awakened out of her dreamy state, and spoken some nonsense to her friend.

A careful collection of cautious and accurate observations will no doubt be required, before the inhalation of sulphuric ether is adopted to any great extent in the practice of midwifery. It will be necessary to ascertain its precise effects, both upon the action of the uterus, and of the assistant abdominal muscles; its influence, if any,

upon the child; whether it gives a tendency to hemorrhage or ot
complications; the contraindications peculiar to its use; the m
certain modes of exhibiting it; the length of time it may be
ployed, &c.[1] In no case have I observed any harm whatever,
either mother or infant, follow upon its employment. And, on
other hand, I have the strongest assurance and conviction, th
have already seen no small amount of maternal suffering and ag
saved by its application. The cases I have detailed sufficie
show its value and safety in cases of operative midwifery.
here, as in surgery, its utility is certainly not confined to the m
suspension and abrogation of conscious pain, great as, by itself, s
a boon would doubtlessly be. But in modifying and obliterat
the state of conscious pain, the nervous *shock*,[2] otherwise liabl
be produced by such pain—particularly whenever it is extreme,
intensely waited for and endured—is saved to the constitution,
thus an escape gained from many evil consequences that are too
to follow in its train.[3] Granting that experience will yet be abl
prove its safety and efficacy in modifying and annulling the pa
of labor, will (I have repeatedly heard the question asked) the st
of etherization ever come to be generally employed with the sim
object of assuaging the pains of natural parturition? Or (as
problem has not unfrequently been put to me), should we be "ju
fied" in using it for such a purpose? In conclusion, let us consi
this point for a moment.

Custom and prejudice, and, perhaps, the idea of its inevita
necessity, make both the profession and our patients look u
the amount and intensity of pain encountered in common cases

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[1] I have, during labor, kept patients under its influence for upwards of half an hour.
exhibiting it, the first, or exhilarating stage of its effects should be passed through as ra
as possibly, and the patient never allowed to be excited or irritated by the nurse or ot
I have heard its use strenuously denounced on the ground that its effects, though good
evanescent, are still of an intoxicating character. But on the same ground, the u
opium, &c. &c., in medicine, to relieve pain and procure sleep, should be equally repreh
and discarded.

[2] On the extent of the nervous *shock* accompanying human parturition, see Dr. Hamil
Practical Observations, p. 179, &c.; and Dr. Churchill's Chapter on Convalescence
Labor, in his work on the Diseases of Pregnancy and Childbed, p. 240, &c.

[3] On what division or divisions of the nervous system does the nervous shock operat
the cerebral, spinal, or ganglionic? If on the former, it should be kept in abeyance by
anæsthesia. Some years ago, I saw Dr. J. Argyll Robertson, when he was Acting Su
at the Royal Infirmary, amputate, at the shoulder-joint, an arm sadly shattered an
so before by a railway injury. The man at the time of receiving the injury, during
operation, and for several hours afterwards, was in a state of insensibility from deep in
cation; and at last wakened up, not knowing what had happened. His recovery
rapid and uninterrupted. Would it have been so if his nervous system had been suffici
alive to the double shock of the operation and injury? Out of eighteen cases of pri
amputation, performed during four years in the Edinburgh Hospital, and mentioned in
Peacock's Report of the Institution (1843), this man and another patient were the only
out of the eighteen that survived.

natural labor, as far less worthy of consideration than in reality it is. Viewed apart, and in an isolated light, the degree of actual pain usually endured during common labor is as great, if not greater, than that attendant upon most surgical operations. I allude particularly to the excessive pain and anguish, which in nine out of ten cases accompany the passage of the child's head through the outlet of the pelvis and external parts. Speaking of common or natural labor in its last stages, Dr. Merriman observes, the pulse gradually "increases in quickness and force; the skin grows hot; the face becomes intensely red; drops of sweat stand upon the forehead; and a perspiration, sometimes profuse, breaks out all over the body; frequently violent tremblings accompany the last pain, and at the moment that the head passes into the world, the extremity of suffering seems to be beyond endurance."[1] Or, take the picture of the suffering of the mother in the last stage of natural labor, as portrayed by the most faithful of living observers—Professor Naegele of Heidelberg—"The pains," he observes, "of this stage are still more severe, painful, and enduring; return after a short interval, and take a far greater effect upon the patient, than those of the previous stage. Their severity increases so much the more from the additional suffering arising from the continually increasing distension of the external parts. They convulse the whole frame, and have hence been called the *dolores conquassantes*. The bearing down becomes more continued, and there is not unfrequently vomiting. The patient quivers and trembles all over. Her face is flushed, and with the rest of the body, is bathed in perspiration. Her looks are staring and wild; her features alter so much that they can scarcely be recognized. Her impatience rises to its maximum with loud crying and wailing, and frequently expressions which, even with sensible, high-principled women, border close upon insanity. Everything denotes the violent manner in which both body and mind are affected."[2]

I have stated that the question which I have been repeatedly asked is this—shall we ever be "justified" in using the vapor of ether to assuage the pains of natural labor? Now, if experience betimes goes fully to prove to us the safety with which anæsthesia may, under proper precautions and management, be employed in the course of parturition, then, looking to the facts of the case, and considering the actual amount of pain usually endured, as shown in the descriptions of Merriman, Naegele, and others,[3] I believe that the question will require to be quite changed in its character. For instead of determining in relation to it whether we shall be "justified" in using this agent under the circumstances named, it will become, on the other hand, necessary to determine whether on any grounds, moral or medical, a professional man could deem himself "justified" in withholding, and *not* using any such safe means, as we at present presuppose this to be, provided he had the power by it of assuaging the pangs and anguish of the last stage of natural labor, and thus counteracting what Velpeau describes as "those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,"[1] which accompany the termination of natural parturition in the human mother.

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[1] Synopsis of Parturition, p. 15.

[2] Lehrbuch der Geburtshülfe, p. 104. See British and Foreign Medical Review, vol. xix. p. 64.

[3] Dr. Rigby, in his System of Midwifery, p. 103, observes, "This is the moment of greatest pain, and the patient is quite wild and frantic with suffering; it approaches to a species of insanity," &c. &c.

EDINBURGH, February 18, 1847.

ANÆSTHESIA IN PUERPERAL CONVULSIONS.

On May 19, 1847, Dr. Alexander Wood related a case to the Edinburgh Medico-Chirurgical Society,[2] where puerperal convulsions had followed the administration of ether.

In reply to his inquiry whether the ether could have been the cause of the convulsions?

Dr. Simpson considered that nothing had been brought forward to convince him that the convulsions depended on the inhalation of ether—we must take care not to confound coincidence and sequence. He had never heard of a case where convulsions had come on twelve hours after the inhalation of ether. Convulsions and spasms had been observed by some operators during inhalation, but were rare after it. Ether had now been given in thousands of cases, and yet, so far as he was aware, this had not occurred. On the other hand, twitchings, spasms, and convulsions, were very common after delivery, and were more frequent in some seasons of the year than at others. Now it so happened, that within the last few months he had seen six very severe cases of convulsions, and that these were unusually common at present had been also remarked by other practitioners. He did not think that the laudanum could have produced the convulsions in this case, but it was very probable that the constipation might have acted as a cause, for as soon as the bowels were relieved they entirely ceased. The blood also firmly coagulated, whereas, in cases of poisoning by ether, it had been shown to be rendered uncoagulable and unusually fluid. From all these circumstances, he did not think that anæsthesia had much to do with the convulsions in the case which had been related.

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[1] Traité des Accouchemens, vol. i. p. 449. "Ces cris perçans, cette agitation si vive, ces efforts excessifs, ces angoisses inexprimables, ces douleurs qui paraissaient intolerables," &c.

[2] From Edinburgh Monthly Journal of Medical Science, June, 1847, p. 938.

In cases where convulsions depended upon prolonged and violent muscular efforts, he thought ether would be useful by diminishing or preventing these. He had certainly been afraid of its producing hemorrhage, but experience had not justified his fears. In one case, he had lately attended, there had been severe flooding. It was that of a lady who had previously insisted on having the ether when confined. It happened, however, that the labor was so rapid that he had not time to reach the house before the child was born, and consequently no ether had been given. Had inhalation been practised, the hemorrhage would doubtless have been ascribed to it. He had also seen a case of phlebitis, following delivery, but fortunately the ether here also was not given. Dr. Tyler Smith had endeavored to prove, from physiological considerations, that anæsthesia must be injurious, that the pains would cease, and that the action of the glottis would be impeded, but facts had shown the fallacy of such arguments.

Ample experience has now proved, that instead of being a cause of puerperal convulsions, as was at first feared, chloroform and ether are by far the most powerful agents which we can use in preventing and arresting them.—(Ed.)

CHAPTER II.

SUPERINDUCTION OF ANÆSTHESIA IN NATURAL AND MORBID PARTURITION: WITH CASES ILLUSTRATIVE OF THE USE AND EFFECTS OF CHLOROFORM IN OBSTETRIC PRACTICE.[1]

> Serve me—as Mandragora—that I may sleep.
>
> Webster's Duchess of Malfy.

> But there is
> No danger in what show of sleep it makes,
> More than the locking up the spirits a time,
> To be more fresh, reviving.
>
> Shakspeare's Cymbeline.

Among the many improvements by which the operative part of medicine has, from time to time, been enriched, few or none have exerted a more potent or a more beneficial influence over its advancement and progress than the introduction, in the 16th century, of the application of ligatures to arteries, with the object of arresting the hemorrhage attendant upon surgical wounds and operations. Previously to that time, surgeons had no other means of stemming the flow of blood—after amputation of the limbs, for instance—than by scorching over the raw and bleeding wound with a red-hot iron, or by plunging it into boiling pitch, or by applying strong potential cauteries to its surface. With laudable efforts to diminish the fearful severities of their practice, they exerted their ingenuity in devising, as it were, refinements upon these necessitous cruelties. Thus Hildanus, the patriarch of German surgery, amputated the limbs of his patients with red-hot knives, in order that he might divide the flesh and sear up the vessels at one and the same time. Upon all these practices, the great and happy suggestion of Ambrose Paré, viz., to shut up the bleeding vessels, by constricting or tying them with slender ligatures, was a vast and mighty improvement. It at once made the arrestment of hemorrhage in operations far more simple, more certain, and more secure. It saved immeasurably the sufferings of the patients, while it added immeasurably to their safety. But the practice was new, and an innovation; and consequently, like all other innovations in medical practice, it was, at first and for long, bitterly decried and denounced. The College of Physicians of Paris attacked Paré for his proposed new practice: they attempted, by the authority of the French Parliament, to suppress the publication and dissemination of his observations; and, for nearly a long century afterwards, some of the hospital surgeons of Paris continued, with the characteristic obstinacy of the profession, to prefer cauterizing bleeding arteries "with *all* the ancients," rather than simply tie them "after the manner of a few ignorant and presumptuous moderns."[1] "Without," writes the late Mr. John Bell—"without reading the books of these old surgeons, it is not possible to imagine the horrors of the cautery, nor how much reason Paré had for upbraiding the surgeons of his own time with their cruelties. . . . . . The horrors of the patient, and his ungovernable cries, the hurry of the operators and assistants, the sparkling of the heated irons, and the hissing of the blood against them, *must* have made terrible scenes, and surgery *must*, in those days, have been a horrible trade."[1]

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[1] Read to the Medico-Chirurgical Society of Edinburgh, at their meeting on the 1st December, 1847.

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[1] All writers on surgical history give more or less full details upon this opposition to the practice of Paré. Thus, for example, Professor Cooper observes: "By many surgeons, however, the tying of arteries continued to be deemed too troublesome, and hence they persisted in the barbarous use of the actual cautery; of this number were Pigrai, F. Plazzoni, and P. M. Rossi. Nay, so difficult was it to eradicate the blind attachment shown to the ancients, that Theodorus Baronius, a professor at Cremona, publicly declared, in 1609, that he would rather err with Galen than follow the advice of any other person. . . . . I shall not here expatiate upon the ill-treatment which Paré experienced from the base and ignorant Gourmelin, President of the Parisian College of Physicians; nor upon the slowness and reluctance with which the generality of surgeons renounced the cautery for the ligature. . . . Almost 100 years after Paré, a button of vitriol was ordinarily employed in the Hotel Dieu at Paris for the stoppage of hemorrhage after amputations; Dionis was the first French surgeon who taught and recommended Paré's method. This happened towards the close of the 17th century, while Paré lived towards the end of the 16th."—Cooper's Dictionary of Practical Surgery. 7th edit. pp. 46, 47. See also Sprengel's Histoire de Médecine, vol. iii. p. 315; Bell's Surgery, vol. i. p. 226, &c.

The sentiments which Mr. Bell here expresses are those with which the human mind often *looks back* upon our opinions and practices, when these opinions and practices are past and gone, and have become mere matters of history. In the above, as in many other instances, we never have become fully awakened to the cruelty and enormity of some of our established doctrines and doings, until from time to time, an advance is made in civilization or science, and we find that this or that doctrine and practice, with all its attendant sufferings and inhumanities, was in reality utterly unnecessary, and utterly uncalled for.[2] In general, however, long years elapse before this new aspect in matters is duly seen; or at least, duly acknowledged. While the practices themselves are in full operation, the mind, enthralled by education and habit, cannot be easily made to view them in their true character; and when, in the progress of the march of knowledge and science, their propriety and perpetuation come at last to be challenged and contested, human passions and prejudices ever (as in the above instance of cauterization) rise up to argue for and insist upon the continuance and safety of the past, and the total impolicy and high peril of any attempted alteration. But time passes on, and brings with it, sometimes abruptly—generally almost imperceptibly—a perfect change of doctrine and practice. Any surgeon who, in the days of Paré, dared to arrest the hemorrhages from his amputation wounds, by applying ligatures *instead* of red-hot irons, would have been denounced by his compeers. Any surgeon, on the contrary, who now, at this present day, dared to arrest the hemorrhages from his amputation wounds, by applying to the bleeding vessels, not ligatures but red-hot irons, would as certainly be denounced by his compeers, and his talents, as well as his humanity, would be strongly challenged. We look back with sorrow upon the pitiless practices, in that respect, of the contemporaries and opponents of Paré. In the course of years our successors in the profession will, I most sincerely believe, look back with similar feelings upon the alleged "insignificance," and "propriety," and

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[1] Principles of Surgery, vol. i. p. 212.

[2] Witness, for example—(as compared with the *past* opinions of those who practised them)—our *present* opinions regarding the burning, by our Druidical forefathers, of whole wickerfuls of human living beings, and in the name of religion; or, in times nearer to our own—in Christian times—the application of the fire and fagot by man to man, still under the plea of religion; or the use of the rack and torture; the incremation, in the sixteenth and seventeenth centuries, of many poor wretches for the alleged crime of witchcraft; the altered existing ideas regarding the required frequency of capital punishments, and the whole question regarding their policy; the recent rapid and complete change of doctrine regarding the horrors and inhumanity of slavery; the changes in practice regarding insanity from what it was in the last century, when chains and a dungeon were the portion of every poor lunatic, &c. &c.

"desirability" of pain in surgical operations, as maintained by many members of the profession at the present day; and they will equally marvel at the idea of men—of humane men—placently confessing and upholding that they prefer operating upon their patients in a waking instead of in an anæsthetic state; and that the fearful agonies which they inflict—the agonies of the surgeon's knife—should be endured rather than avoided—quietly and decorously submitted to, and not attempted to be eschewed. I have elsewhere discussed,[1] at some length, the strange opinions and practices of some modern surgeons upon this alleged propriety and necessity of pain in surgical practice and surgical operations. On the present occasion, my object is to offer some remarks regarding the pains attendant upon parturition, and the propriety of alleviating and annulling the sufferings of our patients in obstetrical practice and obstetrical operations. But let me first adduce some evidence of *their* intensity and amount.

"The distress and pain," observes Dr. Denman,[2] "which women often endure while they are struggling through a difficult labor are *beyond* all description, and seem to be more than human nature would be able to bear under any other circumstances." But even the amount of agony endured in most cases of *natural* parturition, is abundantly severe.[3] Viewed apart, and in an isolated light, the total sum of actual pain attendant upon common labor is as great, if not greater, than that attendant upon most surgical operations. It is, I believe, education and custom, and perhaps the idea of its inevitable necessity, which have made the profession in general look upon the degree of maternal pain and physical suffering accompanying natural parturition, as less deserving of consideration than in reality it is. These circumstances have, in a great measure, blinded us to its actual amount, and intensity, and importance. For it was, no doubt with perfect truth, remarked by an author[4] who wrote three hundred years ago, "Mulier, in partu, maximos et fere intolerabiles sustinet dolores."[5]

"This," observes Dr. Rigby, "is the moment of greatest pain, and the patient is frequently quite wild and frantic with suffering; it approaches to a species of insanity, and shows itself in the most quiet and gentle dispositions. The laws in Germany have made great allowances for any act of violence committed during these

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[1] See Monthly Journal of Medical Science for September, 1847, pp. 156–166, "On the Allegation of the Prevention of Pain in Surgical Cases being Unnecessary and Improper;" also p. 476 of this volume.

[2] Introduction to Midwifery, 5th edition, p. 377.

[3] Cases undoubtedly ever and anon occur, in which the mother suffers comparatively little or no pain; but these are exceptions, rare exceptions, to a general rule.

[4] Hieronymus Mercurialis, in Spachius' Gynæcia, p. 233.

[5] A portion of this article has been omitted.

ents of frenzy, and wisely and mercifully consider that the pa-
at the time was laboring under a species of temporary insanity.
the act of child-murder, when satisfactorily proved to have
n place at this moment, is treated with considerable leniency.
state of mind is sometimes manifested in a slighter degree by
ns and words, so contrary to the general habit and nature of
patient, as to prove that she could not have been under the
er control of her reason at the moment. It is a question how
his state of mind may arise from intense suffering, or how far
circulation of the brain may be affected by the pressure which
erted upon the abdominal viscera."[1]

ch is the description of the amount of pain and agony endured
atural parturition, given by some of our best and most esteemed
orities in obstetric literature.

it right for the physician to interfere with these fearful suffer-
and agonies in order to save and shield his patient from the
rance of them? Is it proper for him to exercise the skill of his
o as to moderate and remove these almost "intolerable pains,"
e intolerabiles dolores?" Would it be fit and meet in him to use
an means to assuage the pangs and anguish attendant upon the
ess of parturition in the human mother?

hese questions, and questions like these, I have often, during
currency of the present year, heard complacently put by medi-
men—men, too, whose opinions and actions in other matters,
in other respects, were fully and truly actuated by that great
ciple of emotion which both impels us to feel sympathy at the
t of suffering in any fellow-creature,[2] and at the same time im-
s to us delight and gratification in the exercise of any power by
ch we can mitigate and alleviate their suffering. Such ques-
s, I repeat, are seriously asked by physicians and surgeons, the
essed object of whose whole science and art is the relief of
an disease and human suffering. They are questions propounded
all imaginable gravity and seriousness by individuals who, in
ere abstract point of view, would, no doubt, strongly object to
g considered as anxious to patronize and abet human misery,
raffic in the perpetuation of human pain. Nay, probably at the
at which I write, there is not one in twenty—perhaps not one
hundred—of the physicians and surgeons of Great Britain who
e, as yet, thought seriously upon the propriety of alleviating and
ulling the tortures attendant on human parturition; or who have
nowledged to their own minds the propriety of bestirring them-

ystem of Midwifery, p. 103.

Inditus est, ab ipsâ Naturâ, homini, *misericordiæ* affectus nobilis et excellens." Bacon
e Augmentis Scient.," Lib. viii. cap. ii.

selves so as to be able, in the exercise of their profession, to secure for their patients an immunity from the throes and agonies of childbirth.

Perhaps, as an apology for their indolence and apathy, some may be ready to argue, that the pain and suffering attendant on parturition is not dangerous and destructive in its results, however agonizing and distressing it may be to the patient during its continuance. But the argument is fundamentally unsound. All pain is *per se*, and especially when in excess, destructive and even ultimately fatal in its action and effects. It "exhausts," says Mr. Travers, "the principle of life."[1] "It exhausts," says Mr. Burns of Glasgow, "both the system and the part."[2] "Mere pain," observed the late Dr. Gooch, "*can* destroy life."[3] And the great pain accompanying human parturition is no exception to this general pathological law. For, in fact, the maternal mortality attendant upon parturition regularly increases in a ratio progressive with the increased duration of the woman's sufferings. The statistical data published by Dr. Collins, in his Report of the Dublin Lying-in Hospital, affords ample proof of this general principle. According to calculations which I some time ago made from Dr. Collins' data, I found that while in the woman delivered in the Dublin Hospital, and whose sufferings were terminated within 2 hours, only 1 in 320 of the mothers died; where the labor varied in duration from 2 to 6 hours, 1 in 145 of the mothers died; in those in whom it continued from 7 to 12 hours, 1 in 80 died; where it endured from 12 to 24 hours, 1 in 26 died; where it lasted from 24 to 36 hours, 1 in 17 died; and out of all those whose parturient sufferings were prolonged beyond 36 hours, 1 in every 6 perished.[4]

Again, some may possibly be inclined to reason, that any means by which we could produce a state of anæsthesia or insensibility to these physical pains of labor, must, of necessity, be of such a character as to add to the perils and dangers of the patient. I believe this argument to be as futile and untenable as the one that I have just noticed. Indeed, judging from analogy, and from what is the fact in surgery, I believe that, as a counteraction to the morbific influence of pain, the state of artificial anæsthesia does not only imply a saving of human suffering, but a saving also of human life. Out of above 300 cases of the larger amputations performed during the current year, upon patients in an etherized or anæsthetic state, and which I have collated from different hospitals in Great Britain, Ireland, and France, a smaller proportion died than formerly used to perish in the same hospitals under the same operations without næsthesia. I shall take one of these amputations as an illustration f the whole—and that one the most severe of all—viz., amputation f the thigh. Malgaigne (1842) showed, that under amputations of he thigh, in the hospitals of Paris, 62 in every 100 died; in Edin-urgh, the mortality from this operation, in the only years during hich the hospitals reports were published (1839–42), was 50 in every 00; Mr. Phillips of London (1844), found the average mortality 0 in 100; Dr. Lawrie at Glasgow (1839), found it also in the hospi-als of that city to be 40 in 100.[1] I have notes of 135 cases in hich this same operation has been performed in hospital practice pon patients in an anæsthetized state. Out of these 135 cases 3 died, or only 24 in 100. Hence I repeat, that the condition of næsthesia not only preserves the patient in surgical practice from gony and torture, but actually preserves him too from the chances f danger and death. And I firmly believe, that the superinduction f anæsthesia in obstetric practice will yet be found to diminish and emove also, in some degree, the perils as well as the pains of labor.

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[1] Inquiry concerning Constitutional Irritation, vol. i. p. 76.

[2] Principles of Surgery, vol. i. p. 502.

[3] Dr. Merriman's Synopsis of Parturition, p. 239.

[4] See vol. i. of this work, p. 468.

In an essay which I wrote in February last, "On the Employment f the Inhalation of Sulphuric Ether in the Practice of Midwifery,"[2] offered some remarks on its application to cases of common as well s of morbid parturition, and took occasion to observe, "The ques-ion which I have been repeatedly asked is this—Shall we ever be justified' in using the vapor of ether to assuage the pain of natural abor? Now, if experience betimes goes fully to prove to us the safety ith which ether may, under proper precautions and management, e employed in the course of parturition, then, looking to the facts f the case, and considering the actual amount of pain usually endured, I believe that the question will require to be quite changed n its character. For, instead of determining in relation to it whether we shall be 'justified' in using this agent under the circumstances named, it will become, on the other hand, necessary to de-

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[1] The following table exhibits the actual number of the cases of amputation of the thigh ferred to in the text, with their respective results:

Mortality accompanying Amputation of the Thigh.

Name of Reporter.	No. of Cases.	No. of Deaths.	Per Cent. of Deaths.
Malgaigne, Paris, ...	201	126	62 in 100
Peacock, Edinburgh.	43	21	50 in 100
Phillips' Collection of Cases, ...	660	263	40 in 100
Lawrie, Glasgow, ...	184	73	40 in 100
Total, ...	1088	483	44 in 100
Upon Patients in an Anæsthetic State, .	135	33	24 in 100

[2] Monthly Journal of Medical Science for March, 1847, p. 728.

termine whether on any grounds, moral or medical, a professional man could deem himself 'justified' in withholding, and not using any such safe means (as we at present presuppose this to be), provided he had the power by it of assuaging the pangs and anguish of the last stage of natural labor, and thus counteracting what Velpeau describes 'as those piercing cries, that agitation so lively, those excessive efforts, those inexpressible agonies, and those pains apparently intolerable,'[1] which accompany the termination of natural parturition in the human mother."

Since the latter part of January,[2] I have employed anæsthesia, with few and rare exceptions, in every case of labor which has been under my care. And the results, as I have already elsewhere stated, have been, indeed, most happy and gratifying. I never had the pleasure of watching over a series of more perfect or more rapid recoveries; nor have I once witnessed any disagreeable result to either mother or child. I have kept up the anæsthetic state during periods varying from a few minutes to three, four, five, and six hours. I do not remember a single patient to have taken it who has not afterwards declared her sincere gratitude for its employment, and her indubitable determination to have recourse again to similar means under similar circumstances. All who happened to have formerly entertained any dread respecting the inhalation, or its effects, have afterwards looked back, both amazed at, and amused with, their previous absurd fears and groundless terrors. Most, indeed, have subsequently set out, like zealous missionaries, to persuade other friends to avail themselves of the same measure of relief, in their hour of trial and travail; and a number of my most esteemed professional brethren in Edinburgh have adopted it with success, and results equal to my own. All of us, I most sincerely believe, are called upon to employ it by every principle of true humanity, as well as by every principle of true religion.[3] Medical men may oppose for a time the superinduction of anæsthesia in parturition, but they will oppose it in vain; for certainly our patients themselves will force the use of it upon the profession. The whole question is, even now, one merely of time. It is not—Shall the practice come to be generally adopted? but, When shall it come to be generally adopted? Of course, it will meet from various quarters with all due and determinate opposition. Medical men will, no doubt, earnestly argue that their established medical opinions and medical practices hould not be harshly interfered with by any violent innovations of octrine regarding the non-necessity and non-propriety of maternal uffering. They will insist on mothers continuing to endure, in all heir primitive intensity, all the agonies of childbirth, as a proper acrifice to the conservatism of the doctrine of the desirability of ain. They will perhaps attempt to frighten their patients into the edical propriety of this sacrifice of their feelings;[1] and some may e found who will unscrupulously ascribe to the new agency any isadventures, from any causes whatever, that may happen to occur n practice. But husbands will scarcely permit the sufferings of heir wives to be perpetuated, merely in order that the tranquillity f this or that medical dogma be not rudely disturbed. Women hemselves will betimes rebel against enduring the usual tortures nd miseries of childbirth, merely to subserve the caprice of their medical attendants. And I more than doubt if any physician is really justified, on any grounds, medical or moral, in deliberately desiring and asking his patients to shriek and writhe on in their agonies for a few months or a few years longer—in order that, by doing so, they may defer to his professional apathy, or pander to his professional prejudices.

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[1] Traité des Accouchemens, vol. i. p. 449. "Ces cris perçans, cette agitation si vifs, ces efforts excessifs, ces angoisses inexprimables, ces douleurs qui parassaient intolerables," &c.

[2] See Edinburgh Monthly Journal of Medical Science, January, 1848, p. 526; also Lancet, December, 1847, p. 623.

[3] See page 547, "Answer to the Religious Objections urged against the Employment of Anæsthetic Agents in Midwifery and Surgery."

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[1] We can all recollect the many absurd stories of apocryphal disasters and deaths, that e opponents of etherization busily and anxiously reported towards the commencement of e present year, as having occurred from the employment of ether-inhalation in surgery. r. Forbes, in his excellent article on etherization, in treating of these unscrupulous and isreputable pieces of professional gossip, observes—"One day we had death from asphyxia; nother from coma; another from hemoptysis; some from convulsions; a few from pneuonia; and one or two from actual incremation, or explosion, through the accidental firing f the ethereal vapor within the air-passages. We have not had time to investigate all ese terrible cases, but we may state that we traced *the one* which seemed the *best* authenated—that from hemoptysis—from its full-blown majesty in after-dinner gossip, to its umble source in the hospital. And this was the case, as the man himself detailed it to us: -A day or two after a successful operation for hernia, under etherization, the man pricked is gums while picking his teeth with a pin; and it was the product of *this* operation, not f the ether, seen in the spitting-pot by the patient's bed-side, that was bruited about town, s of itself sufficient to settle the question in all future time."—(British and Foreign Medical eview, No. xlvi. April, 1847, p. 564.) When first employing etherization in midwifery, I et with no small number of similar strange tales and accusations. For example, in Febuary last, a patient who happened to be severely frightened, had, in consequence, a premaure labor. The child presented preternaturally; and died a day or two after birth. The other was attacked with phlegmasia dolens, and made a very long and protracted recovery. arious kind friends, anxious about the results of etherization in midwifery, warned me of he professional odium which this case was bringing upon the new practice, and of the trong argument which it was affording to others against the safety of ether-inhalation in bstetrics. I was repeatedly and credibly told that ladies had informed their physicians, hat the quantity used was *so* great that they had felt the odor of it perfectly oppressive hen calling, even days afterwards, at the house of my patient. The answer to all this as sufficiently simple. The danger of death to the child from its prematurity and preteratural presentation appeared to be, from the first, so imminent, that I did not choose to eril the character of the new practice by following it in this case. The ether had not only ot been used, but not a drop of it had ever been in the house: One of my patients was ealously attempted, some months ago, to be persuaded against the "horrors of ether," on e strong and round assertion, that some dozen ladies or more in Dublin, upon whom the ractice had been tried, had indubitably perished from the effects of it. Unfortunately for e veracity of this statement, ether inhalation had never once been used, or attempted to e used, in obstetric practice in Dublin, up to that date, or for a long time afterwards. Ineed, the first case in which ether was employed in midwifery in Dublin only occurred this eek (28th Nov.), as I am informed in a letter of that date, which I have just received rearding it, from Dr. Tyler.

Two agents have the power of producing anæsthesia during labor, viz., the inhalation of sulphuric ether, and the inhalation of chloroform. With most, if not all of my professional brethren, I believe that the latter agent possesses various important advantages over the former, particularly in obstetric practice; and that in particular, it is far more portable; more manageable and powerful; more agreeable to inhale; is less exciting than ether; and gives us far greater control and command over the superinduction of the anæsthetic state. In the remaining part of these observations, I shall detail briefly some instances illustrative of its effects and utility in the production of anæsthesia in cases of natural and morbid parturition.

CASE I.—The patient to whom it was first exhibited had been previously delivered in the country by craniotomy, after a very long labor. Her second confinement took place a fortnight before the full time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded and the pains very severe. In twenty-five minutes the child was born. The crying of the infant did not rouse the mother, nor did she awake till after the placenta was removed. She was then perfectly unaware that her child was born. She stated her sensations to be those of awakening from "a very comfortable sleep." It was, for a time, a matter of no small difficulty to persuade her that the labor was over, and that the living child presented to her was her own.

CASE II.—I exhibited it, with Mr. Carmichael, to a patient who had, at her preceding confinement, been in severe labor for twenty hours, followed by flooding. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. She was kept under its influence for a quarter of an hour longer, till the placenta was removed, and the binder, body, and bed-clothes, all adjusted. On awaking, she declared she had been sleeping refreshingly; and was quite unconscious that the child was born, till she suddenly heard it squalling at its first toilet in the next room. No flooding. An hour afterwards, she declared she felt perfectly unfatigued, and not as if she had borne a child at all.

CASE III.—Patient unmarried. A first labor. Twins. The first ld presented by the pelvis, the second with the hand and head. e chloroform was exhibited when the os uteri was nearly fully ated. The passages speedily became greatly relaxed, as has hap- ied in other cases placed under its full influence; and in a few ns the first child was born, assisted by some traction. I broke membranes of the second, pushed up the hand, and secured the st complete presentation of the head. Three pains expelled the ld. The mother was then bound up; her clothes were changed, l she was lifted into another bed. During all this time she slept soundly, and for a full hour afterwards; the chloroform acting in s, as in other cases of its prolonged employment, as a soporific. e patient recollected nothing from the time of the first inhala- ns; and was in no small degree distressed when not one—but two iving children were brought by the nurse to her. Dr. Christison ompanied me to this case.

ASE IV.—Primipara of full habit. When the first examination s made, the passages were rigid, and the os uteri difficult to ch. Between six and seven hours after labor began, the patient, o was complaining much, was apathized with chloroform. In out two hours afterwards, the os uteri was fully dilated, and in ır hours and a half after the inhalation was begun, a large child s expelled. The placenta was removed, and the patient bound and dressed before she was allowed to awake. This patient uired an unusual quantity of chloroform; and Dr. Williamson, o remained beside her, states to me, in his notes of the case, he handkerchief was moistened often in order to keep up the sopo- ic effect. On one occasion, I allowed her to emerge from this te for a short time; but on the accession of the first pain she led out so for the chloroform, that it was necessary to pacify her giving her some immediately. In all, four ounces of chloroform re used." Like the others, she was quite unconscious of what d gone on during her anæsthetic state; and awoke altogether aware that her child was born.

ASE V.—Second labor. This patient, after being several hours labor, was brought to the Maternity Hospital. I saw her some ie afterwards, and found the first stage protracted by the right e of the cervix uteri being thick, œdematous, and undilatable. e inhalation of chloroform was begun, and the first stage was minated in about a couple of hours. Two or three pains drove child through the pelvic canal, and completed the second stage. teen minutes in all elapsed from the termination of the first to termination of the third stage, or the expulsion of the placenta.

The patient was dressed and removed into a dry bed, where she slept on for a short time before awaking, and being conscious of her delivery.

CASE VI.—Second labor. The patient, a person of small form and delicate constitution; bore her first child prematurely, at the seventh month. After being six hours in labor, the os uteri was fully expanded, and the head well down in the pelvic cavity. For two hours subsequently it remained fixed in nearly the same position, and scarcely if at all advanced, although the pains were very distressing, and the patient becoming faint and exhausted. She entertained some mistaken religious feelings against ether or chloroform, which had made her object to the earlier use of the latter; but I now placed her under its influence. She lay as usual like a person soundly asleep under it, and I was now able, without any suffering on her part, to increase the intensity and force of each recurring pain, by exciting the uterus and abdominal muscles through pressure on the lower part of the vagina and perineum. The child was expelled in about fifteen minutes after the inhalation was commenced. In a few minutes she awoke to ask if it was really possible that her child had been born; and was overjoyed to be told that it was so. I had the conviction that in this case the forceps would in all probability have been ultimately required, perhaps hours subsequently, provided I had not been able to have interfered in the way mentioned. I might, it is true, have followed the same proceeding though the patient was not in an anæsthetic state, but I could not have done so without inflicting great misery and agony upon her, and meeting with great resistance.

CASE VII.—A third labor. The patient had been twice before confined of dead premature children; once of twins, under the care of Mr. Stone of London; the second time of a single child, under my own charge. The liquor amnii began to escape about one o'clock A.M., but no pains followed for some time. I saw her between three and four, with the pains commencing, and the os uteri beginning to dilate. In two hours afterwards the first stage was well advanced, and, the pains becoming severe, she had the chloroform exhibited to her, and slept soundly under its influence. In twenty minutes the child was born, and cried very loudly without rousing the mother. In about twelve or fifteen minutes more she awoke, as the application of the binder was going on, and immediately demanded if her child was really born and alive, as she thought she had some recollection of hearing the nurse say so. She was rejoiced beyond measure on her son being brought in and presented to her.

Case VIII.—Fourth labor. The patient had borne three dead children prematurely, about the sixth and seventh months of utero-gestation. During her present pregnancy I placed her under strict rules and discipline; and she used, from an early period, small doses of chlorate of potass several times a day. She carried her child to the full time. Labor came on about one o'clock A.M. The membranes broke at eight A.M., when the os uteri was still very slightly open. It had made very little progress at ten o'clock, when Dr. Keith exhibited the chloroform to her. The pains continued very strong and regular, the passages relaxed, and at half-past eleven she was delivered of a large living child. The placenta came away immediately; and she was bound up, and her soiled clothes removed, before she awoke. She remembered nothing whatever that had occurred after she began to inhale the chloroform till the period of her awaking.

The preceding instances afford, perhaps, a sufficient number of examples of the use of chloroform in natural labor. In these and in all others which I have seen, or that have been reported to me, the immediate effects of the chloroform have been delightful. The mothers, instead of crying and suffering under the strong agonies and throes of labor, have lain in a state of quiet, placid slumber, made more or less deep at the will of the medical attendant, and, if disturbed at all, disturbed only unconsciously from time to time by the recurring uterine contractions producing some reflex or automatic movements on the part of the patient—like those of a person moving under any irritation of the surface, or from the touch of another, though still in a state of sleep. Nor have the ultimate consequences and results been less happy. No difficulties have been met with in the third stage; and the uterus has contracted perfectly after delivery. I never saw mothers recover more satisfactorily or rapidly—or children that looked more viable. And the practice is not a great blessing to the patient merely; it is a great boon also to the practitioner. For whilst it relieves the former from the dread and endurance of agony and pain, it both relieves the latter from the disagreeable necessity of witnessing such agony and pain in a fellow-creature, and imparts to him the proud power of being able to cancel and remove pangs and torture that would otherwise be inevitable. It transforms a work of physical anguish into one of painless muscular effort; and changes into a scene of sleep and comparative repose, that anxious hour of female existence, which has ever been proverbially cited as the hour of the greatest of mortal suffering.

The effects of the superinduction of anæsthesia in parturition are, if possible, still more marked and beneficial in cases of morbid labor and operative delivery. In proof of its influence in this respect, I shall cite some examples of its employment in cases of turning, of the application of the forceps, and of embryulsio.

CASE IX.—Fourth labor. The mother deformed, and the conjugate diameter of the brim of the pelvis contracted from the projection inwards and forwards of the promontory of the sacrum. Her first child was delivered by embryulsio: the second by the long forceps; the third was small, and passed without artificial assistance. On the present occasion, after suffering slight pains during the whole night, labor set in with great severity towards morning. After being in strong labor for some hours, she was seen first by Mr. Figg, and afterwards by Dr. Peddie, her ordinary medical attendant. I was called to her about four o'clock P.M. The pains were then enormously powerful and straining, imparting to the mind the dread of the uterus rupturing under their influence; but the head of the child was still altogether above the brim, and only an œdematous ridge of the scalp passed through the superior and contracted pelvic opening. The passages had become heated, the mother's pulse raised, &c., and Dr. Peddie had tried two different pairs of long forceps. After I arrived, he applied with great skill another pair of long forceps which I had with me; but it was found impossible to move the head in the least degree forwards. The urgency and power of the uterine contractions, the immobility of the head upon the brim of a deformed pelvis, and the state of the patient and of the parts, all showed the necessity of relief being obtained by artificial delivery. In her first labor I had assisted Dr. Peddie in delivering her under similar circumstances by perforation of the head. But here the child's heart was heard distinctly with the stethoscope, and he at once agreed to my proposition, that I should try to deliver her by turning the infant—compressing and indenting the flexible skull of the fœtus, instead of perforating it, and thus affording, as I have for some time past taught and believed, some chance of life to the child, and more chance of safety to the mother.[1] The patient was placed under the influence of chloroform still more deeply than when the forceps were used, in order, if possible, entirely to arrest the uterine contractions. I passed up my hand into the uterus, seized a knee, and easily turned the infant; but very great exertion and pulling was required to extract the child's head through the distorted brim. At last it passed, much compressed and elongated. The child was still-born, but, by applying the usual restorative means, it speedily began to breathe and cry. The child continues well, and the mother has made a rapid recovery.

[1] See vol. i. p. 449—"On turning as an alternative for Craniotomy," &c.

CASE X.—In the Maternity Hospital; first child. Labor began at 10 P. M. (21st Nov.) I was desired to see her at six A. M. (22d). The os uteri was well dilated, but it was evident that the pelvic canal was contracted throughout, and the head was passing with unusual difficulty through the brim. The patient was complaining much of her sufferings. It was clear that it would be a very tedious and probably at last an instrumental case, and one therefore calculated to test the length of time during which chloroform might be used. She began to inhale it at a quarter past six A. M., and was kept under its influence till a quarter past seven P. M., the date of her delivery; thirteen hours in all. From the time it was begun to he time delivery was completed, her cries and complaints ceased, and she slept soundly on throughout the day. The bladder required o be emptied several times with the catheter. The head passed the s uteri at ten A. M.; and, during the day, gradually descended hrough the pelvis. At seven P. M. I at last deemed it proper to deliver her by the forceps; the head, which was now elongated and edematous, having by that time rested for some hours against the ontracted pelvic outlet with little or no evidence of advancement, he bones of the fœtal cranium overlapping each other; and the œtal heart becoming less strong and distinct in its pulsations. A warm bath, irritation of the chest, &c., were necessary to excite full nd perfect respiration in the infant. Whilst we were all busied with the infant, the mother lost some blood; but the placenta was mmediately removed, and the uterus contracted perfectly. On afterwards measuring the quantity of blood lost, it was calculated to amount to 15 or 18 ounces. The mother's clothes were changed; she was bound up and removed to a dry bed before she awoke. She had at first no idea that the child was born, and was in no espect conscious of being delivered. In fact, she had been "sleep-ng," according to her own account, from the time she had begun he inhalation, and only thought she once or twice remembered or dreamed that she heard Dr. Williamson, the house surgeon, speak near her. Dr. Beilby, Dr. Zeigler, &c., saw the case with me. The mother and child have continued perfectly well.

In this, as in other cases, I have watched and noted the effects of the chloroform upon the duration of the pains and of the intervals, the rate of the fœtal and maternal pulse, &c.

CASE XI.—Patient with a deformed spine and contracted pelvic outlet. At her first confinement two different medical gentlemen had failed in effecting delivery by the forceps. At this, her second confinement, she placed herself under the care of Dr. Paterson of Leith. After being very long in labor, and the symptoms of the case becoming urgent, I saw her with Dr. Paterson. The head was low down in the pelvis; but it was placed in the right occipito-posterior position (the third of Naegele), and the forehead instead of the vertex was presenting, one orbit being easily felt behind the symphysis pubis. It had been lodged in nearly the same position for many hours. The fœtal heart was still distinct, but weak. I applied the forceps, turned the head round with them a quarter of a circle, into an occipito-anterior position (the second of Naegele); and, after being so adjusted, it still required considerable force to extract it. Before applying the forceps, the patient was sent into a state of deep anæsthesia by the inhalation of chloroform; and subsequently, when she awakened out of it, she was in no small degree surprised to find that she had really been delivered while she was sleeping and resting so soundly. The placenta separated, and the uterus contracted firmly. The child, which was large, lived for eight hours after delivery; but, despite of all the measures tried, full and perfect respiration was never established in it—apparently in consequence of some effusion or injury about the base of the brain. Unfortunately a post-mortem examination was not obtained. The mother has made an excellent recovery.

I quote the following instance of craniotomy under chloroform from a letter (dated 29th November), which I have received from my friend Professor Murphy of London. I give the case in Dr. Murphy's own words:

Case XII.—"I have tried the chloroform with great success in a case of distorted pelvis. It was the ovate deformity, the conjugate measurement being only 2½ inches; the head of the child could not enter the brim, and I was obliged to perforate. I got Dr. Snow to assist me in bringing her under the influence of chloroform. She made some resistance, and struggled a good deal at first, chiefly, I think, from apprehension that we were going to do something very dreadful; however, she soon began to inhale quietly, and gradually fell into a kind of dreamy sleep. I perforated the head, and labored with the crotchet, sometimes with the craniotomy forceps, for three quarters of an hour before I could get the head through the brim. She was at length delivered; the placenta was separated in about ten minutes, the bandage applied, soiled clothes removed, and she was made 'clean and comfortable,' as the midwives say. My patient was perfectly unconscious all this time, and did not awake for about a quarter of an hour after the operation; she did so then quite quietly, and was greatly surprised to find that all her miseries were over. There was no hemorrhage; but the uterus felt rather spongy and large. She is now recovering most favorably. I never had a case recover so far, so well."

Other cases, both of natural and morbid labor, in which the patients were delivered in an anæsthetic state from the inhalation of chloroform, have been reported to me by Dr. Protheroe Smith, Dr. Imlach, Dr. Robertson of Birkenhead, Dr. Malcolm, Dr. Buchanan, &c.; but as these and some other instances which I have myself seen, presented nothing new or different in their phenomena from the cases which I have already detailed, I have thought it unnecessary to give at present the details of them.

CHAPTER III.

ANSWER TO THE RELIGIOUS OBJECTIONS ADVANCED AGAINST THE EMPLOYMENT OF ANÆSTHETIC AGENTS IN MIDWIFERY AND SURGERY.[1]

"For every creature of God is good, and nothing to be refused, if it be received with thanksgiving."—1st Timothy, iv. 4.

"Therefore to him that knoweth to do good and doeth it not, to him it is sin."—James iv. 17.

Along with many of my professional brethren in Scotland, and perhaps elsewhere, I have, during the last few months, often heard patients and others strongly object to the superinduction of anæsthesia in labor, by the inhalation of ether or chloroform, on the assumed ground that an immunity from pain during parturition was contrary to religion and the express commands of Scripture. Not a few medical men have, I know, joined in this same objection;[2] and have refused to relieve their patients from the agonies of childbirth, on the allegation that they believed that their employment of suitable anæsthetic means for such a purpose would be unscriptural and irreligious. And I am informed, that in another medical school, my conduct in introducing and advocating the superinduction of anæsthesia in labor has been publicly denounced *ex cathedrâ* as an attempt to contravene the arrangements and decrees of Providence, hence reprehensible and heretical in its character, and anxiously to be avoided and eschewed by all properly principled students and practitioners. I have been favored with various earnest private communications to the same effect. Probably, therefore, I may be excused if I attempt, however imperfectly, to point out what I conscientiously conceive to be the errors and fallacies of those who thus believe that the practice in question ought in any degree to be opposed and rejected on religious grounds.

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[1] Published by Sutherland and Knox, Edinburgh, December, 1847.

[2] "Pain during operations is, in the majority of cases, even desirable; its prevention or annihilation is, for the most part, hazardous to the patient. In the lying-in chamber, nothing is more true than this: pain is the mother's safety, its absence her destruction. Yet, there are those bold enough to administer the vapor of ether, even at this critical juncture, forgetting it has been *ordered*, that 'in sorrow shall she bring forth.'"—On the "Injurious (?) Effects of the Inhalation of Ether;" in Edinburgh Medical and Surgical Journal for July, 1847, p. 258.

It is almost unnecessary to begin with premising, that those who object to the superinduction of anæsthesia in parturition upon religious grounds, found their objections principally on the words of the primeval curse which God pronounced after the temptation and fall of our first parents. Few or none, however, of those who have most zealously urged the existence of this curse as a reason against the employment of anæsthetic means in obstetric practice, have, I believe, made themselves at all intimate with the words and tenor of the curse itself. I shall therefore, in the first place, quote the words of it in full from the third chapter of Genesis, interpolating in *Italic* letters the Hebrew originals of those two nouns which are the more immediate subjects of doubt and difference of opinion.

GENESIS, chap. iii. v. 14.—"And the Lord God said unto the serpent, Because thou hast done this, thou art cursed above all cattle, and above every beast of the field; upon thy belly shalt thou go, and dust shalt thou eat all the days of thy life:

15. "And I will put enmity between thee and the woman, and between thy seed and her seed; it shall bruise thy head, and thou shalt bruise his heel.

16. "Unto the woman he said, I will greatly multiply thy sorrow (*'itztzabhon*) and thy conception: in sorrow (*'etzebh*) thou shalt bring forth children; and thy desire shall be to thy husband, and he shall rule over thee.

17. "And unto Adam he said, Because thou hast hearkened unto the voice of thy wife, and hast eaten of the tree, of which I commanded thee, saying, Thou shalt not eat of it: cursed is the ground for thy sake; in sorrow (*'itztzabhon*) shalt thou eat of it all the days of thy life;

18. "Thorns also and thistles shall it bring forth to thee; and thou shalt eat the herb of the field.

19. "In the sweat of thy face shalt thou eat bread, till thou return unto the ground; for out of it wast thou taken: for dust thou art, and unto dust shalt thou return."

In the form of a few separate observations, I will now add the remarks and answers which I wish to make. And I would begin by observing, that,—

1. The primeval curse is triple. It contains a judgment, First, upon the serpent (verses 14, 15); Secondly, upon the woman (v. 16); and, Thirdly, upon the ground for the sake of the man (v. 17-19.) With the first of these three curses—that on the serpent—and its apparent permanence (Isaiah lxv. 25), our present inquiry has nothing to do. It is enough for me to remark, that the second and third curses—on the woman and on the ground—are evidently, from different parts of the Holy Word, not immutable. God himself, on more than one occasion, promises the removal of them, and in general conjunctly, to the Israelites, provided they would

keep their covenants and obey his laws. See, for example, Deuteronomy, vii. 18, "I will bless the fruit of thy womb, and the fruit of thy land," &c.; xxviii. 4, "Blessed shall be the fruit of thy body, and the fruit of thy ground," &c. See also chap. xxviii. 11, &c. In Isaiah (xxviii. 23–29), man's culture by the plough, &c., of the ground cursed by God, is said to come from the providence of God himself. "For his God doth instruct him to discretion, and doth teach him" (v. 26); and, "This also cometh forth from the Lord of hosts, which is wonderful in counsel and excellent in working" (v. 29).

2. Those who, from the terms of the first curse, argue against the superinduction of anæsthesia in labor, aver that we are bound to take and act upon the words of the curse *literally*, "I will greatly multiply thy sorrow and thy conception;" or, as Gesenius and other Hebrew authorities state, that, being a case of Hendiadys, it may be *more* correctly rendered, "I will greatly multiply the sorrow *of* thy conception;[1] in sorrow thou shalt bring forth children." If, however, we are bound to take *this* part of the curse literally, and act accordingly, then we are bound to take and act also upon *all* other parts of the curse literally. If it is sinful to try to counteract the effects of this part of it, referring to childbearing women, it is sinful to try to counteract the other parts of it, regarding the state of the ground, and the judgment upon man. The agriculturist, in pulling up "the thorns and thistles," which the earth was doomed to bear, so far tries to counteract that part of the primary doom; and yet is never looked upon as erring and sinning in doing so. Or grant, as I have heard argued, that he may be entitled to pull up "the thorns and thistles," because the curse further implies that he was doomed to till the ground,—still he was doomed to till it by "the sweat of his face." Now if, I repeat, the whole curse is, as is averred, to be understood and acted on literally, then man must be equally erring and sinning, when, as now, instead of his own sweat and personal exertions, he employs the horse and the ox—water and steam power—sowing, reaping, thrashing, grinding machines, &c., to do this work for him, and elaborate the "bread" which he eats. The ever active intellect which God has bestowed upon man, has urged him on to the discovery of these and similar inventions. But if the first curse must be read and acted on literally, it has so far urged him on to these improper acts by which he thus saves himself from the effects of that curse. Nay, more; if some physicians hold that they feel conscientiously constrained not to relieve the agonies of a woman in childbirth, because it was ordained that she should bring forth in sorrow, then they ought to feel conscien-

[1] "Augebo tibi *Graviditatis* molestias."—Dathe's Pentateuchus, p. 38.

tiously constrained, on the very same grounds, not to use their professional skill and art to prevent man from dying; for at the same time it was decreed, by the same anthority, with the same force, that man should be subject to death,—"dust thou art, and unto dust shalt thou return." If, on the other hand, it be allowed that it is justifiable in the physician to try to counteract the effects of one part of the curse, and justifiable in the agriculturist to try to counteract the effects of another part, it is surely equally justifiable in the accoucheur to try to counteract the effects of a third part of it. But if, on the contrary, it is unjustifiable for him to follow out this object of his profession, it is equally unjustifiable for the physician and agriculturist to follow out the corresponding objects of their professions. Are those who maintain the uncanonical character of using human means to contravene the pains of childbirth ready, then, to maintain that we should not use human means to contravene the tendency to death, or to increase the fertility and produce of the ground except by personal labor, and the actual "sweat" of the brow? To be consistent, they must of necessity maintain this strange and irrational view of man, and of the duties and destinies which God has appointed for man. Or, otherwise, they must own that if it is right and meet in us to exert the human intellect so as to ameliorate the condition of man from the results of the fall, it is equally right and meet in us to employ the same means to ameliorate the condition of woman from the results of the same cause.

3. But does the word sorrow ("in sorrow thou shalt bring forth children") really mean physical and bodily *pain*, as is taken for granted by those who maintain the improper and irreligious character of any means used to assuage and annul the sufferings of childbirth? Now the word "sorrow" occurs three several times in two consecutive verses of the curse; (verses 16 and 17.) The corresponding word, or rather words, in the original Hebrew, as I have already shown when citing the terms of the curse, are *'etzebh*, and *'itztzabhon*. These nouns are both synonymous in meaning and origin, although longer and shorter in form (like labor, laboriousness—pain, painfulness—in our own language). All philologists agree that they are derived from the same root, viz., the verb *'atzabh*. The true and primitive meaning of a derivative word in the Hebrew, as in other languages, is generally the best attained by considering the signification of the root from which it is derived. The meaning of the verb *'atzabh* (the root of these nouns) is given as follows, by Professor Gesenius, the highest authority, I believe, I could quote on such a point. In his Lexicon he enters "*'atzabh*, 1. To *labor*, to *form*, to *fashion*. The original idea (says he) is perhaps that of cutting, whether wood or stones. 2. To *toil with pain*,

o *suffer*, to be *grieved ;* used also of the mind" (Tregelles' Transla-
ion of Gesenius' Hebrew and Chaldee Lexicon, p. DCXLVI.) Of
he disputed nouns, the noun *'etzebh* ("in sorrow—*'etzebh*—thou shalt
ring forth children") is nearest in form, and hence in meaning, to
he original verb-root *'atzabh*—and, I believe, no scholar would
eem it erroneous to affix to it the simple original signification,
labor," "*toil*," without deeming it requisite to believe that it at
ll farther necessarily imports that the implied labor and effort
nust essentially be to such an excess as actually to amount to the
upervention of pain and agony. In fact, the Hebrew word for
bor (in the sense of work or toil) is exactly like the English word
bor, used also to import the act of parturition. Certainly the
reatest characteristic of human parturition as compared with par-
urition in the lower animals, is the enormous amount of muscular
ction and effort (labor) provided for, and usually required for its
onsummation. The erect position (*vultus ad sidera erectus*) of the
uman body, renders a series of peculiar mechanical arrangements
nd obstructions necessary in the human pelvis, &c., for the pre-
ention of abortion and premature labor, and for the well-being of
he mother during pregnancy. But these same mechanical adapta-
ions and arrangements (such as the angle at which the pelvis is set
o the spine,—the great difference in the axis of the pelvic brim,
avity, and outlet—the rigidity of the soft structures, &c.) render
lso, at last, the ultimate expulsion of the infant in labor, a far
ore difficult, and more prolonged process than in the quadruped,
r instance, with its horizontal body. To overcome these greater
echanical obstacles, the human mother is provided with a uterus
nmensely more muscular and energetic than that of any of the
wer animals. The uterus of woman is many times stronger and
ore powerful than the uterus, for example, of the cow. In other
ords, I repeat, the great characteristic of human parturition is the
stly greater amount of muscular effort, toil, or labor, required for
s accomplishment.[1] The state of anæsthesia does not withdraw or
olish that muscular effort, toil, or labor; for if so, it would then
op and arrest entirely the act of parturition itself. But it removes
e physical pain and agony otherwise attendant on these muscular
ntractions and efforts. It leaves the labor itself (*'etzebh*) entire.

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[1] In some of the black tribes of the human race, the muscular efforts and exertions of
e uterus seem to be accompanied with comparatively little or no physical pain—there is
bor *without* suffering. But the black woman was cursed as well as the white; and surely
cannot be irreligious to *reduce* the sufferings of the civilized female to the degree and
nount which nature has left them existing in the uncivilized female of our race. There
e abundance of "maternal sorrows" connected with children and child-bearing in civilized
oman, quite independently of the actual agonies of parturition. My friend Dr. Churchill,
Dublin, some years ago, published a large octavo volume on the affections *peculiar* to the
regnant and puerperal states, without at all including those observable *during* labor.

And in relation to the idea that the Hebrew noun in the text truly signifies muscular *toil* and effort, and not physical *pain* and maternal agony, it is further highly important to remark, that in the very next verse (verse 17), viz., in the first part of the curse on man, the analogous Hebrew noun (*'itztzabhon*) which we translate by "sorrow," assuredly does *not* in any degree mean or imply mortal suffering or pain, but toil and labor. "In sorrow thou shalt eat of it (the ground) all the days of thy life." Indeed, the very same noun (*'itztzabhon*), when it occurs with the same meaning, and in relation to the same curse two chapters onwards—Genesis v. 29—is, in our version, rendered by the word "toil," and not "sorrow." "And he called his name Noah (rest or comfort), saying, This same shall comfort us concerning our work or toil (*'itztzabhon*) of our hands, because of the ground which our Lord hath cursed."

The word "sorrow" is a term at once simple and striking, but, at the same time, very comprehensive in its signification; and used under various specific meanings in our authorized English version of the Bible. In the Old Testament, above twenty different terms or nouns in the original Hebrew text, are translated by the single term or noun "sorrow" in the English text.[1] And perhaps it may not be considered irrelevant, if I remark that the identical Hebrew noun *'etzebh*, translated "sorrow" in the 16th verse ("in sorrow—*'etzebh*—thou shalt bring forth children"), recurs in six, and I believe only in six, other passages in the Old Testament; and in not one of these does it certainly imply physical pain. In two of these six places it is rendered, in our English version, by the very word "labor," in the signification of toil or work—viz. in Prov. xiv. 23, "In all labor (*'etzebh*) there is profit;" and Prov. v. 10, "Lest thy labors[2] (*'etzebh*) be in the house of a stranger." In one passage it is translated "anger,"[3] Prov. xv. 1, "Grievous words stir up anger (*'etzebh*)." In another passage in which it occurs, in Prov. x. 22, it is rendered sorrow, but still in the sense of toil and work—"The blessing of the Lord, it maketh rich, and he addeth no sorrow (*'etzebh*)[4] with it." In Psalms cxxvii. 2, it is also, in our English version, translated "sorrows"—"It is in vain for you to rise up early, to sit up late, to eat the bread of sorrows (*'atzabhim*, the plural of *'etzebh*)."[5] And, lastly, in Jeremiah xxii. 28, the same noun is translated "idol" (a thing made, worked, or fashioned), "Is this man Coriah a despised, broken idol (*'etzebh*)?"

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[1] See a list of these various Hebrew words which the translators of the English Bible have rendered by the word "sorrow," in "The Englishman's Hebrew and Chaldee Concordance of the Old Testament," p. 1639.

[2] "Labors," *i. e.* "things done with toil."—Gesenius.

[3] "A word pronounced with anger—a bitter, sharp word."—Gesenius.

[4] That is, no "heavy and toilsome labor."—Gesenius.

[5] "Bread obtained by toilsome labors."—Gesenius.

The context, I repeat, in these six Biblical passages in which the noun *'etzebh* recurs, shows that in *them* the word is not, in any respect, employed to designate the *sensation* of pain which accompanies the act of parturition in the human female. And it is surely not an unfair or illegitimate deduction, to infer that in the only *one* remaining, or seventh instance in which the word occurs in the Bible—viz., in Genesis iii. 16—it would be used in the sense in which it is generally elsewhere used—of effort, toil, or labor—and not in a new sense, in which it is nowhere else used—of the *feeling* or perception of excruciating suffering, or bodily anguish.

4. But that the preceding deduction is sound and just, admits of additional, and still stronger corroborative evidence. In various passages in the Bible, the proverbial agony and pain of a woman in travail is brought in—and particularly in the inspired language of the Prophets—as a striking and beautiful simile, to mark the greatest possible degree of anguish and suffering. In not one of these passages, in which the pure pain and super-sensitive suffering of the parturient mother are thus referred to, is the word in Genesis iii. 16, viz,—the word *'etzebh*—employed to designate this feeling of pain and suffering. Two other and totally different Hebrew nouns are used for this purpose in the passages to which I allude. These two nouns are *hhil* and *hhebhel.* They mark and designate the sensations of agony accompanying parturition, as contra-distinguished from the muscular efforts (or labor) (*'etzebh*) in which the physiological part of the process of the expulsion of the child essentially consists. To illustrate the particular signification thus attached to the words *hhil* and *hhebhel*, as contra-distinguished from *'etzebh*, I will cite the passages in which the two former nouns are used. In the following instances, the noun *hhil* is translated "pain," "pangs," &c.:—Psalm xlviii. 6, "Fear took hold upon them there, and pain as of a woman in travail." Jeremiah vi. 24, "Anguish hath taken hold of us, and pain as of a woman in travail." Jeremiah xxii. 23, "When pangs come upon thee, the pain as of a woman in travail." See, also, Jeremiah l. 43; Micah iv. 9, "Now why dost thou cry out aloud? is there no king in thee? is thy counsellor perished? for pangs have taken thee as a woman in travail." In the following instances, the noun *hhebhel* occurs in the original Hebrew with the same meaning attached to it:—Isaiah xiii. 8, "Pangs and sorrow shall take hold of them; they shall be in pain as a woman that travaileth." Isaiah xxvi. 17, "Like as a woman with child, that draweth near the time of her delivery, is in pain and crieth out in her pangs." See, also, Isaiah lxvi. 7; Jeremiah xiii. 21, and xlix. 23; Hosea xiii. 13, "The sorrows of a travailing woman shall come upon thee."

From what I have stated under the two preceding heads, we are then, I believe, justly entitled to infer that the Hebrew term which, in our English translation of the primeval curse, is rendered "sorrow" (Genesis iii. 16), principally signifies the severe muscular *efforts* and *struggles* of which parturition—and more particularly human parturition—essentially consists; and does not specially signify the *feelings* or *sensations* of pain to which these muscular efforts or contractions give rise. And, 2. On the other hand the *feelings* or *sensations* of excruciating pain accompanying the process of parturition, are designated throughout the Bible by two Hebrew words which are entirely and essentially different from that term which is translated "sorrow," in the oft-repeated expression—"in sorrow thou shalt bring forth children."

5. But even if—contrary to what, I think, the whole philological consideration or the very terms and words of the Bible shows to be the case—we were to admit that woman was, as the results of the primal curse, adjudged to the miseries of pure physical pain and agony in parturition, still, certainly under the Christian dispensation, the moral *necessity* of undergoing such anguish has ceased and terminated. Those who believe otherwise, must believe, in contradiction to the whole spirit and whole testimony of revealed truth, that the death and sacrifice of Christ was not, as it is everywhere declared to be, an all-sufficient sacrifice for all the sins and crimes of man. Christ, the "man of sorrows," who "hath given himself up for us an offering and a sacrifice to God," "surely hath borne our griefs and carried our sorrows;" for God "saw the travail of his soul, and was satisfied." And He himself told and impressed on his disciples, that His mission was to introduce "mercy, and not sacrifice."—(See Matthew ix. 13; xii. 7; also Hos. vi. 6.) At the end of his commentary upon the curse in the third chapter of Genesis, the sound and excellent Matthew Henry, in his own quaint, pithy, and zealous style, justly observes, "How admirably the satisfaction our Lord Jesus Christ made by his death and sufferings, answered the sentence here passed upon our first parents. 1. Did *travailing pains* come in with sin? We read of the 'travail of Christ's soul,' Isa. liii. 11; and the pains of death he was held by, are called ωδῖναι, Acts ii. 24—the 'pains of a woman in travail.' 2. Did *subjection* come in with sin? Christ was 'made under the law,' Gal. iv. 4. 3. Did the *curse* come in with sin? Christ was made 'a curse for us;' died a 'cursed death,' Gal. iii. 13. 4. Did *thorns* come in with sin? He was crowned with 'thorns' for us. 5. Did *sweat* come in with sin? He sweat for us, 'as it had been great drops of blood.' 6. Did *sorrow* come in with sin? He was 'a man of sorrows;' his soul was in agony 'exceeding sorrowful.' 7. Did

death come in with sin? He became 'obedient unto death.' Thus is the plaster as wide as the wound. Blessed be God for Jesus Christ."[1]

6. It may not be out of place to remind those who oppose the employment of anæsthetic means in labor on supposed religious grounds, that on the very same grounds many discoveries in science and art—even in the medical art—have been opposed upon their first proposition; and yet, *now* that their first introduction is over, and the opinions and practices they inculcate are established, no one would be deemed exactly rational who would turn against the present or future *continuance* of their employment any such improper weapon. I might adduce many instances, but one may suffice for all. When small-pox inoculation was introduced, towards the commencement of the last century, Rev. Mr. Delafaye and Mr. Massey published sermons against the practice as indefensible, on religious as well as medical grounds.[2] Inoculation was declared a "diabolical operation," and a discovery sent into the world by the Powers of Evil. And, again, when Dr. Jenner introduced vaccination instead of small-pox inoculation, towards the commencement of the present century, theological reasons again were not wanting for calling in question the orthodoxy of this other new practice. "Small-pox," argued Dr. Rowley, "is a visitation from God, and originates in man, but the cow-pox is produced by presumptuous, impious man. The former, heaven ordained; the latter is perhaps a daring and profane violation of our holy religion." And he subsequently proposed, "whether vaccination be agreeable to the will and ordinances of God, as a question worthy of the consideration of the contemplative and learned ministers of the Gospel of Jesus Christ; and whether it be impious and profane, thus to wrest out of the hands of the Almighty the divine dispensation of Providence!"[3] "The projects of these vaccinators seem," it was affirmed, "to bid bold defiance to heaven itself, even to the will of God."[4] "Providence," reasoned another author, "never intended that the vaccine

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[1] Exposition of the Books of Moses, p. 19.

[2] See Delafaye's sermon on "Inoculation; an Indefensible Practice."—Massey's "Sermon against the Dangerous and Sinful Practice of Inoculation." In his admirable "Account of the Inoculation of Small-pox in Scotland (1765)," Dr. Monro (*primus*) states, "the first and most general prejudice against inoculation is its being deemed a tempting of God's providence, and therefore a heinous crime."—P. 5. "Clergymen," observes Dr. Baron, in his Life of Jenner, vol. i. p. 231, "preached from their pulpits in this style of argument, if so it might be called. Some went so far as to pronounce inoculation an invention of Satan himself, and its abettors were charged with sorcery and atheism. These things," he adds, "would scarcely obtain credence were it not that similar arguments and assertions have been employed against vaccination itself."

[3] Blair's Vaccine Contest, p. 84.

[4] Rowley on "Cow-pock inoculation; with the Modes of treating the Beastly new Diseases produced by it," p. 9.

disease should affect the human race, else *why* had it no
this time, visited the inhabitants of the globe. The law of
continues, "prohibits the practice; the law of man and th
nature loudly exclaim against it."[1]

Such historical facts and efforts, and the results in wh
have invariably terminated, are surely sufficient to ma
cautious and hesitating against always recklessly calling
the same religious, or supposed religious, arguments under
circumstances.[2] Views and arguments of this description
every new practice intended to increase the well-being a
piness of mankind certainly are greatly more calculated
damage than benefit upon the interests of true religion.

Probably I may here be excused for adding, that my fri
fessor Miller informs me, that when reluctantly consenting
the elaborate article on Etherization which he afterwards
for the North British Review (No. for May, 1847), he stat
late Dr. Chalmers, who solicited him to undertake the tas
he "wrote the medical, Dr. Chalmers should himself
theological part." Dr. Chalmers at once professed that h
see any theological part pertaining to it. Mr. Miller then e
to him, that some had been urging objections against th
anæsthesia in midwifery, on the ground of its so far im

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[1] Dr. Squirrell's Preface to the second edition of his "Observations on Cow-
dreadful consequences of this new Disease," p. iv.

[2] Perhaps, in the history of misplaced religious arguments against all novel
practices, none in the retrospect may appear stranger than one that has been
mentioned to me during the few past months. Formerly, among my count
agricultural operations were performed, as commanded in the primeval curse,
exertion, and the "sweat of the face." Corn, in this way, was winnowed fro
by tossing it repeatedly up into the air, upon broad shovels, in order that any ac
rents which were present might carry off the lighter part. At last, however,
tury ago, "fanners," or machinery made for the production of *artificial* currents
same purpose, were invented and introduced into different parts of the countr
the more rigid sects of Dissenters loudly declaimed against the employment
machinery. "Winds (they argued) were raised by God alone, and it was irrelig
to attempt to raise wind for the aforesaid purpose for himself, and by efforts
Mr. Gilfillan, the well-known Scottish poet, has furnished me with evidence of
man debarring from the communion of the Lord's Supper those members of h
thus irreverently used the "Devil's wind" (as it was termed). And such sen
lieve, were not uncommon almost within the memory of some aged members of
generation. Sir Walter Scott, in his Old Mortality, introduces honest Mause
charging the Lady Margaret Bellenden and the authorities at Tillietudlem w
this reprehensible practice. "And since your leddyship is pleased to speak o'
us, I am free to tell you a piece o' my mind in another article. Your leddysh
steward hae been pleased to propose that my son Cuddie suld work in the barn
fangled machine for dighting the corn frae the chaff, thus impiously thwarting
Divine Providence, by raising wind for your leddyship's ain particular use by
instead of soliciting it by prayer, or waiting patiently for whatever dispensati
Providence was pleased to send upon the shieling hill." (Chap. vii.)

ling woman to avoid one part of the primeval curse. At last, Mr. Miller was enabled to convince him that he was in ear- in saying that such ground *had* been taken, Dr. Chalmers ght quietly for a minute or two, and then added, that if some all theologians" really took such an improper view of the sub- he would certainly advise Mr. Miller not to "heed them" in rticle. Dr. Chalmers' mind was not one that could take up rbor the extraordinary idea, that, under the Christian dispen- n, the God of Mercy should wish for, and delight in, the fice of woman's screams and sufferings in childbirth. Perhaps ought also, as I have heard other clergymen state, that if God eneficently vouchsafed to us a means of mitigating the agonies ildbirth, it is His evident intention that we should employ means. The very fact that we have the power by human ures to relieve the maternal sufferings, is in itself a sufficient rion that God would rather that these sufferings be relieved removed. If He had willed and desired them not to be averted, uld not be possible for man to avert them. For while it is duty to avoid all misery and suffering that is avoidable, it would inly be impossible for us to eschew any that God had perma- ly and irreversibly decreed should not be eschewed.

I have heard objections urged against the state of anæsthesia counteraction to pain in surgery and midwifery, on other and rent grounds from any I have yet noticed, viz., that in superin- ng a temporary absence of *corporeal* sensibility, we also super- ce, at the same time, a temporary absence of *mental* conscious- . And it is argued, that, as medical men, we are not entitled t the activity and consciousness of the mind of any patient in ance, for the mere purpose of saving that patient from any ly pain or agony. Some medical men even, have gravely sed this argument. But if there were any propriety in it, why , these same medical men could never have been justified in g what they have one and all of them done perhaps hundreds of s; viz., exhibiting by the mouth, opium and other narcotics and notics to their patients, to mitigate pain and superinduce anæs- ia and sleep. There is no greater impropriety or sin in pro- ing sleep and freedom from pain by exhibiting a medicine by the th than by exhibiting it by the lungs. There is *less* impropriety he latter practice than in the former, even according to the very trine of these opponents. For narcotic or anæsthetic agents ch are swallowed, are far more prolonged in their "insensibiliz- ' action upon both the mind and body than those that are inhaled. questionable character of the practice (supposing it for a moment e questionable), must be much less when the effect is short and evanescent, as with ether and chloroform when respired, than when it is long and protracted, as with opium, morphia, henbane, &c., when swallowed. The proper anæsthetic state is one physiologically and psychically analogous to natural deep sleep. It is an artificial deep sleep. Those who object and urge that we should never ourselves follow, or induce others to follow, the practice of voluntarily surrendering up our mental consciousness for a time, in order to avoid any corporeal torture or agony that we should otherwise endure during that time, forget how often and how long they and others are in the habit of voluntarily surrendering up their mental consciousness in common sleep, far, far beyond the time required merely for the refreshment and renovation of the system. Many thus *daily* surrender their minds and reason for unnecessary hours to the state of unconsciousness existing in common or natural sleep without any object except the reprehensible indulgence of sloth and indolence; and then they turn round, and declaim against others having induced upon them, at some *rare* and extraordinary time, the unconsciousness of artificial sleep, when there is a great and laudable object in view, viz., the avoidance of excruciating corporeal suffering, and the saving of human life by saving the human system from the shock and dangers accompanying that suffering.[1] Besides, those who urge, on a kind of religious ground, that an artificial or anæsthetic state of unconsciousness should not be induced merely to save frail humanity from the miseries and tortures of bodily pain, forget that we have the greatest of all examples set before us for following out this very principle of practice. I allude to that most singular description of the preliminaries and details of the first surgical operation ever performed on man, which is contained in Genesis ii. 21: "And the Lord God caused a deep sleep to fall upon Adam; and he slept; and he took one of his ribs, and closed up the flesh instead thereof." In this remarkable verse the whole process of a surgical operation is briefly detailed. But the passage is principally striking, as affording evidence of our Creator himself using means to save poor human nature from the unnecessary endurance of physical pain. "It ought to be noted (observes Calvin in his commentary on this verse), that Adam was sunk into a profound sleep, in order that he might feel no pain."[2] In his collected commentaries on the same verse, Pool quotes different authorities for the same opinion, that this deep sleep was induced upon Adam,

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[1] See evidence of its saving human life, as well as saving human suffering, under surgical operations, in a table which I have given of the results of amputations with and without anæsthesia, at p. 515, in "Remarks on the Superinduction of Anæsthesia in Natural and Morbid Parturition."

[2] "Notandum, Adam profundo sopore fuisse demersum, ut nihil doloris sentiret."—Johannes Calvini in Librum Geneseos Commentarius, Hengstenberg's edition, p. 36.

rder that "he might not feel pain from the removal of the rib."[1]
l the *profundity* of the sleep, as expressed in the Hebrew is also
thy of note. For the noun "*tardemah*," translated in our ver-
ι "deep sleep,"[2] signifies, according to all the best Hebrew
olars, the deepest form of induced slumber. In the early and
illiteral Greek translation which Aquilla made of the Bible, he
lers, in this passage, the Hebrew word *tardemah* by the expres-
Greek term *καταφορα*, a term which Hippocrates, Galen, Ætius,
other Greek physicians, used as implying that state of insensi-
ty and total unconsciousness which in modern medical language
express by "coma" and "lethargy."[3] Gesenius renders *tardemah*
the Latin word "sopor," the Hebrew term for common sleep
ng *shenah*. In the Vulgate it is translated "sopor" (*immisit Deus*
rem in Adam). In the quotation which I have given from Cal-
, that great authority renders the term *tardemah* by the expres-
ı profound "sopor" (*profundo sopore*); and Pool quotes different
horities to show that the Hebrew word does signify "sopor" of
rofound kind, "*notat profundum soporem.*"[4]

CHAPTER IV.

SAME SUBJECT CONTINUED, IN A LETTER TO DR. PROTHEROE SMITH OF LONDON.[5]

EDINBURGH, 8th July, 1848.

Y DEAR SIR—According to promise, I sit down to write you a
hurried notes on the subject of the avowed religious objections
the adoption of Anæsthesia in Human Parturition.

regret to hear from you that, in London, the progress of Anæs-
tic Midwifery is impeded by any groundless allegations as to its

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"Ne ablationis costæ dolorem sentiret."—Poli Synopsis Criticorum aliorumque Scripturæ
rpretum, vol. i. p. 29.—See also the same opinion expressed in Rosenmuller's Scholia
in Testamentum, vol. i. p. 106, "Adamo, somno sopito, ne dolorem sentiret:" and in
English Commentaries of Bishop Patrick, p. 14, "Whereby he was made less sensible
e pain which otherwise he would have felt in the opening his side;" and of Drs. D'Oyly
Mant, "Adam was thus less sensible of bodily pain;" &c. &c.

In Luther's German Bible, an exactly corresponding expression, "*tiefen schlaf*" is used.
athe's valued Latin version of the Pentateuch, a similar translation is given, "*Deus*
Adamo soporem immisit," p. 27.

" Cataphora (from *καταφερω*, to sink or fall down), a term used by some authors to desig-
a state of coma, and by others an unusually profound sleep."—Hooper's Medical Dic-
ry.

See his Synopsis Criticorum et Scripturæ Interpretum, p. 29.

See Appendix to Dr. Protheroe Smith's pamphlet on the "Scriptural Authority for the
ation of the Pains of Labor." S. Highley, London, 1848.

unscriptural character; and I can sincerely sympathize with you in your exertions to annihilate these scruples. Here, in Edinburgh, I never now meet with any objections on this point, for the religious like the other forms of opposition to chloroform, have ceased among us.

But in Edinburgh matters were very different at first. I found many patients with strong religious scruples on the propriety of the practice. Some consulted their clergymen. One day, on meeting the Rev. Dr. H——, he stopped me to say that he was just returning from absolving a patient's conscience on the subject, for she had taken chloroform during labor, and so avoided suffering, but she had felt unhappy ever since, under the idea that she had done something very wrong and very sinful. A few among the clergy themselves, for a time, joined in the cry against the new practice. I have just looked up a letter which a clergyman wrote to a medical friend, in which he declares that chloroform is (I quote his own words) "a decoy of Satan, apparently offering itself to bless woman but, in the end," he continues, "it will harden society, and rob God of the deep earnest cries which arise in time of trouble for help." And you are aware how earnestly some medical men attempted to preach, and, as you state, still preach against it on religious grounds. The medical friend who sent me the note from which I have quoted, himself read a wild and fanatical paper before the Medical Society of —— on the subject; and, I am told, it met with no small favor from the Society. I have enclosed a copy of this paper for your perusal. Some Lecturers on Midwifery, in London and Dublin, publicly adopted the same line of opposition and argument.

With the view of meeting, if possible, these strange and extraordinary objections, I wrote, in December last, a pamphlet on the subject of the so-called Religious Reasons against the employment of Anæsthetic Agents in Midwifery and Surgery. After its publication, I received a variety of written and verbal communications from some of the best theologians and most esteemed clergymen here and elsewhere, and of all churches, Presbyterian, Independent, Episcopalian, &c., approving of the views which I had taken. I have letters of the same kind from some men of high rank in your church; and a note in approval was brought to me, emanating from one of your most exalted and most esteemed episcopal dignitaries.

The pamphlet itself, however, was no doubt imperfect. It was principally written during a day's confinement to my room when convalescing from the prevailing influenza. I do not know what views you intend to take in your forthcoming publication, but there are some points on which, if I had time, I would perhaps have more insisted on in mine; and, if you will bear with me, I will briefly state them.

.. In the whole inquiry, nothing appeared to me more satisfac-
y or striking than the philosophic precision of the language of
Bible upon the point; and I did not sufficiently insist upon this,
an evidence of the fact that the primal curse on woman did *not*
er to the pure physical sufferings and agonies of parturition.
ch so-called labor-pain consists, as you well know, of two dis-
ct and separate elements; viz., *first*, of contraction of the uterus
l other assistant muscles; and, *secondly*, of sensations of pain,
re or less agonizing, accompanying these contractions, and
ectly resulting from them. Now, I have been often struck, as
u must have been, in chloroform labors, with the fact that, in the
esthetic state, not only does the uterus contract powerfully, but
abdominal muscles often do so also, and even the face of the
tient will sometimes betoken strong expulsive muscular action,
ile all accompanying suffering is quite annulled. We abrogate
second element of the so-called labor-pain, without destroying
first. We leave intact the expulsive muscular efforts, but
nove the sense and feeling of pain accompanying these efforts.
is only of late that these two elements or constituents of labor-
ins have been recognized and studied by the profession as *two*
parate objects. But it is surely, as I have above stated, worthy
remark and wonder, that the language of the Bible is, on this as
other points, strictly and scientifically correct, and long ago
de, with perfect precision, the very distinction which we are
wadays only recognizing. For the Hebrew noun, *'etzebh*, dis-
ctly signifies the muscular contraction or effort, and the nouns
l and *hhebhel*, as distinctly signify the sensations of pain accom-
nying these efforts; and you are aware, as I have elsewhere fully
wn, it is not the latter but the former of these nouns that is
d in the language of the primary curse: "In sorrow (*'etzebh*)
lt thou bring forth." Now, I repeat, the efforts of muscular
tractions (the *'etzebh* of the curse) are, as I have just stated, left
their full and complete integrity under the state of anæsthesia;
ile the pangs or sufferings (or *hhil*), against which the language
the curse does *not* bear, are alone annulled and abrogated.

2. Some of your London medical divines, however, argue, I hear,
t *'etzebh must* mean pain, and that, as meaning such, the curse
st be taken *literally;* and hence that woman *must* be allowed to
on suffering. In the pamphlet referred to, I have attempted to
swer this by showing that then we of the sex of Adam must
here literally, also, to the words of the curse, so far as they apply
us, and hence must earn our bread by the "sweat of our face,"
d by that only. Nay, the very physicians who thus insist on
ading and acting upon this and other texts literally—and literally

only—forget, I fear, that, according to their own doctrines, in practising physic, they are really and truly practising a profession of sin and iniquity, in so far as man in the primeval curse was doomed to die, and yet they daily and hourly persist in attempting to make him live. An esteemed clerical friend, in writing to me on the matter, stated that he was afraid *his* cloth was perhaps even more sinful than ours, if this outrageous view were true; for the introduction of sin was the consequence of the fall, and the church, in laboring to banish and abrogate that effect—in trying to turn mankind from sin—were actually trying to cancel the greatest and most undoubted effects of the first curse upon the human race.

3. But the accoucheurs and surgeons among you who object to the use of chloroform, on the ground that it goes, in their opinion, against the object and end of the primeval curse upon woman, strangely forget that the whole science and whole art and practice of midwifery is, in its essence and object, one continuous effort to mitigate and remove the effects of that curse. By warm baths, aperients, regulated diet, &c., they attempt to destroy the intensity of the approaching pains and penalties of childbirth;—during labor, they use counter-pressure on the back, to relieve the intense pains there; they use unguents, perineal support, venesection, &c., &c., to ease the pains and insure the safety of the mother. By these means they succeeded partially, in times past, in mitigating the sufferings and effects of parturition, and thought they committed no sin. But a means is discovered by which the sufferings of the mother may be relieved far more effectually; and then they immediately denounce this higher amount of relief as a high sin. Gaining your end, according to their religious views, imperfectly, was no sin—gaining your end more fully and perfectly is, they argue, an undiluted and unmitigated piece of iniquity. To relieve our patients, however, by our interference, a little, and a little only, is assuredly, in a moral and religious point of view, just as sinful as if we succeeded in affording them complete relief from suffering. The *principle* of interference is not altered by the *degree* of relief afforded being more or less, greater or smaller. "For whosoever shall keep the whole law, and yet offend in one point, he is guilty of all." If, on religious grounds, your obstetric friends object to relieving entirely a woman of her worst pains, now that they have the means of doing so, they must, on the very same grounds, refuse to relieve her imperfectly and partially of these or any other pains and sorrows connected with parturition; they must, or at least ought to abstain, in fact, from all obstetric practices whatsoever; they should, in short, give up their present profession as a profession of sin—and "in the sweat of their face" eat bread. I can see no other possible alternative for them,

provided, that is to say, they choose to reduce actually their theory into practice. If, on the other hand, they think it not sinful to relieve their female patients, to a small amount, from the alleged sufferings entailed upon them by the first curse, then surely it is not sinful in them to relieve their patients from their sufferings to a far greater amount, now that they have the power of doing so—nay, is it not sinful in them obstinately to withhold that relief? For, "to him that knoweth to do good, and doeth it not, to him it is *sin*."

These remarks apply to medical practitioners. And if any of your female patients hold the same groundless doctrine—a doctrine far more in accordance with the blindness and fatalism of Mahommedanism, than with the spirit and genius of Christianity—if they hold that it is improper, for scriptural reasons, to abrogate the pains and sufferings of childbirth, then such mothers cannot conscientiously content themselves with rejecting merely the use of chloroform in annulling the pangs of parturition; they must reject all kind of medical assistance in their hour of travail; they must give up, indeed, all assistance whatever. If the supposed pains and perils of the primeval curse are to be submitted to, on the ground that they are divinely appointed, and unavoidable ordeals—then they must be submitted to in all their unmitigated power and plenitude; no doctor must sinfully dare to stay the ebbing stream of life, if a fatal flooding suddenly supervene during labor; no nurse must venture, as heretofore, to relieve and mitigate the agonies of the shrieking mother by counter-pressure to her back, &c., "for whosoever shall keep the whole law, and yet offend in one point, he (she) is guilty of all."

4. Those who object to the adoption of anæsthesia in midwifery, on religious grounds, entirely forget that, if God had really willed the pains of labor to be irremovable, no possible device of man could ever have removed them. I have elsewhere attempted to state this argument, but it is so much better and more clearly given in a letter in my possession, from the pen of a clergyman, Dr. ———, who is acknowledged to be one of the ripest biblical scholars, and most profound theological critics and writers of the present day, that I will cite his letter at length to you. I received it a few days after the publication of the pamphlet I have referred to.

"I have just finished the perusal of your pamphlet, entitled, *Answer to the Religious Objections advanced against the Employment of Anæsthetic Agents in Midwifery and Surgery;* and I cannot refrain from expressing to you, though almost a stranger to you, the gratification which I have derived from it. I think your argument irrefragable, both as respects the question of philology, and as re-

tion of it to the relieving of the pains of childbirt
when many pious people had great scruples about
emancipate the negroes, on the ground that they w
dants of Ham, on whom the curse of perpetual sl
pronounced. I should not be surprised, in the cour
upon the emancipation of the Jews, to find some me
as some have pleaded in former times, that to give a
tion in any commonwealth, is a plain contraventior
law of God concerning that people.

"It has strongly appeared to me, for many year
two principles laid down in Scripture, a due regard
preserve good people from those hasty applications
dictions—whether minatory or otherwise. The on
Peter's course for the understanding of prediction
phecy is self-interpreting (ἰδιας ἐπιλυσεως),' from whicl
in the case of a prediction threatening, we are to ge
not from the words themselves in which it is couche
in connection with events or circumstances by whic
and Allwise fulfils his own declarations. The othe
truth that God's blessing and God's curse no one
that if any class enjoying God's blessing meet wit
class exposed to his curse enjoy relief or advantage, th
is, that the pain was not excluded by the blessing,
by the curse. Applying these principles to the ca
ably discussed, I came speedily to the conclusion th
not by chloroform, or anything else, set aside God'

ficent arrangements of the Christian dispensation—for all our greatest divines are agreed, I believe, on one point, viz., that this dispensation, in the application of its principles and precepts, is intended and calculated not only to regenerate and advance our moral condition, but more and more to ameliorate the physical sufferings and state of mankind. Witness, for example, the mighty power and resistless influence by which it has gradually acted, and is acting, through the development of its rules and doctrines, in the extermination from this earth of the curse of human slavery.*

6. Some thoughtlessly argue that the employment of anæsthetic means, and the abrogation of pain in labor, must be irreligious, because it is "unnatural." They seem to think that it looks as if we ancied that Nature, or rather that the God of Nature, had made the unction of parturition in some respects imperfect or improper in ts mechanism. These same individuals strangely forget that they hemselves do not think it "unnatural" to assist and supplement ther physiological functions of the body. They wear clothes to sist the protecting influence of the skin, and do not think that "unatural." They use cookery and condiments to aid the functions f mastication and digestion. Is this because they think that naure has left the functions of mastication and digestion imperfect in eir formation or mechanism? They constantly ride in coaches, c. Is the function of progression imperfect in man? "How unatural," exclaimed an Irish lady to me lately, "how unnatural it for you doctors in Edinburgh to take away the pains of your paents when in labor." "How unnatural," said I, "it is for you to ave swam over from Ireland to Scotland against wind and tide in steamboat." Many habits and practices—in fact, almost all the abits and practices of civilized life—are really and fundamentally as unnatural" as assisting the function of parturition by inducing ansthesia during it. But we do not look upon them as such, simply cause they have already passed into acknowledged and universal doption. Those who lived at the time when each separate iningement and improvement took place, could perhaps tell a story doubt and opposition not unlike that which we can now do with gard to anæsthetic midwifery. And those who have taken up is ground of opposition, in the present as in other cases, always em for the time being, to forget that it is God who has endowed an with mental powers calculated gradually to enable him to exnd his knowledge and improve his earthly condition, and that is extension and this improvement are so far evidently allowed nd willed by God himself.

7. An additional argument is suggested in a letter lying before e, from a gentleman of high name in the literary world, to his

son, who was a pupil of mine during the last winter. To understand its applicability, however, let me premise one or two words.

Some months ago, I published an extensive and carefully collected series of statistical returns from various British hospitals, showing the operations of surgery were much less fatal in their results when patients were operated on under the condition of anæsthesia, and consequently, without any attendant suffering, than when, as formerly, they were submitted to all the horrors and agonies of the surgeon's knife in their usual waking and sensitive state. Thus, I found that, while, before the introduction of anæsthesia, in every 100 cases of amputation of the thigh performed in our hospitals, from 40 to 45 of the patients died; the same amputation, when performed upon anæsthetized patients, did not prove fatal to more than 25 in the 100 cases; or in other words, out of every 100 such operations, the previous induction of anæsthesia was the means of preserving 15 or 20 human lives. So much are all our surgeons here impressed with the fact that the state of anæsthesia saves their patients alike from pain, and from the subsequent hazards and dangers of pain, that I believe not one among them would deem himself justified in submitting a fellow-being to the tortures of the operating table, without the previous employment of chloroform. And, I believe, you are aware that we also use it here, in Edinburgh, constantly in midwifery—its omission being the exception, and a rare exception, to the general rule of its employment. By thus shielding our patients against the more severe portion of the pains of parturition, we not only save them from much immediate suffering, but we save their constitutions also from the effects and consequences of that suffering; and, as a general rule, they assuredly make both more rapid and more perfect recoveries. I most sincerely believe, that in thus cancelling the pains of labor, we also, to a great extent, cancel the perils of it; for all our highest authorities in pathology, admit that pain, when either great in excess, or great in duration, is in itself, and by itself, deleterious and destructive: and the mortality accompanying parturition is regulated principally by the law of the length and degree of the patient's struggles and sufferings. In the Dublin Lying-in Hospital, when under Dr. Collins' able care, out of all the women, 7050 in number, who were delivered within *two* hours from the commencement of labor, 22 died; or *one in every* 320. In 452 of his cases, the labor was prolonged above *twenty* hours, and out of these 452 mothers, 42 died; or *one in every* 11; a difference enormous in amount, and one strongly calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

Now the writer of the letter to which I have alluded, is the author of one of the most eloquent essays in the English language, on the holy character and genuineness of the Bible. He is not a physician, though deeply read in medical, as in all other forms of knowledge; and, aware of the dangers and destructive properties of severe pain, when unmitigated and unrestrained, he reasons thus: "If pain, when carried—as in parturition—to the stage which we call agony, or intense struggle amongst the vital functions, brings with it some danger to life, as I presume no one can deny *must* be the case, then it will follow, that, knowingly to reject a means of mitigating, or wholly cancelling the attendant suffering and its dangers (now that such a means has been discovered), travels, in my opinion on the road to suicide. If I am right in believing that danger to life lies in this direction, then, clearly, the act of rejecting the remedy against it, being wilful, lies in a suicidal direction. It is even worse than an ordinary movement in that direction, because it affects to make God an accomplice through the Scriptures in this suicidal movement, nay, the primal instigator to it, by means of a supposed curse interdicting the use of any means whatever, though revealed by Himself, for annulling that curse." The same argument which is here brought against the wilful rejection of anæsthetic measures by the patient, necessarily applies with the same spirit, but with some changes in the terms, against the wilful rejection of the same means by the medical attendant.

But I must be done; for I fear I have exhausted your patience as well as my own time. Let me merely add, that I am sure you deeply regret and grieve with me that the interests of genuine religion should ever and anon be endangered and damaged by weak but well-meaning men believing and urging that this or that new improvement in medical knowledge, or in general science, is against the words or spirit of Scripture. We may always rest fully and perfectly assured that whatever is true in point of fact, or humane and merciful in point of practice, will find no condemnation in the Word of God.

With many apologies for the unexpected length to which these remarks have extended, believe me, my dear sir, very faithfully yours,

J. Y. Simpson.

gical Society in November last, I attempted to prove t
of cancelling and abrogating the pains inflicted by the
surgeon had not entirely originated in our own times.
that Dioscorides, Pliny, Apuleius, Theodoric, Paré, and
long ago described, and some of them apparently pr
induction of anæsthesia, previous to operations, both by
patients narcotic substances to swallow and narcotic vapo
While making the researches upon which the communica
to was founded, I further attempted to ascertain if any
proposed to assuage or annul, by the same or by other
pains attendant upon human parturition. I failed,
finding any traces whatever, either of any practical
abrogate or modify, by true anæsthetic means, the pai
or of any theoretical suggestions even as to the very p
effecting that desirable result. And I believe the hi
induction of anæsthesia in midwifery does not date fa
the history of anæsthesia in surgery. The first instan
the practice was adopted, occurred in my own practice in
on the 19th January, 1847. The case was one of defor
in which I had predetermined to extract the child by t
to try the inhalation of ether vapor upon the mother,
to facilitate that operation. During a week or two p
had anxiously waited for the supervention of labor in th
for, by the result, I expected that much would be decide
to the effect of ether-inhalation in parturition. Woul

ther-inhalation. The result was most satisfactory and most importa ant; for it at once afforded me evidence of the one great fact upon which the whole practice of anæsthesia in midwifery is founded—it proved, namely, that though the *physical sufferings* of the parturient patient could be annulled by the employment of ether-inhalation, yet the *muscular contractions* of the uterus were not necessarily nterfered with; or, in other words, that the labor might go on in ts course, although the sensations of pain usually attendant upon t were, for the time being, altogether abrogated.

This case, with its more obvious results and inferences, was communicated to the Obstetric Society at their meeting on the 20th of anuary.[1] In the course of the subsequent three weeks I had an pportunity of trying anæsthesia in several cases of natural labor nd in one forceps case; and at the next meeting of the Obstetric ociety, on the 10th February, I took an opportunity of bringing he subject under the attention of the members at greater length. n the published reports of the Society,[2] the various inferences which hen appeared to me to be deducible in regard to it are given in the llowing terms:

1. That the state of anæsthesia procured for the patient a more or less perfect immunity om the conscious pain and suffering attendant upon labor.
2. That it did not, however, diminish the strength or regularity of the contractions of the rus.
3. That, on the other hand, it apparently (more especially when combined with ergot) metimes increased them in severity and number.
4. That the contraction of the uterus after delivery seemed perfect and healthy when it as administered.
5. That the reflex assistant contractions of the abdominal muscles, &c., were apparently ore easily called into action by artificial irritation, and pressure on the vagina, &c., when e patient was in an anæsthetic state.
6. That its employment might not only save the mother from more pain in the last stage of bor, but might probably save her also, in some degree, from the occurrence and conseuences of the nervous shock attendant upon delivery, and thereby reduce the danger and tality of childbed; and,
7. Its exhibition did not seem to be injurious to the child.

Full details of some of the principal cases upon which these infeences were founded, were, along with other additional instances, ubsequently thrown together and published in the form of a communication to the *Monthly Journal of Medical Science.*[3] In that aper I made some observations on the question, Whether it would e proper to employ anæsthesia in natural labor? I adduced various easons from physiology and pathology for believing that the partuient action of the uterus would go on healthily and uninterruptedly though the influence of the mind and purely cerebral functions were suspended, and that the dangers of the nervous shock attendant upon labor would be lessened; and I pointed out the necessity of ascertaining, by a cautious series of observations, what counter-indications there might be to the employment of the practice;—whether it was ever apt to give a tendency to hemorrhage or other complications; its influence, if any, upon the child; the length of time its use might be continued in any one case, &c.

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[1] Monthly Journal, vol. 1846-47, p. 639.

[2] Ibid. p. 795.

[3] "Notes on the Employment of the Inhalation of Sulphuric Ether in the Practice of idwifery."—Monthly Journal of Medical Science for March, 1847, p. 721.

At the date at which the paper that I have just referred to was written, viz., the 18th February, the longest time during which I had ventured to keep a parturient patient in the anæsthetic state was about half an hour. And many who believed that this state might be induced without danger for a few minutes, entirely doubted whether it could be sustained for any great length of time without extreme hazard. During the experience, however, of the next two or three weeks, I ascertained the fact, that the anæsthetic action could be safely kept up during labor for one, two, three, or more hours. At one of the subsequent meetings of the Obstetric Society this result and others were adduced,[1] and the following additional deductions drawn, as stated in the words of the printed proceedings of the Society, viz., that,—

1. The state of anæsthesia had little or no influence upon the fœtus, none, at least, of a deleterious kind—the fœtal heart increasing only a few beats, if at all, when the mother was kept long and fully anæsthetized, either during pregnancy or labor.
2. The mother, during labor, may be kept anæsthetized, if required, for one, two, three, or more hours. Dr. Simpson described two cases, in one of which the mother was about six, and in the other, about four hours anæsthetized before the children were born. In both cases the duration of the intervals and of the pains before and during anæsthesia was noted (as in the experiments which Dr. Simpson had published on galvanism), and the anæsthesia seemed to have no effect either on their frequency or strength. But,
3. In two or three cases, Dr. Simpson had seen a very deep state of anæsthesia modify apparently the full strength of uterine contractions, but they recurred immediately in full force, when the patient was allowed to fall back into a state of slighter anæsthesia
4. Dr. Simpson had hitherto seen no traceable injury to either mother or child from its employment, but the reverse.
5. The inhaler he used was either a concave sponge saturated interiorly with ether and held over the face, or a simple portable flask without valves.

The first case of labor in which I employed artificial anæsthesia occurred, as I have already stated, on the 19th January, 1847. This case and its results were stated publicly, on the following day, to my class in the University, and immediately became extensively known to the profession through the medium of the public journals.[2] In the course of a short time the practice of anæsthesia began to be tried in other medical schools. On the 13th February, Dr. Murphy of London stated to the Westminster Medical Society that he had employed it in a case of turning.[1] On March 27th a case was published of the use of ether in natural labor,[2] by Mr. Lloyd. Dr. Protheroe Smith delivered a patient under a state of anæsthesia on the 28th of March; and in the Lancet for 1st May he published a paper, "On the Employment of Ether by Inhalation in Obstetric Practice," giving an account of this and two later cases in which he had recourse to anæsthesia during labor. He afterwards sent to the same journal several additional cases and remarks.[3] Mr. Lansdowne of the General Hospital, Bristol, subsequently published various cases in which the practice was successfully employed. His first case occurred on the 8th April.[4]

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[1] Monthly Journal of Medical Science, 1847–48, p. 214.

[2] See Medical Gazette for 1847, vol. xxxix. p. 460. Also Provincial Journal for 1847, p. 84.

In Ireland, the first case delivered in a state of artificial anæsthesia was on the 28th November, 1847. The patient was under care of Dr. Tyler of Dublin. It was an instrumental labor.

In France, the practice was much more early tried. In about a week after the first case occurred in Edinburgh, Fournier Deschamps delivered a patient by the forceps when she was in a state of anæsthesia.[5] This was on the 27th of January. On the 8th of February, Professor Paul Dubois exhibited ether in a case of forceps operation at the Hôpital de la Maternité of Paris, and up to the 23d February had used it in four other deliveries. He has not himself published, so far as I am aware, any written observations of his own upon the subject; but he early brought the question, in an interesting and able form, before the French Academy of Medicine,[6] and various reports of this important oral communication immediately appeared in different journals.[7]

Professor Dubois' conclusions, as officially reported in the Bulletin de l'Académie, were as follows:[8]

1. The inhalation of ether can annul the pain of obstetrical operations.
2. It can suspend the physiological pains of labor.
3. It destroys neither the uterine contractions, nor the contractions of the abdominal muscles.
4. It diminishes the natural resistance of the perineum.
5. It does not appear to act unfavorably on the health or life of the infant.

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[1] Lancet for February 27, 1847.—Before this date, viz., on the 26th of January, a state of anæsthesia was attempted to be induced in a patient upon whom the Cæsarean section was performed by Mr. Skey of St. Bartholomew's Hospital, London. "But the inhalation of the vapor of ether was unsuccessful, or but very partially successful."—Lancet, vol. i. 1847. p. 140.

[2] Medical Times, 1847, p. 96.

[3] Lancet for 1847, vol. ii. p. 121, and p. 305.

[4] Lancet, vol. i. 1847, p. 446.

[5] Gazette des Hôpitaux, 30 Janvier, 1847.

[6] Bulletin de l'Acad. Roy. de Méd. tom. xii. p. 400.

[7] "Le 23 Février, M. P. Dubois, qui avait eu connaissance des récherches de M. Simpson, communiqua à l'Académie de Médecine le resultat de ses experiences."—Chambert. Des Effets des Ethers, p. 231.

[8] Bulletin, tom. xii. p. 407.

Professors Villeneuve[1] of Marseilles, and Stoltz[2] of Strasbourg, subsequently published some additional cases and observations.

In Germany, the first case of anæsthetic midwifery, of which I have found any published notice, occurred on the 24th February, 1847, under the care of Professor Martin of Jena.[3] The ether was administered during the operation of separating and extracting an adherent placenta. Professor Siebold[4] read a paper on the subject before the Royal Scientific Association of Göttingen on May 8. He had employed ether in several cases of natural and instrumental labor. On the 3d June, Professor Grenser of Leipsic[5] anæsthetized a patient for a forceps operation, and afterwards resorted to it in several other natural and instrumental cases.

In America—the country to which we are indebted for the first knowledge of the anæsthetic effects of sulphuric ether in surgical operations—the same agent was not employed in midwifery till the reports of its use in obstetric practice in Europe had recrossed the Atlantic. Dr. Channing, Professor of Midwifery in Harvard University, was, I believe, the first to employ it in labor.[6] He adopted the practice in two forceps cases; the first of which occurred on May 5, the other on May 15, 1847. Drs. Clark,[7] Putnam,[8] and other American practitioners, have latterly published the results of their experience in anæsthetic midwifery.

In November, 1847, a new impulse was given to the practice of anæsthesia in midwifery, by the introduction of chloroform as a substitute for sulphuric ether. The ether required to be exhibited in large quantities in order to keep up its action; and hence it was objectionable in ordinary obstetric practice from its bulk, and the inconvenience of its carriage. Most medical men believed that an apparatus of more or less considerable size was necessary for its proper and effectual exhibition, thus further encumbering the practitioner. These and other difficulties were found not to appertain to the use of chloroform, and many, in consequence, adopted it in midwifery, who had previously altogether rejected the employment of sulphuric ether. The first case of labor in which I employed chloroform occurred on the 8th of November. On the 1st December, I reported to the Edinburgh Medico-Chirurgical Society, a series of cases illustrative of its effects and use in natural and operative labors.[1] In the London weekly journals, since that period, Dr. Protheroe Smith, Dr. Murphy,[2] Dr. Rigby, Mr. Lansdowne, Mr. Brown, Dr. Bennett, Mr. Phillips, and others, have published cases and reports upon the subject.

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[1] De l'Ethérization dans les Accouchements. Marseille, 1847.

[2] Gazette Médicale de Strasbourg, 27 Mars, 1847.

[3] Ueber die Künstliche Anæsthesie bei Geburten. Jena, 1848.

[4] Medical Gazette, vol. xxxix. 1847, p. 1052.

[5] Ueber Aether-einathmungen während der Geburt. Leipzig, 1847.

[6] Two cases of the inhalation of ether in instrumental labor. Boston, 1847.—From the Boston Medical and Surgical Journal. Dr. Channing, in a postscript to an American reprint of one of my Essays on Chloroform, has announced his intention of soon publishing a volume "On the Employment of Etherization in Childbirth." (This has since appeared—D.)

[7] Philadelphia Medical Examiner, March, 1848.

[8] Boston Medical and Surgical Journal, February 2, 1848.

CHAPTER VI.

RESULTS OF THE PRACTICE OF ANÆSTHESIA IN MIDWIFERY.[3]

SINCE January, 1847, up to the present time (October, 1848), I ave, in my own practice, delivered about 150 patients under a tate of anæsthesia. The results to the children and mothers have been as follows:

Results to the Children.—In the 150 cases, all the children were orn alive except one. In this exceptional instance, the infant was xpelled in a decomposed and putrid state, between the seventh nd eighth month of utero-gestation. It had not been felt to move, or had I been able to hear the fœtal heart with the stethoscope, r two or three weeks previously. The mother had, before the resent pregnancy, borne several premature dead children. Though he infant was small, yet the suffering attending upon its expulsion hreatened to be excessive, and, to relieve the mother of this unnecessary agony, I placed her under the influence of chloroform.

During the few weeks of my obstetric attendance after delivery pon these 150 cases, only one of the children died; namely, a hild who sank under the symptoms of cyanosis. Nor am I aware hat any of them has suffered, up to this time, from "cerebral effuions," or "convulsions," or "hydrocephalus," or any other of the ffections which have been prophesied as certain to befall all such nfants as should be borne in labors rendered painless by art. Perhaps it may be proper to add, that none of the children have shown ny symptoms of what has been calmly averred, in more than one publication in London, as a possible, or rather probable result of næsthesia, viz., idiocy.[4]—"Dixerit insanum qui te totidem audiet."

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[1] See Lancet for November 20, 1847, p. 533, and December 11, p. 613; also Medical azette for November 26, p. 934.

[2] See also Dr. Murphy's able Harveian Oration on the employment of Chloroform.

[3] From Edinburgh Monthly Journal of Medical Science, October, 1848, p. 214.

[4] How can we "know or ascertain the possible consequences of the use of such an agent the brain of the child? And how can we calculate what may be the ultimate conse-

Results to the Mothers.—Among the 150 mothers, the more immediate and direct effect of anæsthesia has been the alleviation or abolition of the physical sufferings attendant upon the latter stages of labor. And, certainly, if the object of the medical practitioner is really twofold, as it has always, till of late, been declared to be, viz., "the alleviation of human suffering and the preservation of human life," then it is our duty as well as our privilege to use all legitimate means to mitigate and remove the physical sufferings of the mother during parturition. The degree of these sufferings is, as a general rule, assuredly such as to call for this aid and mitigation. In proof of their severity, I might cite the unprejudiced testimony of various obstetric authors. At present, I shall content myself with one. Professor Meigs of Philadelphia—a declared opponent of the innovation of artificial anæsthesia in labor—when speaking of the sufferings of the mother in human parturition, fully admits their intensity. "*What* (says he) do you call the pain of parturition? There is no name for it but *Agony;*"[1] and he elsewhere speaks of the pains in the last stage "as absolutely indescribable, and comparable to no other pains."[2] Now, surely, if it be the duty of the physician (and who doubts it?) to relieve and remove the pains of colic, of pleurodyne, of headache, neuralgia, rheumatism, &c. &c., it is his duty to relieve pain so severe as to be "absolutely indescribable, and comparable to no other pains." There is not one code of humanity for one class of pains and patients, and a different and opposite code for another class of them.

From November last, when I began to use chloroform in labor, up to the present time, none of the patients, with one exception, at

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quences of its action in reference to the development of the mental faculties?"—Dr. Malan, in Lancet for April 29, 1848.

"It is admitted by all that the pulsations of the fœtal heart are greatly increased during inhalation—indeed, to such an extent has this been noticed, that in some instances the pulsations could not be counted, so much were they accelerated. Are not effusions to be feared from this? Are not convulsions after birth likely to ensue? And may not that occur which would make the most heartless mother shudder at the bare possibility of herself, by her want of courage, being instrumental in producing? May not *Idiocy* supervene? Of this we have as yet no experience, nor shall we have, perhaps, for years; but when one such case occurs, will there then be found any one who will afterwards be persuaded to submit herself to etherization during pregnancy?"—Mr. Gream, in London Medical Gazette for 7th September, 1848. It is perhaps superfluous to add, that the premises of the preceding paragraph are as gratuitous as its conclusions; and that the pulsations of the fœtal heart are little, if at all, increased in rapidity when the mother is anæsthetized. "The action of the child's heart," says Professor Siebold, "was found to continue quite unaltered, not the slightest change in its frequency and regularity being detected."—Siebold on the Employment of Ethereal Inhalations in Midwifery, in the Medical Gazette for 11th June, 1847.

[1] Females and their Diseases, p. 49. I leave the italics the same as in the original.

[2] Philadelphia Practice of Midwifery, p. 153.

whose delivery I have attended, has been aware of these last "absolutely indescribable" pains; the state of artificial anæsthesia having always been induced for a longer or shorter time before their supervention. And I have kept up this state for a period varying from a few minutes to four, five, six, or more hours before delivery. In the exceptional case referred to, the patient's sufferings were greatly mitigated; but the state of anæsthesia was not, as usual, perfect and complete, the patient having been unexpectedly taken in labor when not in her own house, and the attendant anxiety and confusion of herself and her attendants being such as totally to preclude the requisite degree of quietude. When employing ether, I repeatedly saw cases in which the patients were thus only partially and not completely anæsthetized—where, in other words, they were not entirely asleep, but were aware of the presence of the uterine contractions, and sometimes experienced from them sensations in some degree painful, but of a very mitigated and blunted character.

Besides thus alleviating and abolishing the sufferings of the mother during labor, the practice of anæsthesia carries along with it other advantages. A number of patients have spontaneously told me, that the prospect of being enabled to pass through the ordeal of parturition with the assistance of anæsthetic agents, and without their usual painful agonies, has destroyed, in a great measure, that state of anxiety and dread of anticipation, which, in former pregnancies, had, for weeks and months previously, silently annoyed and haunted them. If we can thus add to the happiness of our patients, by imparting to them feelings of safety and immunity under one of the severest trials to which nature exposes them, we surely follow out, in its truest sense, that which Dr. Meigs correctly describes as the office of a physician—namely, "a great mission of benevolence and utility."

But the practice of anæsthesia in midwifery not only saves the mother from the endurance of unnecessary mental anxiety and unnecessary physical agony; it saves her also from some of the dangers attendant upon parturition, by husbanding her strength and warding off the effects of that exhaustion and nervous depression which the pains and shock of delivery tend to produce. In most cases the mothers, after delivery, on waking from their anæsthetic sleep, have expressed surprise at their own feelings of strength and perfect well-being; and many, who have borne children previously, have gratefully declared to me the great difference which they have found between their condition after being delivered under anæsthetics, and without pain and suffering, and their state of prostration after former labors, when they were subjected to the endurance of all the usual "pangs and agonies" of parturition. Nor does the benefit end here. By annulling the parturient pains and shock, and their direct and primary depressing effects upon the constitution, we ward off, I believe, to a more or less marked extent, the chances and dangers of those secondary vascular excitements which are always apt to follow indirectly upon them. We increase the chances of a more speedy and a more healthy convalescence. And both patients and practitioners have, as a general rule, had occasion to observe, that the period of convalescence has been evidently curtailed and shortened by the previous adoption of anæsthesia during delivery.

Such certainly has been my own experience. For, since following the practice of anæsthesia, my strong conviction is, that I have seen both more rapid recoveries than formerly, and fewer puerperal complications. One patient, however, had a short attack of peritonitis, requiring leeches, &c. It was her third accouchement and her first living child; and, after her two former deliveries, she had required to be bled, and treated for similar inflammatory attacks. At her first labor she suffered severely from puerperal convulsions. In two others of my patients the convalescence was delayed, in one by an attack of the affection described by Dr. Marshall Hall as "intestinal irritation" in the puerperal female; and in the other by a fit of jaundice, which supervened two or three weeks subsequently to delivery, and after the patient had been for several days in the drawing-room. In December and January last, an epidemic of puerperal fever swept fatally over Edinburgh and other parts of Scotland. During the period of its prevalence, two of my patients were seized with it and died. But the previous employment of anæsthesia in these cases had nothing to do with this distressing result. Some of my professional brethren here and elsewhere, who were not using ether or chloroform, were much more unfortunate than I was. In a district in the neighborhood of Edinburgh, one of the medical attendants informed me that, at that time, above twenty mothers were attacked and died, and in none of those whom the disease seized upon, did ether or chloroform happen to be used; while several who demanded chloroform during their labors, all fortunately escaped. The first of the two cases which I met with was after a second labor. The patient's first labor was extremely tedious and prolonged, and, at last, symptoms supervened which demanded the delivery of the child by the forceps. In her second delivery, the labor was much shorter; the second stage lasted only for about twenty minutes, and during it she was completely anæsthetized. For fifty hours after delivery she progressed most favorably; and after seeing her at that time with a pulse at eighty, and otherwise well, I was suddenly summoned, in consequence of extremely severe pain having come on in the uterine region after some muscular exertion. Rigors, rapidity of the pulse, tympanitis, &c., supervened, and she speedily sank, with all the usual symptoms of puerperal peritonitis. The second case alluded to was in a primipara. The labor was tedious, the pain severe, and the patient was anæsthetized for four or five hours before delivery. For some days after delivery she went on prosperously, until she became unhappily and greatly excited by discovering intemperate habits on the part of the monthly nurse who was taking charge of her infant and herself. A fit of convulsions (a disease to which, in earlier life, she had been long subject) immediately supervened, and recurred several times. Fatal febrile symptoms then set in, with tympanitis and excessive diarrhœa.

I may add that, in the period during which these 150 cases occurred, I have had under my professional charge 20 or 30 other cases of labor in which anæsthesia was not employed, from the rapidity and facility of the delivery, from the patient being too late to send for assistance, from an aversion on the part of patients to the use of anæsthetics, more especially when ether first began to be used during last year, or from other causes.[1] One of the children in these cases was still-born, and a second died two or three days after delivery. Two of the mothers suffered from crural phlebitis; a third had a severe attack of puerperal fever, but recovered. Two others died; one of them under an attack of puerperal convulsions and coma, which supervened fourteen days after delivery.[2] In the other fatal case, the patient, who had suffered much in her previous labors, came to the immediate neighborhood of Edinburgh to be confined, and with the view of using chloroform. But the labor proved unusually rapid, and she was delivered before the call for assistance reached my house. Her recovery went on uninterruptedly for two weeks, when a severe attack of dyspnœa supervened. My friend, Professor Miller, her ordinary medical adviser, saw her in my absence, and suspected some acute affection of the heart. When we visited her together shortly afterwards, the symptoms were then apparently those of acute endocarditis. She was submitted to the usual antiphlogistic treatment, and in four or five days felt again so well as to insist upon being allowed to rise, which was forbidden. In the course of a few hours afterwards, another fit of dyspnœa suddenly supervened, and, before Mr. Miller reached the patient's house, she was dead. We did not procure an autopsy. If unfortunately she had used chloroform during the labor, as was her intention and wish, many of the objectors to its employment would, I fear, have unhesitatingly attributed the fatal issue in this case to its previous employment.[1]

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[1] Since November last I have used chloroform in all the cases of labor, where I have been called in time, except two.

[2] See details of it in Monthly Journal for 1847, p. 213.

In addition to the 150 cases of artificial anæsthesia that have occurred in my own midwifery practice, and to which the preceding remarks apply, I have witnessed, during the last eighteen months, a considerable number of instances in which anæsthetic agents were employed in consultation and hospital practice; and I have frequently had recourse to their assistance in various obstetric operations which I have been called upon to perform, as in the separation and removal of the placenta, in various cases of turning, in one craniotomy case, and in several patients who required to be delivered instrumentally by the long or short forceps.[1] In all these varieties of operative practice, the previous superinduction of anæsthesia has appeared to me to be of the greatest and most undoubted benefit. For, besides freeing the mother from the additional corporeal suffering and additional mental anxiety attendant upon operative delivery, the state of anæsthesia enables the practitioner to apply any operative interference that may be necessary with more ease and facility to himself, and consequently also with more safety and success to his patient. When the state of anæsthesia is rendered adequately deep, it renders the patient quiet and unresisting during the required operative procedures; it prevents, on her part, those sudden shrinkings and changes of position which the boldest and firmest woman cannot sometimes abstain from when her mind and body have been worn out, as happens in most operative cases, by a previous long and protracted endurance of exhausting but still ineffectual labor pains;—the introduction of the hand into the maternal passages, or of the hand to guide our instruments, is greatly facilitated both by the passiveness and apathetic state of the mother, and by that relaxation of the passages which deep anæsthesia almost always induces; and, lastly, this state of relaxation and dilatability renders the process of the artificial extraction of the infant through these passages alike more easy for the practitioner, less dangerous for the child, and more safe for the structures of the mother. Besides, in midwifery, as in surgery, the utility of anæsthesia before operating, is not, I believe, limited to the mere annulment and abrogation of conscious pain on the part of the patient, and the rendering of the operation itself more easy to the practitioner, but it adds to the safety of our instrumental or artificial interference. For, in modifying and obliterating the condition of conscious pain, the "nervous shock" otherwise liable to be produced by such pain, particularly wherever it is extreme in degree or duration, or intensely waited for and endured, is saved to the already tried and shattered constitution of the mother; and thus an escape is so far gained from those states of immediate vascular and nervous depression, and of subsequent febrile and inflammatory reactions, that are always apt to follow more directly or indirectly in its train.

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[1] In order to show the caution that is necessary in reasoning upon cases of death apparently from the exhibition of chloroform during surgical operations, I may add that, since November last, scarcely an operation has been performed in Edinburgh without previous anæsthesia, except where the throat or mouth was the seat of incisions, or the operation itself slight and trivial. Amid all the numerous patients thus operated on in public or private practice, when under the use of chloroform, no kind of misadventure or accident has happened; except one case of temporary fainting, a few minutes after recovery from the state of anæsthesia, be regarded as such. On the other hand, among the *few* exceptional cases in which, since November last, patients have been operated on in this city without chloroform, two have died on the table. One of the two was being operated on by Professor Miller for a hernia, which had been strangulated for about fifteen hours; when, after the skin merely was divided, the patient complained of great faintness, vomited, and died with the operation unfinished. This occurred on the 8th of November, two or three days after the anæsthetic effects of chloroform were discovered, and nearly proved the first operation in which it was tried. The second case, a patient of Dr. Pattison, had an abscess high up in the neck, requiring simple puncture for its evacuation. He died without hemorrhage, or admission of air, or other apparent cause, a minute or two after the puncture was made. If chloroform had been used in these cases, would it not by some have been blamed for the result?

Twelve or fourteen months ago, Professor Syme was performing primary amputation of the thigh in the hospital, upon a patient upon whom there was no sign of reaction, and who was not etherized for the operation. "Upon the incisions being made, relaxation of the sphincters took place, the contents of the rectum and bladder were voided, and an effort at vomiting seemed the prelude of immediate dissolution. Before tying," says he, "the arteries, I waited to ascertain whether the condition of the patient depended upon syncope or death. My colleague, Dr. Duncan, by causing alternate pressure and relaxation of the chest, effected artificial respiration for some time without any sign of returning life; but by and by the actions of the system were gradually restored, and maintained through the use of stimulants."—See Monthly Journal of Medical Science, vol. 1847–48, p. 76. Such dangerous symptoms, coming on in an anæsthetized patient, might have been mistaken for the effects of the anæsthesia.

Some time ago, before either ether or chloroform were used in surgery, Dr. John Argyll Robertson was called, a few miles out of Edinburgh, to perform the operation for strangulated hernia. After having shaved the groin for this purpose, his patient complained of sickness and faintness, and died before any incisions were made.

Last year, Dr. Girdwood of Falkirk, came to Edinburgh to see the practice of anæsthesia, in order to be able to apply it in a case of amputation. The day for the operation and anæsthesia was fixed; but, some hours previously, sudden apoplexy came on, and the patient died.

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[1] In one case of placenta prævia to which I was called, the mother had lost much blood, and her lips were pale, and her pulse very weak. On administering chloroform, the circulation and pulse rallied; I separated the placenta, no bleeding recurred; and several hours afterwards the child was born. The mother made an excellent recovery.

CHAPTER VII.

MODE OF EXHIBITING CHLOROFORM; DOSE, ETC.[1]

In the course of the preceding observations I have omitted making any remarks on the degree of artificial anæsthesia required in obstetric practice, with the exception of stating that when instrumental or operative interference is adopted, the anæsthetic state must be made adequately deep—so deep, that the patient must be rendered quite passive and apathetic. In fact, when induced for operative purposes in midwifery, the anæsthetic state should be as complete and profound as when it is induced for operative purposes in surgery. But, in common cases of parturition, the anæsthetic agent employed, whether chloroform or ether, does not, in general, require to be given in such large doses as in surgical practice. And in obstetric practice, the rules which I have usually followed in exhibiting the chloroform (the only agent I believe now used in Edinburgh and most other places), are those which I briefly stated when first writing on the subject for the Monthly Journal in November last. "After the first full dose, a few inhalations, before or with each returning uterine contraction, are generally sufficient. The state of anæsthesia should be made more deep as the head is passing the perineum and vulva."[2] I have elsewhere in the same journal stated these rules at somewhat greater length.[3]

Occasionally I have at first, and especially in the early stages of labor, given the chloroform in small doses only, so as to obtund or obliterate the sensations of pain, without altogether abrogating the state of consciousness. In many patients, this degree of anæsthesia, with the results stated, viz., the loss, in a great measure, of pain without the entire loss of consciousness, can be readily enough induced, and answers excellently well; but, as a general rule, it has appeared to me in some cases objectionable. For not unfrequently small doses, such as produce this condition, are accompanied with excitement and talking; and sometimes patients have complained to me of this renewal of the chloroform in small doses with each pain, being accompanied each time with a renewal of the ringing in the ears, flashes of light, and other disagreeable sensations accompanying, in some persons, the primary effects of the inhalation. Besides, we are never thus sure that we are really saving the patient to the full extent by the means we are using. If, on the other hand, she happen to be thrown at once into a deeper state of anæsthesia, the chances of such inconveniences and drawbacks are avoided. Often, when the anæsthetic state is thus made deep from the first, the uterine contractions are arrested for a few minutes, but speedily return. In order to effect this, we take care that as soon as the patient is asleep—(and, in natural labor, we seldom or never require to push the inhalation so far as to affect the respiration, and produce noisy inspiration, and snoring, as in surgery)—the chloroform should be withdrawn, and not reapplied again till the movements of the patient, or the state of the uterus, as felt through the abdominal walls, indicate a returning uterine contraction. A few inhalations given then, and repeated with each returning uterine contraction, keeps the patient in a state of unconsciousness; and this condition may be easily maintained for hours, by administering in this way the chloroform vapor with each pain, and withdrawing it entirely during each interval. The practice is not to be expected to come upon medical men by intuition; for, like all other practices, some care and experience is necessary in order fully to acquire and apply it. And the *two* main difficulties which every beginner meets with are these: namely, to keep the patient in a state unconscious of pain, and yet not so deeply anæsthetized as to have the uterine action interrupted. For too deep a state of anæsthesia in general interferes with the force and frequency of the uterine contractions; while a lesser degree of the anæsthetic state leaves these contractions unaffected; and a still smaller dose often excites and increases them—the effects, in this respect, of chloroform upon the uterus, being similar to the effects of opium in different doses. But the influence of the inhaled agent passes off in a few minutes, differing in this respect from the more permanent influence of a drug when swallowed; and if, at any time, the anæsthetic effect is too deep, and the uterine action is in consequence impeded, all that is necessary is to abstain entirely from exhibiting the chloroform for a short time, till the parturient contractions have been allowed to come back to their proper degree of strength and frequency; and then the anæsthetic agency is to be sustained as before, by giving the vapor with every recurring pain, but in smaller doses, or for a shorter time during each pain, than was previously practised.

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[1] From Edinburgh Monthly Journal of Medical Science, October, 1848, p. 220.

[2] Monthly Journal, vol. for 1847–48, p. 417.

[3] Ibid. No. for April, p. 762.

Anæsthetic vapors, when given in large doses, have less power of reining up the action of the uterus in the last than in the first part of labor. And as the sensations of pain become more agonizing as the head is distending the perineum, and passing through the vulva, the anæsthetic state usually requires to be then rendered more deep and complete than in the early stages of the process; and in most patients this may be done without at all impeding the rapidity of the delivery. Indeed, in many women, this latter part of the process of parturition seems to be accelerated by the superinduction of anæsthesia; for the degree of relaxation of the muscular structures of the perineum and vaginal orifice, commonly resulting from it, usually more than compensates for any diminution of uterine action that may occur. If in any instance it proves otherwise, and the depth of the anæsthetic state interferes too much with the parturient contractions, the simple remedy is the one I have already mentioned —a diminution in the state of anæsthesia, so as to allow a return and increase of the expulsive efforts of the uterus.

The degree and depth of anæsthesia which different patients are capable of bearing without the irritability and contractions of the uterus being impeded, appears to differ greatly in different persons. In some, a very deep state will still leave the uterus almost or altogether unaffected; in others, its action is interrupted by a comparatively slight degree of the anæsthetic state. It is this variability which at first forms the principal difficulty to those commencing the use of chloroform in obstetric practice. But experience and care will soon enable any attentive observer to overcome this apparent obstacle, and to adapt the dose of the agent to the powers and capabilities of each different patient. I have never yet seen an instance, but I can conceive it possible, that in some rare exceptional cases and idiosyncrasies, the action of the ether or chloroform should, even in such small doses as merely produce unconsciousness to pain, interfere, especially in the first stage of labor, too much with the muscular action of the uterus, and require to be given up, at least till the labor be more advanced. But this would, of course, be no reason for not employing it in those other persons in whom it had no such influence; any more than because opium occasionally does not act as an hypnotic on particular patients, it should not be given with that indication to any other patients with the view of inducing sleep.

During the anæsthetic sleep which chloroform induces in natural labor, the patient usually lies perfectly quiet and passive in the intervals between the pains, but moves more or less, and sometimes moans, as each uterine contraction begins to return. In the last stage she generally, with every recurring uterine contraction, makes the usual violent bearing-down muscular efforts, and the struggle can often be marked in the expressions of her face. The muscular action of the uterus and assistant muscles goes on, and yet she remains quite unconscious. The strictest quietude should always be observed and enforced around the patient, for noises and speaking, particularly soon after the chloroform is commenced, will sometimes excite and make her talk; and, if this happen, we may require to exhibit to her a deeper dose than would otherwise be at all necessary. One or two practitioners of midwifery in London have averred and repeated, over and over again, in our medical journals, and in pamphlets intended for non-medical readers, that obstetric patients, under the influence of chloroform, must be liable to talk and act grossly and obscenely. This objection to the practice of anæsthesia in midwifery has been repeated and gloated over by those who have propounded it, in a way which forms, apparently unconsciously on their own part, the severest self-inflicted censure upon the sensuality of their own thoughts. An impure mind, more especially in a professional man, may easily fancy and find impurities where none whatever exist; but he is not on that account entitled to imagine that his own lewd thoughts are typified in the thoughts or actions of his patients. In answer to the supposed objection itself, I have merely to observe, that I never once witnessed any trace of indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren, of whom I have inquired on the subject here, is to the same effect. In a paper on temporary delirium occurring in the course of labor, Dr. Montgomery several years ago described more marked instances of effects of this description, arising merely from "the extreme distress and pain," to which the mother was subjected in the dilatation of the os uteri, &c., during natural parturition, than were ever seen to arise from the influence of means used to abate and abrogate that "extreme distress and pain."

In administering chloroform in obstetric practice, I have always used the handkerchief, as the simplest and best apparatus. Sometimes, when the case is likely to be tedious, I have it folded and sewed into the form of a deep cup or cone. The chloroform is poured into the bottom of the cup, the open end of it held over the nose and mouth of the patient when the action of the vapor is required; and, when its application is suspended, by closing the open end of the cone, the escape and loss of the vapor is prevented during the intervals. Such an arrangement saves the chloroform. But a handkerchief merely folded together and sprinkled with chloroform, answers quite well; and in the intervals it may be compressed together in the hand, so as to prevent the escape of the chloroform. In *first* throwing the patient over into the anæsthetic sleep—(the point which requires the most management)—a handkerchief, thus presenting a large surface is often much more serviceable than one folded into a cup shape; for the patient, when first coming under the influence of the chloroform, is apt to move her head from side to side; and, in order to keep up the constant inspiration of the vapor, she can be more easily followed by using a simple handkerchief, than by trying to keep any kind of apparatus applied to her mouth or face.

The quantity of chloroform used varies both according to the duration of the labor, and the susceptibility of the patient. Usually, when the handkerchief is used, about an ounce an hour is necessary—a small quantity being poured upon it from time to time. A less dose will suffice in some, and others require more. In one case lately, where the patient, in a first labor, was anæsthetized for two hours, I expended nearly six ounces, large doses being necessary to keep her in a sufficiently deep state of unconsciousness. The first quantity which I pour on, usually amounts to three or four drachms; but I always judge by the *effects*, not by measuring the dose; and I pour on an additional quantity in a minute or so, if it be required. In holding the handkerchief towards the patient, I take care that plenty of atmospheric air is admitted, and seldom or never put it in contact with the face. At first, it is better to hold it at a considerable distance, in order to prevent any chance of irritation and coughing; and then gradually approach it. It is always to be remembered, that the vapor of chloroform is nearly four times the specific gravity of atmospheric air; and if the patient is lying on her side, the handkerchief or pillow can be easily arranged so as to keep a larger supply of this heavy vapor opposite the mouth and nostrils. I have always held and managed the handkerchief myself in the first instance, and till the patient was asleep. Afterwards, I have generally trusted it to the husband or nurse, teaching them to apply it near the face when the pains supervened, and to fold up the handkerchief, in the way mentioned, so as to preserve the chloroform during the intervals.

When exhibiting chloroform in obstetric practice, and in the way I have described, I have often been struck by the circumstance that its use is very rarely followed by sickness or vomiting. I do not remember having seen vomiting follow its exhibition during labor in more than four or five cases, and two of these, in the practice of Dr. Paterson and Dr. Cochrane, were instances in which I was called in to apply the forceps, and where the patients were placed for the operation in a state of anæsthesia as deep as that used in surgery. I have repeatedly seen it arrest the sickness and vomiting occasionally accompanying the first stage of labor.

In addition, let me state that I have usually begun the employment of the chloroform when the os uteri was well dilated, or towards the termination of the first and the commencement of the second stage of the labor. But when the pains were severe I have commenced it earlier, and when the os uteri was still comparatively little dilated. There is, I believe, no limit as to the date of the labor at which we may give it.

CHAPTER VIII.

REPORT OF THE RESULTS OF ANÆSTHESIA FROM DIFFERENT OBSTETRIC HOSPITALS AND PRACTITIONERS.[1]

THE following account of the results of anæsthesia in the practice of the Maternity Hospital, Edinburgh, has been drawn up by Dr. Duncan and Mr. Norris, two gentlemen who have acted as resident house-surgeons in the institution, and upon whose power and accuracy of observation all who are acquainted with them will place implicit reliance:

"Since the use of anæsthesia in labor became general in the Maternity Hospital, shortly after the discovery of chloroform, 95 women in all have been delivered in the house under its influence. Among these, 88 were natural and 7 were morbid labors. In the 88 cases of natural delivery, only one of the mothers died, convulsions coming on five hours after delivery, and proving fatal after a continuance of six days. On a post-mortem examination, the kidneys were found to have undergone, in some parts, the true stearoid degeneration.[2] Among the same 88 cases of natural labor, there were 5 dead-born children. In two of these cases, the birth was premature, being at the sixth month. In the third case, the mother had previously given birth to two dead infants. The fourth dead child had a very large hydrocephalic head. The proportion of still-born children was thus 1 in 17. In the Dublin Hospital the proportion of still-born children, as reported some years ago by Dr. Collins, was 1 in 15. The 7 instrumental cases were as follows;—one application of the short forceps, in an unsuccessful attempt to save the life of the child; the mother recovered well;—two cases in which the long forceps were applied; one of the women, in whom the head was very long impacted in the pelvic brim, died from sloughing of the maternal passages; the other made a good recovery; both the children were born alive;—four cases of version; one of the mothers died from rupture of the uterus, the others recovered quickly; three of the children were still-born, and in one of these three cases the cord was prolapsed.

"In addition to these 88 cases of anæsthetic delivery, there have been upwards of fifty women delivered in the house without chloroform. These have been chiefly very rapid labors, where the women have come into the hospital just in time to give birth to their infants, or where the house-surgeon has not been able to see them till very shortly before delivery. From the expense attending a large consumption of chloroform, it has always been an object to husband it as much as possible;[1] and therefore, in the hospital, it has not been given in cases where the mothers did not very severely complain of their sufferings, nor were harassed with feelings of anxiety and fear.

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[1] From the Edinburgh Monthly Journal of Medical Science, Oct. 1848, p. 225.

[2] See a report of the case, Monthly Journal for September, 1848, p. 196.

"On the whole, the results of anæsthetic midwifery, as observed by us in the hospital, have been perfectly satisfactory; and we can confidently state that the recoveries have been altogether more perfect and speedy than before. This has been remarked in so great a proportion of the cases, that there can be no doubt whatever of the truth of the observation. Besides the increased rapidity of recovery, we have noticed the almost entire absence of those uncomfortable feelings of fatigue, languor, and shivering, and of that shattered feeling which so frequently comes upon the mother immediately after an ordinary delivery. Instead of this, we have found the mother almost invariably awake from the anæsthetic sleep comparatively fresh, easy, and cheerful. Not unfrequently the anæsthetic has been found to change, without an intermission, into a natural sleep, which may continue for an hour or two.

"Further, there have been, since the introduction of chloroform into the practice of the hospital, far fewer than formerly of those violent attacks of rigors, ephemeral fevers or weeds, and abdominal pains, which are so common in most crowded hospitals, forming a class of cases which used formerly to cause much anxiety, and was a common cause of the mother's being detained in the hospital after the usual fortnight allowed for recovery. In fact, since using chloroform, there have been scarcely any women detained in the house by these causes, and much less Dover's powder, calomel and opium, abdominal fomentations, &c., have been used.

"The women have been, invariably, found deeply grateful for the relief to their sufferings afforded by the anæsthetic influence of chloroform. Yours, &c.

"J. M. DUNCAN. H. NORRIS."

At a meeting of the Edinburgh Medico-Chirurgical Society in June last, along with other practitioners, I gave in a report on the employment of chloroform in midwifery. At that time, and since, I have been favored with written statements of the results, by various medical friends in Edinburgh, and in different parts of the ountry. I shall now give extracts from a variety of the letters hich I have received, relative to this subject. Many more such ommunications might easily have been called up and adduced; ut I have deemed it useless to multiply unnecessarily this kind of idence. It will be observed that, with one exception (see the mmunication of Mr. Lansdowne), the following letters refer, like e preceding statements regarding the Maternity Hospital, to the e of chloroform alone.

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[1] Perhaps, in a short time, a benevolent government will allow chloroform to be made cheaper, by removing the very high duty on proof spirit when used for medical and chemical purposes (tinctures, &c.). At present that high duty is, in one respect, a direct tax upon the relief of human disease, and the mitigation of human pain; and a great obstacle to the progress of British organic chemistry.

The first statement which I give is from my assistant and friend, r. Keith.

"I have employed chloroform in every case of labor under my re since its introduction, with one exception; and also in almost ery case to which I have been called in by other practitioners. my own cases, amounting to about four-and-twenty, it has been ven for a period varying from half an hour to eight hours. The antity of chloroform consumed has been, on an average, about one nce per hour; in a few cases double this quantity was found quisite. The anæsthesia has been in almost every case complete; at is, the patient on awakening has declared that, while under the fluence of the chloroform, she was utterly unconscious of all pain. n most cases the patient has lain quiet even during the pains, the resence of which is then generally indicated by the breathing ecoming more rapid and somewhat labored. In other cases, there suppressed moaning during the pains, or even, in some, loud anifestations of powerful straining and muscular exertion. I can tate most positively that I have seen no serious symptom which ould be traced to the chloroform, in any one case, either as affecting the mother or the child. Most of the mothers have made ncommonly good recoveries. Those who have had children previously, have, almost without exception, stated to me, that they felt ery decidedly stronger after delivery than on former occasions. n two cases the recovery was rather slow; but this was owing to he patients having been in a very delicate state during pregnancy —and in both instances, I considered the chloroform was of very great service, by saving their strength. All the mothers are now in heir usual health.

"In no one of the twenty-four cases was the child still-born. In ne case, labor was brought on at the end of the seventh month, wing to the brim of the pelvis being much contracted. The child as born alive, but died on the second day. All the other children re now alive. They have all been nursed by their own mothers, ith one exception.

"I have had occasion to use the forceps seven times since the troduction of chloroform, and once to break up the child's head and extract by the crotchet. In all these cases the patient w put into a deep anæsthetic state, and in most she lay perfect and apathetic during the operation. All the mothers hav well, except in the case of craniotomy, where the uter ruptured previous to the use of chloroform. Yours, &c.

"G. S. Ke

From Dr. Moir, Edinburgh.

"Since the beginning of December, I have, with a ve exceptions, used chloroform in the course of my midwifery p and I have not met with a single case where any unpleasant either to mother or child, can be traced to its use.

"As far as my observation has gone, I think it will be that, in some cases, the chloroform, if freely administere early period of the first stage, retards the pains a little, others also lessens their power; and when this does occur, remedy is either to intermit its use till the labor is further ad or to give it in smaller quantities and at longer intervals, so at that stage, to induce complete unconsciousness. But, correct or not in this opinion, I am quite satisfied that the stage is much accelerated, especially towards its terminat the chloroform doing away with the resistance offered to the sion of the head by the muscles at the outlet of the pelv this to such an extent that, in some *first* cases, there is a risk very great care is taken, that the perineum be slightly la from the head being so rapidly expelled as not to give tim parts to yield so rapidly as they would otherwise do. But comparatively a very rare occurrence, and requires to be me principally with the view of putting young practitioners o guard against it, and of leading them to use the necessary to prevent it.

"In exhibiting so powerful an agent as chloroform, I th point of importance to use as small a quantity as is compati the obtaining of its full anæsthetic effects; and as this s depend much upon the rapidity with which it is conveyed i system, it seems a point worthy of consideration to ascert readiest means of so doing. Various instruments hav invented for this purpose, though they have been almost sally superseded by the use of the handkerchief, as recom by you, and used either in the form of a hollow cone, applie and again, after renewing the chloroform, over the nose and of the patient; or simply folded up several times, and fre having interposed between the folds a piece of wool or f with the view of better retaining the chloroform. In both methods there is a considerable loss of chloroform, much of it being retained in the folds of the handkerchief, and much of it escaping without passing into the lungs, or else passing into them so slowly as not to produce the desired effect. I have, for some time, been in the habit of using a linen or white cotton handkerchief, folded only once, or, if very thin, folded twice; the point requiring attention being, that it should not be so thick as to offer any impediment whatever to free respiration when applied over the mouth and nostrils. Since using the chloroform in this way, I have never failed in rapidly producing the anæsthesia, either in my own practice, or when accompanying some of my patients, who were several months advanced in pregnancy, to their dentists to have one or more teeth extracted. For administering the vapor to patients who are in the erect position, the chloroform should be poured on that part of the handkerchief placed on the palm of the operator; the edge of the little finger should then be applied close to the chin, and the hand gradually raised up towards the mouth, till the sensation of choking which generally accompanies the first inspirations has passed off, after which, the handkerchief should be left on the face, and the hand removed; the patient then breathes freely through that part of the handkerchief wetted with the chloroform, and, in general, half a drachm is sufficient to produce anæsthesia.

"In obstetric practice, I find it the most convenient plan to place one end of the folded handkerchief under the left cheek of the patient, to pour a little chloroform on it, and then, taking hold of the loose end of the handkerchief, to bring it gradually near the mouth, till it can be left there without inconvenience, the patient breathing freely through it. And it is not necessary again to remove the handkerchief, but simply to pour on it occasionally a very few drops, whenever the patient begins to show symptoms of returning consciousness, or on the accession of a pain. The only precautions necessary are, to raise a small fold of the handkerchief from the skin when the chloroform is to be applied, so as not to blister the skin; and to drop the chloroform, not on that part of the handkerchief immediately over the mouth, but a little above it, so that the vapor, being heavy, may flow down towards the mouth or nostrils, and thus be, during inspiration, more readily received into the lungs. By adopting this plan, I have had the handkerchief applied for nearly two hours without removing it; and the quantity of chloroform I have used in single patients, has been much less than other practitioners have used in cases of the same duration. Yours, &c.,

"JOHN MOIR."

FROM DR. MALCOLM, EDINBURGH.

"Since November last, I have employed chloroform in
thirty cases of labor, and with the most satisfactory and deli
results. A majority of these were first labors. I have ke
patients under it for periods varying from half an hour to six
and have never found the slightest unpleasant effects result fr
use. All the children have been born alive, and are at this m
in perfect health, with the exception of one that died when a
month old, of a sudden and severe attack of dysentery. A
mothers have made recoveries with rapidity and completen
above the average which I had previously observed in my pr
This has struck me as the more remarkable, seeing a large p
tion of my patients were primiparous; and I can only attribu
result to the entire absence of suffering and shock to the n
system which is effected by the use of chloroform. Althou
few cases my patients and their friends have at first objected
use of anæsthesia to abolish pains which they considered 'na
yet every one has afterwards expressed to me sincere gratitu
saving them from their agonies; and I am sure not one wh
experienced the beneficial effects of the practice will ever sub
these agonies again, now that they know that they are so
unnecessary, and can be so easily and safely abolished.
repeatedly found the mothers of my patients object to anæs
as if they grudged that their daughters should not experien
same sufferings as themselves—but I have uniformly found
afterwards as grateful as their daughters for the relief admini

"Generally, I have employed about an ounce of chlorofor
hour. I have never seen the uterine contractions arrested by
although I have no doubt a large dose would, when necessary
that effect. I have seen no case of hemorrhage, or convulsio
any other complication whatever. Let me add, that I cannot co
on what principle the employment of chloroform in natural
should be objected to, as long as it is our duty, and assuredl
our duty, as physicians, to relieve and mitigate human suffer

"Yours, &c.,
"R. B. MALCO

FROM DR. THOMSON, EDINBURGH.

After stating the details of ten cases, Dr. T. remarks:

"Among the middling classes I have met with more diff
in using the chloroform than I had anticipated, as it has only
when the sufferings of the patient were very severe, or her fr

ad begun to dread the effects of prolonged continuous suffering
n her constitution, that I could get my wishes carried into effect.
hat fatal Newcastle case, which was trumpeted a good deal in the
ewspapers, is still haunting their minds, and is very frequently
rged by some timid friend when you propose the chloroform to
lieve the sufferings of the patient.

"My experience of it has been, in all obstetric instances, analogous
your own; with one exception, I have had no difficulty in getting
e patient under its influence; a minute or two was in general suf-
cient to lay the most restless or ungovernable patient quiet on her
llow.

"Not the slightest *post partum* hemorrhage has taken place in my
n cases, though in the two forceps cases, where it was given deeply,
e uterus remained flabby for nearly thirty or forty minutes, and
rew off the placenta with difficulty.

"Its relaxing effects are, I think, undeniable. In one case, the
ft parts had resisted for a considerable time the descent of the
ad; they yielded very readily within an hour after the chloroform
as begun. I have not had another instance of this kind lately; but
ere I to meet with one, I feel confident it would yield with much
ore facility under the chloroform than without it. Yours, &c.,

"ALEX. THOMSON."

FROM MR. CARMICHAEL, EDINBURGH.

"I have given the chloroform in twenty-six cases of midwifery, four
which were first labors; the others varied from the second to the
eventh pregnancy. The quantity given varied from two drachms to
ur oz., and the length of time during which it was exhibited from
few minutes to four hours. The preparation I have used has always
een that of Messrs. Duncan, Flockhart and Co., and I have never
en the slightest bad effects from it, either in midwifery or other
edical cases, or in any cases where I have administered it for
musement, except occasional sickness where it was exhibited
hortly after a meal.

"I have met with no case of flooding whatever. I have heard it
lleged that it drives away the milk; but I have not found it so, as
y patients have all been able to nurse, with the exception of one lady,
ho has not been able to suckle her child for the last three times.

"In all of these cases it was administered with the greatest ease
d with perfect success, and, in no case, with any bad results.

"The recoveries have been certainly more than usually speedy.
deed the only objection I have met with as to its use, has been on
e part of the monthly nurses, who seem afraid that the new prac-
ce will curtail their attendance and pay.

"All the children were born alive, and are doing well. The only case in which the child proved the least refractory was a footling one.

"I have also found it most useful in cases of dysmenorrhœa, in spasmodic colic, and tic douloureux.

"I also gave it in a case which you saw along with me, of most severe neuralgia of the uterus, with the most perfect success.

"In no case whatever have I seen any bad results of any kind arise from the use of the chloroform. Yours, &c.,

"W. S. Carmichael."

From Dr. Burn, Edinburgh.

"I regret that I cannot give you the number of cases of labor in which I have exhibited the chloroform, but I may state that I have given it repeatedly, and have not seen any bad consequences either to the mother or child result from its use.

"All the mothers made rapid recoveries, and the children did not appear to suffer from its use.

"I have given the chloroform in three or four cases of adherent placenta, where the uterus was firmly contracted, and had far less difficulty in extracting it than I have experienced in similar cases where the chloroform was not exhibited. Yours, &c.,

"J. Burn."

From Dr. Purdie, Edinburgh.

"I have now used chloroform in seventeen cases, which I have noted, and in every instance with decided effect, not merely by lessening suffering, but I am perfectly convinced, by the most careful observation, by shortening the duration of labor. The pains have never in my experience been interfered with except by rendering them quicker, and far more effectual.

"There is one of the cases which I would wish to recall to your memory. The patient, thirty-eight years of age, was in her first labor, which commenced early on Wednesday morning, and went on well but slowly till the evening, when its progress ceased, although the pains continued regular and strong. The os uteri was well dilated, but the head made no progress, although there was no very evident cause. About one o'clock on Thursday morning I sent for you to deliver her with forceps. On your arrival, you thought that still there was hope of the labor being terminated naturally. The patient, who was suffering much, was then put under the influence of chloroform and ergot, while you waited patiently for any advancement, for nearly two hours, without effect. You then delivered with the long forceps, which cost you great rtion, from the head being impacted in the brim. The patient's ition was changed, the placenta was extracted, she was bound up l laid in a comfortable and easy posture, in which state she con- ued to sleep soundly, until she was awakened after the child was ssed, the crying of which surprised her, as she had not been scious of what had taken place from the time she got the first dose the chloroform after your arrival. This patient had an excellent overy. I never saw a patient suffer less after labor, or recover re rapidly. I may just add, that there are few things vex me re with regard to patients, than to witness the sufferings of a ldbed patient, who will not allow, from ignorance or prejudice, use of chloroform. Happily, however, such cases are very rare ong us.

"Yours, &c.,
"W. PURDIE."

FROM DR. FINLAY, NEWHAVEN, NEAR EDINBURGH.

"I have used chloroform in a considerable number of cases of tural labor. It was with much reluctance that I first administered and only at the urgent entreaty of a patient, who was enduring ense agony before the birth of a first child. It was completely ccessful. Her screams had been audible across the street. In a minutes they ceased, and she fell asleep, while the uterus con- ued to act as powerfully as before. She was not aware that she d got her baby, until a quarter of an hour after it was born. In e of the other cases the influence of the chloroform was as com- te. In the other three cases *entire* insensibility was not induced, t the sufferings were greatly mitigated, and the relief was so evident the patients, that, whenever the labor-pain was approaching, they asped with great eagerness the handkerchief on which the drug s sprinkled. Satisfactory as these cases have been, I have hitherto ed the chloroform with considerable hesitation and caution, and ly when it was asked for by the patient. But every trial has em- ldened me to employ it with greater confidence on future occasions. each of my cases the placenta was soon and easily detached; in ne of them did hemorrhage occur; and they all made excellent coveries.

"Yours, &c.,
"A. FINLAY."

FROM DR. CUMMING, EDINBURGH.

"I have now attended thirty-five cases of labor under chloroform, d it has been used in all with marked advantage. All the tients have made unusually good recoveries; and I have been very much impressed by the fact, which was remarked by the first patient submitted by me to chloroform, and repeated by all, that the convalescence was not accompanied by the crushed and dislocated feeling that they have experienced without it.

"I always begin by introducing the chloroform slowly and gradually into the lungs, allowing a large proportion of air to be inhaled along with it. In every instance it was administered, not with reference to the quantity given, but to the effect produced—this effect being complete unconsciousness during the pain; and thus administered, I have never seen any unpleasant or absurd consequences, nor anything to excite alarm or even uneasiness.

"Two of the cases had had large floodings in a previous labor; with the chloroform there was none. This, of course, I do not impute to the medicine; but it at least tends to prove, that hemorrhage is neither a necessary nor a likely consequence, as many at first were disposed to imagine.

"All the children were born alive, and are so still. None of them as yet give the slightest indications of idiocy, either present or future; nor have I observed in any the temporary stupefaction immediately after birth, ascribed to the presence of chloroform in the apartment, that some have remarked.

"I am quite satisfied that, if properly given, it acts as a calmative; and I believe, from what has passed under my observation, that very many of what are called exceptional cases are not so in reality, but appear to be such from error in the mode of administration, and that further experience will amply demonstrate the truth of this.

"In short, I am, unfortunately for the appearance of veracity, compelled to say, that all my cases hitherto have been so successful, the recoveries so uniformly good, and the satisfaction on the part of the patient (I may add also my own) so great, that I am rapidly approaching to, if indeed I have not already arrived at, the conviction, that, *if there be any sin connected with chloroform, it is chargeable on those who refuse to administer it.*

"I may add, that not one of those patients who have already inhaled it will ever be denied it in any subsequent pregnancy, as they have repeatedly assured me; and certainly I shall not attempt to keep it from them, and that not more for their sake than my own.

"Yours, &c.,
"W. Cumming."

If necessary, I might have adduced more evidence in favor of the anæsthetic effects of chloroform in midwifery practice, from Dr.

Beilby, Dr. Zeigler, Dr. Weir, Dr. Young, Dr. Menzies, Dr. Gilchrist, Dr. Campbell, and other medical practitioners in Edinburgh, who have been using it.

In order to vary this kind of evidence, I shall next adduce extracts from various communications which I have received on the use of chloroform in midwifery, from medical correspondents in different parts of Scotland, England, and Ireland. It is needless, I believe, to attempt to arrange them in any special order; and I shall content myself, therefore, with beginning with the letters of those practitioners who live farthest north, and proceed southward.

The following is an extract from a letter written to me this summer:

From Dr. Grigor of Nairn.

"Dr. Allan of Forres and myself would as soon think of going to an obstetric case without our chloroform phial, as we would of going to bleed a patient without a lancet. In this quarter, doctors are only called in when things are going wrong, or in extreme cases; so that, since your grand discovery, he and I have only used it in about twenty-four cases, in all which it came up to all you have written about it, no still-born children, mothers recovering well, fewer after-pains, &c. &c. One of my cases was a first child, the mother nearly forty-eight years of age, weakly in constitution, and of small formation. Had it not been for the chloroform, I do think she would have sunk. "Yours, &c.,

"J. Grigor."

Dr. Dyce, lecturer on midwifery in Marischal College, Aberdeen, favored me some time ago with the following interesting communication regarding the obstetric employment of chloroform:

"I have reports from my friends Drs. Harvey, Pirrie, and Gilchrist, all of whom, I am aware, had been employing it. I may at once state that I consider it a most invaluable agent; that I have every confidence in its safety; that I recommend it almost on every occasion; that no evil consequences have ever attended its use; and that I have found its effects nearly alike in all. I have used it eleven times. The labors have, with one exception, been natural. The exception was a breech case, and a first child. Two of the eleven children were lost; one was putrid, and in the other (the breech case), though the funis did pulsate for some minutes, the child could not be recovered.

"Dr. Harvey has given me a brief account of four cases delivered under chloroform; all the children were born alive.

"Dr. Gilchrist at Woodside writes to me—'I have observed no evil results to the infants themselves from the use of chloroform.' He does not state the number of his cases; but I presume they have been numerous from his remarking, that 'latterly I have not used this agent so generally as when it was novel, reserving it now, unless when urgently requested by the patient, for cases unusually painful, whether arising from excessive sensibility of the system, rigidity of the soft structures, or cases requiring manual assistance.'

"Dr. Pirrie has employed chloroform in fourteen cases. Ten of these were natural, two instrumental, and one a case of turning. He says, 'as to the children, they have all been born alive, and continue to go on satisfactorily.'

"I never use it early in labor; generally the second stage has come on, or at least the os uteri is tolerably well dilated. This appears to be the practice of my brethren here. I then keep the patient in a state of insensibility more or less complete during the future progress of the labor. Occasionally I have allowed them to come completely out of the anæsthetic state; but, on the recurrence of the pain, the patients invariably and urgently seek for the handkerchief. When I used chloroform, I imagined that the intenseness of the stupor induced did lessen the frequency and force of the pains, and even put a stop to the labor altogether; and I am still of opinion, that if the full effect is produced and kept up for any time, the pains will cease; but if a more moderate effect is produced, so that consciousness to a certain extent remains, my conviction is, that instead of checking uterine action, its use enables the organ to act with more freedom; the mental influences are quickened; and, more than this, it has a decided power in relaxing the soft parts, and thus removing a very frequent cause of protracted labor. I had a very interesting case, illustrative of these remarks, in January last, in a lady in her fifth pregnancy, of a very irritable, anxious, and highly nervous temperament, who had, previously to labor, determined to use this wonderful agent. Her former labors had always been tedious, and very painful throughout every stage. This commenced, and had continued for a couple of hours before I saw her. She was complaining much of the severity of the pains, especially of her inability to move from one posture. Her anxiety and agitation were very considerable; her pulse was above 100, and her body was already wet with perspiration. On examination I found the parts rigid, and the os not larger than a shilling, while the rectum actually encroached upon the vagina from its loaded state. I determined, therefore, on emptying the gut by an enema before employing the chloroform, and mentioned this to my patient, and gave the nurse the necessary instructions. I very soon found that I had added not a little to her already excited state, and she was not prepared for delay, and would only, after some persuasion, consent to its exhibition, and only then on condition that immediately after she was to be allowed the 'stuff.' Her request was complied with, and I confess to you its effect on her general state, from the moment she was under its influence, not only gratified myself, but pleased her friends, who complained much of her impatience and her imagined evils. A few inhalations sufficed to induce insensibility, before which she gave one or two hysterical laughs, and made some incoherent remark. She then became still and quiet, until a pain returned—the only indication of which was a gentle writhing of the body. Her pulse very soon fell to 80, at which it remained; her skin became cool; the parts rapidly relaxed; she moved, and allowed herself to be moved in any direction readily; the pains were certainly more regular and efficient than in any of her former labors; and in two hours and a half from the first inhalation, the child was born. Once only during this period, and that was after one hour had elapsed, was she perfectly sensible. This was permitted at the solicitation of the nurse and friends, who had not seen chloroform used before, and could not be convinced that so sudden a change could exist and the labor progress perfectly. They soon had evidence of this, by the patient screaming out in her former impatient tone, 'Pain, pain! where is the handkerchief?' From this time until delivery she was perfectly unconscious. The placenta came away naturally in ten minutes. The roller was put on, some of the soiled things were changed, and it was not for several minutes ere she was convinced that her trials were over. I need hardly say that she was gratified in the highest degree to find that she had escaped so much of her former sufferings, and expressed her deep thankfulness for so valuable a boon. The child was putrid. Her recovery was perfect, and more rapid than on any former occasion.

"In my practice I have met with no instance of chloroform failing in producing its usual effects in a very few minutes, where it was willingly and readily inhaled; in some nervous patients, a few seconds are sufficient for this purpose. In the case I have just given, I was surprised at the few inhalations she took; and Dr. Pirrie mentions that one of his patients was so highly susceptible of its effects, that, on making three or four inhalations, the state of anæsthesia became so complete, that the labor was entirely suspended; he therefore, in this case, discontinued its use. On some occasions, I had difficulty in persuading the patients to use it; hence a longer time elapsed. In one case, where persuasion failed, I watched an interval of drowsiness, and, in spite of herself, brought the woman fairly under its influence, in which state she remained for twenty minutes, when the child was born unconsciously. Its cry aroused her, she turned herself hastily around with an inquiring look, and after a momentary gaze said, 'What is that? you have been giving me the stuff.'

"The quantity of liquid used has varied in my hands from ℥ss to ℥iiss. This last quantity was expended in the case of a young woman, eighteen years of age, pregnant of her first child, and who was more or less under the influence of chloroform for five hours. As this was amongst my first cases, if not the very first, and the longest period in which I have employed this agent, perhaps you will excuse my giving it somewhat in detail, more especially as ergot was given along with it, which I am not aware that I had seen anywhere recommended. Jane ——, æt. eighteen, unmarried; first child, December 18, 1847. When seen at five P.M., the membranes were reported to have ruptured twelve or thirteen hours before; on examination, the os uteri was found dilated to the size of half a crown, and the breech was discovered as the presenting part; the pains were frequent, irregular, at times only effectual, but they had been increasingly severe during the after part of the day, and now were strong.

"About seven P.M., after some persuasion, she first inhaled the chloroform. She soon became insensible; it was then only presented to her at intervals as a pain recurred, a single inhalation being generally sufficient. She laughed and sang for a few minutes, but at length seemed perfectly unconscious, and we were only sensible that she had pain by her now and then rolling about, as is the case when labor-pains continue under convulsions. For an hour she was kept in this state; but thinking that the pains were becoming more distant and the progress of the labor slower, she was allowed to come out of this torpid state. Soon after ʒss of powdered ergot was given, and repeated four times at intervals of a quarter of an hour. The pains now became stronger and more frequent, and my patient, who was conscious of the powerful agency of the chloroform, and the advantages she had derived, insisted on its repetition. She had, in fact, secreted the handkerchief, and was now keeping it at her mouth. But alas! its talismanic powers had vanished; she threw it rather angrily at the student, who, at that moment, sat by her bedside, desiring him to give her the bottle in its stead; she assured us that she had felt no pain until now, that she had been dreaming of her illness, which she believed was finished. From this time (nine o'clock), until her delivery, which took place between one and two o'clock the following morning, she was kept in a perfectly unconscious state, at times more so than at others, according to the severity of the pains. Towards the end of the labor, the throes were very severe, but during none of them did the patient complain of the slightest pain. The only indication, as I have already said, of apparent suffering, was a movement of the body, and occasionally a low moan. The child in this case did not breathe or cry; it was not therefore, legally in life, though the funis pulsated for some minutes.

"I can scarcely think that the death ought to be attributed to the chloroform, or even to the ergot. The length of the labor, the nature of the presentation, and especially its being a first child, were almost against its being born alive. So that, with this exception, no injury has befallen the infant in the practice of any of us here who have employed chloroform.

"I have only further to say, that the recoveries in every case have been most satisfactory. They appeared even more rapid than under ordinary circumstances. "Yours, &c.,

"R. DYCE."

FROM MR. LAWRENCE, MONTROSE.

"I am sorry I cannot furnish much information as to the employment of chloroform in midwifery in this quarter. It has been very little used by myself or brethren, owing chiefly to a very general prejudice on the part of our parturient patients against it. The very last case I attended, the patient resolutely refused it, although suffering very severe pain! However, in one of those in which I administered it, and to which I had gone unprovided with the chloroform, my patient compelled me to send for it! In one case in which a patient of mine had convulsions in her first labor, I exhibited chloroform at her second confinement with the usual success.

"I have seen no injurious consequences of any kind. Yours, &c.,

"SAMUEL LAWRENCE."

FROM DR. STEELE, MONTROSE.

"I have used chloroform in midwifery practice on six separate occasions. In two of the cases, it was given to the extent of producing only a partial immunity from suffering; but with the effect of converting, especially in one to whom it was administered for four hours, what would have been a very severe into a very easy labor. In the other four, the effect was highly satisfactory; two of the children being born without the consciousness on the part of the mothers, for some time after, of that event having taken place. One lady remarked, that she thought 'the march of intellect had never taken a happier direction than when it led to the discovery of chloroform.'

"All the children did well. One of them, however, was asphyxiated for nearly a quarter of an hour after its birth.

"From the little experience I have had of chloroform, any opinion of mine as to its merits is not entitled to much weight. It seemed certainly in several of my cases to prolong the interval between the pains, and thereby retard the termination of the labor; and I thought also that the uterine action was sometimes less effective when the patient was under the power of the medicine. Yours, &c.,

"GEORGE STEELE."

FROM DR. PATON, DUNDEE.

"I have ascertained that chloroform has been employed in upwards of fifty cases up to this date (5th June, 1848), several of them instrumental, and with the best effect; and no bad consequence has attended it, either to the mothers or children. All the children have been born alive. In the cases in my own practice in which I have used it, it speedily induced the anæsthetic state, and appeared to accelerate the uterine action in some. The recoveries of the mothers were more favorable than in former instances where no such agent was employed. In all, the children were born alive, except one, where the child was *acephalous*.

"To explain the small number of cases in which it has been used here, I ought to mention that, in consequence of the deaths of two ladies in a respectable rank of life, from puerperal fever, when it was epidemic a few months ago—in both of which cases chloroform was used—the public of course attributed the unfortunate result to the new agent; and since then it has been difficult, and in many instances impossible, to overcome the prejudice against it. Of late, however, from the favorable opinion entertained of it by those who have had it administered, it is not so frequently objected to.

"Yours, &c.,
"GEO. PATON."

Dr. Anderson, President of the Medico-Chirurgical Society of Glasgow, and one of the most experienced and esteemed physicians in that city, some time ago wrote me a letter, from which I make the following extract:

"Not being connected with any obstetric institution, and having now little practice of this kind amongst the poor, I am sorry I cannot offer you any sufficient statistics on the use of chloroform; but I have had so much reason to be satisfied with its effect, that I almost always use it, and most of my patients have expressed themselves as strongly in its favor as you have done. I recollect only two exceptions, where, although the usual effects were produced, both patients complained that, previous to the full anæsthetic effects, their feelings, instead of being agreeable, were very much the reverse.

One lady said that she felt it like approaching insanity; and that she would rather endure the labor-pains than be subjected to this again. In no case have I had reason to suppose that mother or child suffered injury. All the mothers have done well, and several of them have spontaneously remarked that they made better and quicker recoveries than after former accouchements. In this I coincide. Two children were still-born. One, in the sixth month, died during parturition, after long-continued discharge of the liquor amnii for many weeks before the induction of labor; the other, in the eighth month, after hemorrhage from fatigue, followed by protracted labor; in this case a placental clot was found after delivery, so that the child's death was sufficiently accounted for.

"On the whole, my experience of chloroform in parturition is more favorable than in other cases, where I have several times seen it produce alarming spasmodic and other nervous symptoms, followed by sickness. These effects, I think, are most apt to occur in subjects who have had spinal irritation, or an excitable state of the nervous system, or who use the chloroform when exhausted, or after abstinence from nourishing food.

"Yours, &c.,
"A. D. ANDERSON."

The following communication was sent me in June last—

BY MR. SPENCER, DOUGLAS, ISLE OF MAN.

"I forgot how often I had used the chloroform when I last wrote you; but, since then, I have used it frequently, with invariable success so far as it is concerned. There have been no still-born children from its use, and all the children have continued to thrive well; none as yet have had a fit from teething—this I consider a great matter. We want some powerful proof here to bring it into general use. I cannot use it in half my cases, as I would not do it without leave from the patient. I had a case of puerperal mania the other day, and did *not* use it in it. If I had used it, I should have been blamed. This is so small a place that everything one does is at once known. I have often operated under its use, with no ill effect.

"Yours, &c.,
"J. H. F. SPENCER."

FROM MR. CEELY, AYLESBURY.

"I have much pleasure in communicating to you the general results of my very limited use of anæsthetic agents in midwifery. I have used both ether and chloroform in numerous operations, small and great, in public and private practice, have been several times the subject of their influence, and have repeatedly employed them in hysteria, epilepsy, and other nervous and spasmodic affections; but my employment of them in midwifery has been more limited, and at present restricted chiefly to primiparous and difficult or tedious cases. I have used them in only six cases as yet.

"It has happened that most of my late obstetric cases have been so easy and so rapid as to supersede the use of chloroform. My own observations of its effects in midwifery, would induce me to *urge* its use in all *severe*, *protracted*, or *operative* midwifery; and in any case of natural labor, with only the ordinary suffering, I would not hesitate to employ it, with the concurrence of the patient or friends, if I saw no contra-indication; because I believe that, in proper doses, with suitable management, it is harmless, and may be beneficial; but if the patient, under these circumstances, were averse, I should not, of course, press it. Neither do I think I should venture to employ it in natural and easy labor, where I suspected or knew that subsequent hemorrhage would arise, from flabbiness of fibre and flaccidity of the organ; because I think the *secale cornutum*, during the last hour or two of such labors, has in my hands been so beneficial, and seems more appropriate.

"In all the cases in which I have employed ether or chloroform, the patients and their friends have been perfectly satisfied and gratified with the results, and abundantly thankful for the boon. In only one case has there been any sickness or syncope. In a town about twelve miles hence, it has also been successfully used by Mr. Knight of Brill, in several cases. In Bicester, I have heard of its use, but chiefly by younger medical men.

"I will only add that, in using the chloroform, I have, in some of the cases, now and then omitted to repeat the dose, to show the friends and attendants how differently the patient was situated; but the latter has soon removed any hesitation of the friends by calling out hastily for that 'nice chloroform' again.

"Yours, &c.,
"ROBERT CEELY."

In a printed pamphlet[1] obligingly furnished me some time ago by Mr. Stallard, surgeon to the Leicester General Dispensary, that gentleman observes:

"I have exhibited the chloroform in upwards of thirty cases of midwifery during the present year, and they have included a greater mber of severe cases than the general experience would support; is is accounted for by the fact that two of the worst were pauper tients, to whom the surgeon's attendance is required only in cases unusual severity. One other formidable case also occurred to y father, and in it I was requested by him to use the chloroform. this experience I have never once observed the least retardation the parturient paroxysms, *so long as absolute insensibility was not duced;* and in several cases the pains were palpably increased. hen a patient is suffering acute pain, she does all in her power to ppress it, and the act of doing so is well known to retard the pro- ss of parturition; when, therefore, the pain is no longer felt, the fort to suppress it no longer ensues, and the accouchement is celerated in a remarkable degree. But again, it has been argued at the pain of labor is desirable; and that it is wrong to interfere ith this natural indication of what is going on. Now, with this sertion I am totally at variance: there is nothing so depressing to he powers of life; nothing so soon exhausts the already feeble ody; nothing so effectually prevents the sufferer from aiding ture by her volitional effort, as pain; and consequently nothing o powerfully maintains the strength, or invigorates the system, as s annihilation. * * * In the thirty cases I have attended, I have ot had a single case of flooding, and two individuals had never een free from it on former occasions. This is, in my opinion, an ditional recommendation for its exhibition; and in all those citable persons in whom flooding is most likely to occur, the minution of fear and excitement caused by its exhibition has a rect tendency to prevent the flooding. Nothing but a very tended experience can justify any conclusion upon this head; far as my own experience goes, I am decidedly of opinion that loroform exerts no perceptible influence upon the child."

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[1] Practical Observations on the Administration and Effects of Chloroform in Natural Labor, p. 16, &c.

From Dr. Protheroe Smith, London.

"I have records in my own practice and that of my friends, of owards of 125 cases of anæsthetic labor; and, with one exception, l have done well. In several thus treated, no hemorrhage has sued, though in previous labors there was flooding. In nearly l, the getting up has been more speedy, requiring no aid of piates and purgatives; and it is my sincere conviction that chloro- rm lessens the chance of puerperal inflammation and fever. I sually employ my inhaler to administer it, and with it I fancy I n administer it with greater success and precision. I have kept atients under its influence from half an hour to twenty-eight and half hours. I have used it in cases in which bronchitis was pre-

primipara, æt. twenty (administered in the seco
under its influence four and a half hours, slept swee
movement of the body, save during the last expulsi
the usual abdominal muscles were called into actio
under the chloroform became more frequent and
fine male child was born. After the expulsion o
patient awoke refreshed as from a sweet sleep, with
evidence (save the advent of her first-born) of havin
as far as her own feelings were concerned. The u
firmly, and I instantly left her for the second—a lad
required to inhale for a minute and a half (in the th
the induction of anæsthesia was complete, when s
talkative, addressing her maid in French, speak
wholly foreign to her condition, and of an amusi
though she was perfectly at ease and had nothing
labor, which advanced rapidly, and terminated in a
first exhibition of chloroform. On recovery, in abo
after, she was introduced to a fine boy, and remark
sound asleep. The third case had the chlorofor
stage; she slept soundly in half a minute, and th
scious of what was going on—described to the nurs
the progress of the labor, and observed that s
delighted that she had her senses, and yet was wh
of any pain or distress of any kind. She remarke
entire freedom from suffering, she was sensible of
the labor, as though it was effected by an agency

FROM DR. RIGBY, LONDON.

f course I use the chloroform in midwifery, but I cannot think large dose at first is the best way. In almost all private ts, you can draw the line between loss of pain and loss of ousness. I give but a small quantity at a time; the patient it herself (on a strip of sponge), and when it is exhausted she or more. "Yours, &c.,

"E. RIGBY."

FROM MR. LANSDOWNE, BRISTOL.

have now used ether or chloroform in seventy-one midwifery I have two modes of administering it, the one with a bladder, ich is placed a brass pipe with a stopcock, and into this is ed, after I have poured the chloroform into the bladder, a of elastic tubing with a mouthpiece, the whole being pierced a bore ⅜ of an inch, through which the vapor can be readily d. If I find I am likely to be giving the chloroform for a time, I use the apparatus, both for the sake of convenience, lso of economy, as ʒj, will last me nearly or quite an hour this; and, should I use it many hours, it not only effects a saving of material, but does not so frequently require replg, and is always ready at the approach of each separate uterine ; and it may (as has been the case with me) be used by any ls, or by the nurse, should the practitioner require to be absent short time. The other apparatus is an inhaler, such as is comy sold; it is made of a thin and pliable lead, adapted over the and mouth, having a piece of perforated zinc in its front, and ining a piece of sponge, over which the chloroform is thrown: epth of this inhaler is such as to prevent the nose being touched e chloroform. It is home-manufactured, not expensive, and easy of construction. This latter I make use of if I am likely wanting it for a short time only; it requires to be supplied every five or ten minutes, and, accordingly, I use ʒj or ʒss, latter is my quantity when about to extract a tooth. If the of the uterus causes great pain, as is frequently the case in rly period of the labor with the first child, I commence giving oon as the os uteri is sufficiently dilated for the head to pass; e given it when the opening has not exceeded the size of half wn. I believe it may be given with impunity as early in the as we please, and the only obstacles to its being so used that see, are the inconvenience to the medical attendant in being occupied with one patient for such a length of time, and also the very great expense which such a lengthened use of it must entail. On the patient's account, I can see no possible reason why it may not be used for a whole day, or even more; indeed, I cannot see why a limit should be set to the length of time in which it may be used. I have no doubt but that it will soon be the anodyne generally used at the latter stages of painful cancerous diseases. The greatest length of time in which I have used it has been 16½ hours, a fresh inhalation being made at every renewal of the action of the uterus; in other cases I have given it 11½ and 12 hours, and the only reason of the inhalation being limited to this time, has been the cessation of the necessity for its use, namely, that the child has been born, otherwise it would have been continued until such event had taken place.

"I have found that nearly all my patients have recovered very rapidly; most of those who have had children previously, have been astonished at the unusual rapidity of their recovery.

"I find no difference as to the expulsion of the placenta and the subsequent discharge, when administering chloroform, to what takes place in the usual natural labor. I have, upon two occasions, used it for very severe after pains—pains so severe that their cries could be heard at a considerable distance; indeed they appeared worse than the pains of actual labor; in both cases the pain was completely subdued by its use. Both these persons had determined not to avail themselves of the benefit of the chloroform during labor, neither did they, but they were delighted afterwards with its soothing effects. The former of these I had long resolved to give it to for this express purpose; it was her thirteenth child; her labors have always been very rapid, scarcely any pain accompanying them; but no sooner has the child been born than her agony has been almost past bearing, the pain recurring at intervals for a fortnight. Upon this occasion I gave it to her three times within the first ten hours, and she had nothing to complain of afterwards.

"As regards sickness, I have not found that symptom, except where fluid has been previously taken; on the contrary, if the patient has been sick, the chloroform has almost invariably checked it. The cramp I have not heard them suffer from whilst under its influence. I have never yet met with anything which has caused me to regret having used it. "Yours, &c.,

"J. G. Lansdowne."

During the course of last winter and spring, the practice of anæsthesia in midwifery was tried in London by a very intelligent young Russian physician, Dr. Haartman of Helsingfors, while, for the sake of information, he was living as house-surgeon in the great

Lying-in Hospital at Westminster. Dr. Haartmam subsequently visited Edinburgh, and kindly drew up for me, before leaving England, the following account of the results of his practice. I have much pleasure in publishing his letter, both on account of its own intrinsic value, and because the results of the practice of anæsthesia in this Hospital have been in no small degree misrepresented, both publicly and privately.

RESULTS OF ANÆSTHESIA IN THE WESTMINSTER LYING-IN HOSPITAL.[1]

"I fulfil with the greatest pleasure your wish, in writing down my observations on chloroform as used in midwifery. I do it certainly not without much embarrassment, the greater the more I think of my ignorance of the English idiom; but encouraged by your indulgence, I do not hesitate to begin the task, more especially as I know that some erroneous ideas of the use of the chloroform in the General Lying-in Hospital in London have been spread about.

"During the three months I was house-surgeon in that hospital, I had the opportunity of observing 105 women in labor, of whom about 25 were attended by my friend Dr. Delafield of New York, the other 80 by myself. But having been most of the time the only house-surgeon, and otherwise much engaged, I was not able to exhibit the chloroform oftener than in 25 cases, which, I need scarcely say, were in no way selected. The most of these patients, were, however, *primiparæ*. In the administration of the chloroform I followed your method, using simply a towel, upon the interior of which the liquid was diffused; but I held it as a rule to exhibit it in the beginning of each case rather in small doses, and with caution, watching, in the meantime, the pulse, as the surest indicator of any danger. But when the patient got accustomed to it, I gave it freely, not removing the towel till the patient was in deep sleep. In this state, the respiration was in general sonorous, sometimes stertorous; the pupils were usually somewhat contracted, sometimes, however, dilated, sometimes not at all affected. The pulse was, in the beginning, either a little accelerated, or quite natural, I think, when the patient was not frightened. Yet, I have seen the pulse, without any bad consequence, go down to 40, when the chloroform was used for a long time or in large doses. Its strength was very seldom altered, being then weaker. The patient usually lay quiet, all the muscles being relaxed till the uterine contractions came on, during which they all, with few exceptions, pressed down as in common labor. Some of them complained, although they afterward said, 'they felt no pain whatever.' Few talked or laughed, except the Irishwomen, whom I found rather refractory to the anæsthetic influence, probably because they are, in general, too much accustomed to drinking spirits. All the cases were, with few exceptions, observed with the greatest care, and both the duration, the frequency, and the nature of each uterine contraction, both before and after the chloroform was exhibited, were marked down. Thus, I find, by comparing all my annotations, arranged in tables, that, during the full effect of the chloroform, the uterine contractions became less frequent, and, I should say, less powerful, but, when the effect had passed off a little, they then became more frequent and shortened in duration; for example, if the interval between two uterine contractions before the use of the chloroform was 6 minutes, and the duration of each 1½ minutes, both these periods were reduced after its exhibition. The interval, from 6 to 5, 4, or 2 minutes, and the duration of each pain from 1½ to 1 or ½ minute. Besides, I think, that in this state the uterine contractions in general came on more suddenly than in common labor, a circumstance which, however, may depend on an illusion, by the absence of pain existing and announcing the action of the uterus in the usual labor. I have never observed the uterine contractions quite arrested, yet often somewhat delayed. The supposed relaxation of the soft parts seems to me at least uncertain and accidental. In two cases in which I thought the chloroform had very much relaxed the rather rigid parts, I had, notwithstanding, rupture of the perineum. Among the 25 cases of chloroform, I had only in one a slight hemorrhage—a fact the more remarkable, as at the epoch at which I used chloroform most frequently, I can say I had hemorrhage, and that often very severe, in almost all the other cases in which chloroform was *not* used.

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[1] From Edinburgh Monthly Journal of Medical Science, October, 1848, p. 225.

"After the delivery, I found the patient in general quite unconscious of any occurrence during the anæsthetic state, some of them saying nevertheless, that they had felt something going on, although they did not know what. They seldom complained of headache, and it usually disappeared during the following day. The mothers recovered all speedily and perfectly, with one exception, worthy your attention, although I am fully convinced that the accident by no means was the result of the chloroform, of which only two drachms were used—my provision at that time not being larger. During the use of the chloroform this patient was, as usual, insensible and quiet; but for want of chloroform, the effect could not be kept up for a long time, and she was delivered with the usual pains. She was the following day perfectly well, and continued so till the eighth day after the delivery—she had then complained to the other patients of headache. I was, however, not called for, before she, in the afternoon, had had a fit of what is commonly called apoplexia nervosa, after which she got paralytic on the right side. I need not give you a tedious description of the proceeding and the treatment of the disease; allow me only to state that the patient began, by the use of strengthening medicine, to walk about in a month.[1]

"Of all the 105 children, six were either still-born, or died sooner or later after the birth—two only of them belonging to the twenty-five cases of chloroform. But, before I relate these cases, I beg permission to point out some general remarks. In the majority of the cases I could not discover any change whatever upon the child, some of the children being rather bloodfull, others anæmic—states depending, I think, as usual, on the duration and nature of the labor. In one case, however, in which Dr. Fergusson was present, the newly-born infant was found rather strange, or, to use the eminent doctor's expression, 'tipsy.' It breathed less frequently and more abruptly than usual, the sound of its scream being rather singular. It recovered, nevertheless, perfectly. I must here observe that the mother (Irish), who, after the confinement, had brought up a large quantity of gin and broom, confessed having taken spirits before admittance into the hospital. Of the two children who died, the first was rather weak and thin, but continued pretty well till the seventh day, when it got peritonitis, and died on the tenth day after birth. The second, a large and fat child, died four hours after birth; n this case, when the head was born, I observed around the neck mall blue spots, which I found to be blood extravasated under the kin. After birth the same kind of spots began to appear over nearly the whole of the body. The child breathed well in the beginning, but would not suck. It died without convulsions. By the post-mortem examination, the spots were found to be blood extravaated from the capillary vessels of the skin; a large quantity of half oagulated blood was found in the cavity of the peritoneum, and mall blood coagula in both lateral ventricles of the brain. The ungs and the brain were slightly congested. The blood seemed to be quite natural, and had no anormal smell.[2]

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[1] The "puerperal paralysis" was in this, as it is in most other cases, probably connected with albuminuria. One of my patients who was confined for the second time four months ince was attacked two years ago with hemiplegia immediately after the birth of her first hild. I have a patient at present under my care, from Forres, who, several years ago, was attacked with hemiplegia at the time of delivery.

[2] This special case of purpura has been often mentioned. Every one acquainted with nfantile pathology knows that purpura is not a rare occurrence at birth, as the cases and observations of Andral, Billard, Otto, Lobstein, and Graetzer, &c., amply prove. A patient who came some time ago from London, to be confined in Edinburgh, under my care, was gravely assured by a distinguished London physician, that if chloroformed, her child would be sure to be, as they all were in the Westminster Hospital, "either dead-born, or with their

"Before I finish, I beg to add that I have used the chloroform in a case of turning, with the greatest advantage. I was called out from the hospital to a poor woman in labor, and found a presentation of the cord, and the uterus so contracted that I could nowhere introduce my hand, although I made repeated attempts, without success. I then gave a large quantity of chloroform, and I was quite astonished at the great change which took place, it being now uncommonly easy to perform the turning. "Yours, &c.,

"CHARLES DE HAARTMAN."

Of the use of chloroform in midwifery in Ireland, I have few details. From Dublin, my friend Dr. Tyler wrote me, in June last, that the practice has not yet been much tried. He observes:

"As to the progress of chloroform in obstetric practice here, I regret to state that its virtues have not yet been fairly put to the test by any of our Dublin accoucheurs, owing to a dread of bad consequences resulting, although I am unaware of any case followed by such being brought home to it, except a rumored one of *threatened* convulsions and spasms, where —— was administering it.

"As to the surgeons, I hear them all speak in the highest terms of it. The resident surgeon of Stevens' Hospital, Mr. Wilmot, told me yesterday that he administers it previously to every operation there, now in some hundred cases, without meeting with any untoward result. "Yours, &c.,

"A. TYLER."

Subsequently Dr. Tyler informed me that Mr. Shekleton, master of the great Dublin Lying-in Hospital, was trying the effects of chloroform. While this sheet was passing through the press, Dr. Denham, assistant physician in that hospital, visited Edinburgh, and informed me that Mr. Shekleton has now exhibited chloroform in upwards of forty cases of labor, most of them of an operative or instrumental kind, and that the effects have been such as to induce him to go on with the trial of it. No unfavorable results, either as regards the mothers or children, have been observed to follow the use of chloroform. Some of the principal private practitioners in Dublin, are also, Dr. Denham informs me, beginning to employ the practice.

Regarding the use of chloroform in midwifery on the Continent of Europe, I possess little or no information. Dr. Krieger of Berlin tells me that in that city most medical men are opposed to the

blood in a putrid and dissolved state!" The want of the head in the anencephalic child born at Dundee (see Dr. Paton's letter, p. 600), might have been as logically ascribed to the mother's use of chloroform, as the purpura in the above instance.

practice of anæsthetic midwifery, but still, five or six accoucheurs constantly make use of it. Writing to me in July last, Dr. Krieger says:

"About five or six accoucheurs of this place, I don't think there will be more of them, use chloroform in almost every case they attend; many more only in cases of morbid labor; the majority not at all. The cause of this curious occurrence—*curious*, because in *every* surgical operation chloroform is made use of—may be found, not only in the disinclination of the public at large to such extraordinary means in quite a natural process (as they take labor for), but perhaps also in the expense, chloroform being still a costly article. I don't know whether the preparation we get here is less strong than it ought to be, or not, but I seldom require less than six drachms or an ounce, sometimes more, for one delivery, and the price is as much as half-a-crown per ounce at apothecaries' shops. Professor Martin of Jena has used ether in seven, chloroform in ten cases of morbid labor, and cannot strongly enough recommend the latter anæsthetic agent, stating that he never witnessed but favorable consequences. I am sorry I cannot give you any more details about the progress chloroform has made in Germany—but the perplexing political affairs have so bad an influence on scientific publications, that we get but very little medical news from anywhere on the Continent, and those only such as were long prepared before we fell victims to revolutions. "Yours, &c.,

"E. KRIEGER."

In a letter which I have lately received from Vienna, the writer, Dr. Arneth, informs me that the use of anæsthetic agents in midwifery is, as a general rule, discountenanced in that city by the two leading professors of midwifery in the University, but that, in several late cases, they have used it in operative delivery. He further states, that in Würzburg, the use of chloroform in obstetric practice is more common. He states—

"While I am writing this letter, Professor Kiwisch, of Würzburg, tells me, that in that place no woman is confined, either in private or in hospital practice, without having been chloroformed. In two cases of eclampsy, he saw decided effect of these vapors, viz., the fits subsided. "Yours, &c.,

"F. H. ARNETH."

The preceding kind of evidence, in relation to the practice of anæsthesia in midwifery, in this and other countries, might, as I have already stated, be very easily increased and multiplied by a little more extended inquiry, and by an appeal to the experience of the numerous accoucheurs here and elsewhere that have employed it. But the evidence as it stands—and spontaneous as it is in most cases—is amply sufficient to show both the great extent to which the new practice has already been adopted, and the great success that has attended it. Every innovation in medicine which implies, like the present, a violent and extensive change in existing doctrines and old-established practices, has always been, for a length of time after its introduction, stoutly decried and resisted. The history of the first introduction and subsequent progress of the three greatest modern improvements in practical surgery, midwifery, and medicine—viz., the ligature of arteries, the induction of premature labor, and the discovery of vaccination—afford sad but strong historical proof of this observation; and we have many minor instances of the same constant enmity to change, in the bitter opposition which the first employment of antimony, ipecacuanha, cinchona bark,[1] and other medicines encountered. And I believe that I am correct in stating, that probably no innovation, embodying so very direct and decided a deviation from all the former routine and rules of practice, as the employment of anæsthesia in midwifery implies, ever, in the same short period, made such extensive way and progress as it has done among the profession. As a matter of course, however, it has called forth also abundance of published and unpublished opposition and objection. No small share of resistance against it has taken the form of personal or professional abuse of me as the introducer of the practice. All that I most willingly pass over and excuse, as, judging from all past experience in medicine, it was nothing more nor less than I was entitled to expect under the circumstances. But some difficulties and objections of a more palpable nature have been urged against the practice; and, in conclusion, I will very briefly allude to, and attempt to answer, the more prominent among these that happen to have been brought forward.

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[1] The London physicians have, on several occasions, specially distinguished themselves by their determined and prejudiced opposition to all innovations in practice not originating among themselves. In the whole Pharmacopœia, there is perhaps no one remedy which, at the present day, is acknowledged to be of greater value, or to have saved more human lives, than cinchona and its preparations. In the seventeenth century, the proper time and manner of using the cinchona bark, for the cure of the then prevalent intermittent fevers of England, was made out by Robert Talbor, a medical practitioner in Essex. When Talbor subsequently removed to London, and began to use with success the new remedy, in the cure of the common agues of the metropolis, he found that, as he gained in the favor of the world, he lost that of the physicians of London; and apparently their persecution of him became such, that the King at last was obliged to interfere, and in the year 1678, King Charles II. sent a royal mandate to the College of Physicians, commanding the President, Dr. Micklethwait, "and the rest of the College of Physicians," not to give Talbor "molestation or disturbance in his practice." Among the list of London physicians averse to the new practice of curing ague by cinchona bark, De Bergen mentions the illustrious names of Sydenham, Harvey, &c. In 1698, a Dutch physician, Dr. Groenvelt, published a work entitled, "De tuto cantharidis in medicina usu interno." A few years previously, viz., in 1693, when Groenvelt practised in London, the President of the College of Physicians imprisoned him in Newgate for daring to recommend and use the new remedy whose virtues he had discovered. Six or seven years after vaccination began to be generally used throughout England, Dr. Moseley, a member of the London College of Physicians, suggested to his College the propriety of putting down "the beastly new disease," as it was termed, of cow-pox; and in 1805, he boasted that the middle and inferior classes of London had then "renounced the delusion." In the last number of a respectable London medical journal, a London medical practitioner questions whether the practice of relieving women, by anæsthetics, from the pains and agonies of parturition, should not "be considered criminal according to law."—See London Medical Gazette for Sept. 8, p. 424.

CHAPTER IX.

ALLEGED DIFFICULTIES IN THE SUPERINDUCTION OF ANÆSTHESIA.[1]

In a previous page, I have stated the principal circumstances which require to be attended to in the exhibition of chloroform in labor. I have been occasionally told of cases, in which it was supposed that it was impossible to produce the anæsthetic effect of this agent. In my own practice (and I have now used chloroform for many hundred persons), I never yet met with a single instance in which a person was proof against its full influence.

It has been sometimes averred, in the English journals, that on attempting to use chloroform, jactitation, incoherent talking and delirium, spasms, &c., &c., have supervened, *instead* of a state of quiet anæsthesia. These are symptoms which do occasionally come on in the *first* or exciting stage of its action, more especially if strict quietude is not enjoined; and, though they are apt to terrify the beginner, they are in reality no more serious in their effects and character, than some of the equally frightful symptoms sometimes seen in hysteria. They are an evidence, however, of one of two things,—either that the vapor is being given too slowly, or that it is given in too small quantity—in an exciting, in fact, instead of a soporific dose; and the simple remedy, as every one properly experienced in its action knows, is at once to increase the dose, in order to pass the patient as speedily as possible into the *second* or full narcotic stage.

Chloroform, it is alleged, sometimes gives rise to much coughing, and pulmonary irritation. Certainly not so, if the chloroform is of good quality, and its vapor is not at first approached in too strong and concentrated a form to the face of the patient. After some experience, it will be found that it can be given so as seldom or never to induce even coughing. Some time ago, a well-known physician, in a large city of the South of England, wrote me, that he and his townsmen had found it too dangerously irritant a substance to breathe, and that he had seen it produce cough, bronchitis, phthisis, &c. The answer was simple: it never produced any such effects in Edinburgh practice. And I believe that the explanation was equally simple: he and his townsmen had experimented with an imperfect and impure article. A few days ago one of the principal druggists in Edinburgh showed Dr. Christison, Dr. Douglas Maclagan, and myself, a bottle of chloroform of high specific gravity, viz., 1·490, which he had just received from a very large manufacturing chemical house in London. It was impossible to breathe it without feeling great irritation in the throat and chest. It emitted fumes that at once reddened litmus paper; and which, on examination, proved to be muriatic acid. Is it wonderful that bronchitis, coughs, and more serious disasters, should have followed the inhalation of such an improper and dangerous article?

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[1] From Edinburgh Monthly Journal of Medical Science, Oct., 1848, p. 244.

Dr. Letheby has shown, that some kinds of chloroform in the market, besides containing muriatic acid, are also mixed with aldehyde, hydrochloric ether, hypochlorous acid, &c.[1]

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[1] See Medical Gazette for June 16, 1848, p. 1038. The presence of some of these deleterious agents has been supposed to be an inevitable and speedy effect of the spontaneous decomposition of very pure chloroform. But I find that some of the article, manufactured several months ago, in its purest form, by Duncan, Flockhart, and Company, of this city, has undergone no kind of change, even though long exposed to the sun. Messrs. Smith have also shown me the same, in regard to their chloroform. I have reason to know, that the dangerous article alluded to in the text as containing a quantity of muriatic acid, has been extensively sold to the profession, throughout Scotland and England, at a price two or three shillings per pound cheaper than is charged for the pure chloroform manufactured by other houses; and, probably, its very cheapness has led to its extensive use. The following is the formula by which chloroform is prepared by Messrs. Duncan, Flockhart, and Company, of Edinburgh, whose article I have always found of the most superior quality: 4 pounds of chloride of lime, and 12 pounds of water, are first well mixed together, and then 12 ounces of spirit added. Heat is then applied to the still (which ought not to be more than a third full), but as soon as the upper part of the still becomes warm, the heat is withdrawn, and the action allowed to go on of itself. In a short time the distillation commences, and whenever it begins to go on slowly, the heat is again applied. The fluid which passes over separates into two layers, the lower of which is chloroform. This, after having been separated from the weak spirit forming the upper layer, is mixed with half its measure of strong sulphuric acid, added gradually. The mixture, when cool, is poured into a leaden retort, and distilled from as much carbonate of baryta by weight, as there is of sulphuric acid by measure. The product should be allowed to stand over quicklime for a day or two, and repeatedly shaken, and then redistilled from the lime. The specific gravity of the resulting chloroform is generally 1·496 or 1·497.

CHAPTER X.

OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.[1]

OBJECTIONS of various kinds, religious, moral, and medical, have een zealously brought against the practice of anæsthesia in midrifery.

Elsewhere[2] I have attempted to answer the supposed religious bjections that were at first so very strongly urged in various uarters against the practice, on the supposed ground of the perianence of the primeval curse; and I have shown that the disputed 'ord "sorrow," *'etzebh* ("in sorrow thou shalt bring forth children"), oes not in the original Hebrew really signify the sensations of ain, but the muscular efforts and contractions connected with ildbirth. Besides, if this were not the fact, and it was the duty man to give effect to the curse, instead of struggling to ameliote and resist its penalties and influences, then the whole art of ysic should require to be abandoned entirely, for, in the primeval rse, man was doomed to die; and yet is not the great leading m and object of the physician a continuous attempt to preserve m in life? All forms of obstetric assistance would require also be rejected, for the whole art and science of midwifery is one ndivided effort to abate and ameliorate the effects of the curse; nd to attain that object imperfectly, as heretofore, by venesection, aths, by counter-pressure to the back, and other minor practices, as sinful as to attain it more perfectly now by anæsthetics, inasuch as the principle of interference is not altered by the degree of elief given; "for whosoever shall keep the whole law, and yet ffend in one point, he is guilty of all." In short, if there is any vidence of feelings of impiety and irreligion in the whole question, is surely on the side of those persons who suppose that pain is ermanently ordained in the primal curse as an accompaniment of uman parturition; and yet that by anæsthetics, man, the creature, as discovered a power by which he can alter and subvert an immuable decree of God, the Creator.

The principal moral "objection," as it has been termed, against he employment of anæsthesia in midwifery, amounts to the oftenrepeated allegation, that it is "unnatural." "Parturition," it is avowed, is a "natural function," the pain attendant upon it is a

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[1] From Edinburgh Monthly Journal of Medical Science, October, 1848, p. 246.

[2] See p. 548 of this vol.; see also a pamphlet by Dr. Protheroe Smith, entitled, "Scriptural Authority for the Mitigation of the Pains of Labor."

"physiological pain"—(Dr. Meigs);[1] and it is argued that it is impossible "to intermeddle with a natural function;" and that to use anæsthetics is a piece of "unnecessary interference with the providentially arranged process of healthy labor"—(Dr. Ashwell).[2] The above is, perhaps, the most general and approved of all the objections entertained and urged at this moment against the practice of anæsthesia in midwifery. But it certainly is a very untenable objection; for, if it were urged against any of our similar interferences with the other physiological functions of the body, every one of which is as "providentially arranged" as the function of parturition, then the present state of society would require to be altogether changed and revolutionized. For the fact is, that almost all the habits and practices of civilized life are as "unnatural," and as direct interferences with our various "providentially arranged" functions, as the exhibition of anæsthetics during labor. Progression upon our own two lower extremities is a "providentially arranged" function, a "natural process;" and yet we "unnaturally" supplement and assist it by constantly riding on horseback and in carriages, &c. The "physiological process" of walking is apt to produce pain and injury of the uncovered foot of man, and we "unnaturally" use boots and shoes to bind the foot, and add to the protecting power of the cutaneous and other structures of the sole. Mastication and digestion are "natural processes;" but we daily intermeddle with and attempt to aid them by the arts of cookery and dietetics; and so on with regard to other functions.

To annul the pain of labor by anæsthetics, is, argues Dr. Meigs, "a questionable attempt to abrogate one of the general conditions of man." Riding and railway travelling abrogate one of the general conditions of man (progression), and are constantly leading to accidents and deaths. Should we never travel therefore except on foot? Disease and death itself form one of the most "general conditions of man,"—and medicine is a "questionable attempt to abrogate them." Should medicine therefore be abandoned?

In a note now lying before me, an eminent London divine urges the following objections against anæsthesia, either in midwifery or surgery; and I notice it here, because it is an objection which I have often heard repeated. He writes: "The question with me is not the alleviation of pain, but the destruction of consciousness. I should hesitate greatly to take a step which destroys consciousness." Now, certainly, our consciousness is destroyed in natural sleep as much as in the anæsthetic sleep. I have little doubt that the distinguished writer whom I have quoted, has, many a day, perhaps during almost every day for a long lifetime, voluntarily given up and destroyed his own consciousness in sleep, for an hour or two longer each morning than the necessities of his system required. Putting these many hours together he has, perhaps now, from first to last, unnecessarily, but voluntarily, surrendered up his mental consciousness for periods, that, if added together, would count up weeks, and months, and perhaps years. He has done so too, merely for the reprehensible indulgence of indolence; and yet he insists upon his fellow-creatures not surrendering up their *consciousness* for a short time, on rare and extraordinary occasions, when the object is the far more legitimate one of the avoidance of unnecessary physical pains, and the securing life and health by saving the system from the endurance of these pains. If we may sleep, and thus indulge in the destruction of consciousness to avoid and cure corporeal fatigue, surely we may do the same to avoid and cure corporeal agony.

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[1] Philadelphia Medical Examiner, March, 1848, p. 152.

[2] Lancet for March 11, 1848, p. 291.

Dr. Merriman[1] opposes the employment of anæsthesia in natural labor, on the ground of "the great superiority of allowing nature to conduct the whole process of the birth." But the practice of anæsthesia does, in reality, "allow nature to conduct the *whole* process of the birth;" it merely abstracts that intensity of pain and suffering which accompanies the act of labor in the civilized woman—a "disadvantage, inseparable" from civilization, to employ Dr. Merriman's own expression, and which is not an essential part of the process of parturition, according to his own doctrine; for as he himself states, "in the earliest ages of the world, and in savage nations at present, childbirth appears to have been, in almost every instance, *easily* accomplished; the mother suffers *little*." And in this state of natural anæsthesia, the convalescence of the mother is consequently unusually rapid; for, again to quote Dr. Merriman's words, she almost "at once resumes her ordinary occupations." Dr. Merriman afterwards, in speaking of the use of chloroform, decries its propriety in any except "instrumental or very tedious labors,"—arguing that we should not interfere unless where the labor is morbid, for (to use his own words), "the duty of the physician is to *imitate* nature as far as possible, and watch her methods of acting." But surely the physician strictly imitates nature in her most natural state, according to Dr. Merriman's own premises, when, during labor, he induces by art that state of anæsthesia, which, in Dr. Merriman's opinion, *originally* pertains to parturition in the human mother. The female in an uncivilized state more truly shows us the true method and types of nature, than the female in a civilized state. Besides, are we not called upon to relieve the woman, when we can, of her sufferings as an act of common professional duty and common professional humanity? In law and in morals, we judge greatly of actions by their intent. No accoucheur would intentionally inflict upon his patient the agonies of labor by a deliberate act of *commission* on his part. Is an accoucheur properly justified in intentionally refusing to save a patient from the agonies of labor, by a deliberate act of *omission* on his part? When a child, at birth, is intentionally destroyed by the loss of blood, it does not matter in the eye of the law whether the death has been produced by voluntarily *omitting* to tie the umbilical vessels, or voluntarily *opening* other vessels.

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[1] Arguments against the Indiscriminate Employment of Anæsthetic Agents in Midwifery. London, 1848.

Up to within the last few months, and till the power of annulling pain by the inhalation of ether was discovered, severe bodily pain, such as we witness in surgery and midwifery, was universally regarded by the profession as possessing an evil and morbid effect. Some of the opponents of anæsthesia have taken up a novel and different view; and as a medical argument against the practice of anæsthesia in midwifery, it has been particularly averred that a labor-pain is a "desirable, salutary, and conservative manifestation of life force" (Dr. Meigs). Parturient "pain is," says Dr. Copland,[1] "often salutary as respects its effects; neither its violence nor its continuance is productive of injury to the constitution," &c. No opinion, I believe, could be more erroneous. I have already shown, from the evidence of extensive statistical returns, that some of the graver operations of surgery are now much less fatal in their results when patients are operated on under the condition of anæsthesia, and consequently without any attendant pain, than the same operations were formerly when patients were submitted to all the agonies of the surgeon's knife in their usual waking and sensitive state. The prevention of the pain in surgical operations is, in other words, one means of preventing danger and death to those operated on; the saving of human suffering implies the saving of human life. And what holds good in relation to pain in surgery, holds good in relation to midwifery. Pain, whenever it is great in degree or great in duration, is in itself deleterious; and by shielding our patients by anæsthetic measures, against the more severe portion of the pains of parturition, we not only preserve them from the agony of their more immediate sufferings, but we preserve their constitutions also from the effects and consequences of these sufferings. And the evidence which I have adduced in the preceding pages tends to prove, that when thus freed from the endurance of pain by artificial anæsthesia, they assuredly, as a general rule, make both more rapid and more perfect recoveries than when such means are not used; just as woman in a savage state, and where she enjoys a kind of natural anæsthesia during labor, recovers more easily and rapidly from the shock of labor than the civilized female. In short, in cancelling the pains of parturition by anæsthesia, we also I believe to a great extent cancel the perils of the process; for the mortality accompanying labor is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital, when under Dr. Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labor, 22 died; *or* 1 *in every* 320. In 452 of his cases, the labor was prolonged above twenty hours; and of these 452, 42 died; *or* 1 *in every* 11—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution.

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[1] Dictionary of Practical Medicine, vol. iii. p. 484.

The last, and certainly the principal objection against anæsthesia in midwifery, is the supposed danger accompanying the exhibition of anæsthetic agents. In the earliest paper which I published on the subject of chloroform, I pointed out this circumstance in mentioning various cautions in the use of it. When we consider the immense extent to which it has already been employed in all quarters of the world, in medicine, surgery, and midwifery—the little care sometimes observed in its use—and the deleterious and dangerous articles, with which, as we have seen (p. 614), it is sometimes mixed, the wonder is that so few alleged accidents have happened from its employment. By saving a vast amount of human suffering, it has already, I believe, been the means of saving no small amount of human life; and it is assuredly improper to argue, as some have done, that the mere chance of its disagreeing with some rare and special constitution, now and then, is any valid reason for refusing its use for the abatement and abrogation of human suffering. If there were any soundness in the reasoning, a thousand things beside would require to be abandoned. Railways, steamboats, stage-coaches, &c., when used as substitutes for the natural and physiological function of human progression, are ever and anon attended with accidents to limb and life. But surely no one would, from this, maintain that these means of conveyance should, in consequence, be abandoned. Many persons are annually drowned in bathing—should bathing, therefore, be prohibited, and this powerful means of maintaining and restoring health be entirely forsaken? According to the Registrar-General's official returns, a great number of lives are lost yearly in England by the improper medical use of opium[1]—should the use of opium, therefore, be given up? Patients sometimes sink under the depressing acti of antimony, calomel, &c.—should these valuable drugs, therefo be banished from the Pharmacopœia? Many a patient has perisl in consequence of venesection—should this operation be expun from the art of surgery? From mistakes and errors, &c., in di nosis and practice, medicine and surgery are sometimes the unha means of destroying instead of saving life—should these arts c sequently, be interdicted? Works on medical subjects have so times led both patients and practitioners into serious and f errors—should no medical works, therefore, be allowed to printed? Long ago, Raynalde, in sending forth the FIRST w on midwifery ever published in the English language, seem have foreseen that, against the utility of publishing any bool books on midwifery, the same argument would be used as we h found in our own day used against the application of anæsthesi midwifery; and he has answered the argument in a style so ear and apposite, that I shall quote this reply, by anticipation, as it his own words, in an abridged form:

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[1] In 1840, out of every 1,000,000 living in England and Wales, 24 were poisoned by opium, and 22 by other medicines improperly given to children below the age of five years alone.—See Seventh Annual Report, p. 82. See also Taylor on Poisons, p. 187, &c., for the great numbers destroyed in England by opium, &c., improperly given.

"Loe!—such is the lyght judgement of them, the which in e thyng wherof may ensue both good and evyll, have alwayes t eyes wakyng and firmely affixed and directed uppon the e pyckyng and choosyng out the worst of every matter, omitt and leavyng to speake of the best, as the thyng whiche nothyng to theyr purpose. If every thyng in this worlde sho be wayed and passed uppon after this sort, then shoulde w fayne to condempne and banishe those thynges farre from us, wh are at this tyme, accompted and taken for the most necess worthy, and of greatest price or estimation. For to be short, t is nothyng under Heaven so good, but that it may be perve and turned to an evyll use, by them that be evyll and naught t selves, and so abuse it: ne is there anythyng so absolute and fecte, but by the occasion of the abuse thereof, at one tyme or o may and doth ensue great daunger and damage to manky Fyre and water be two right necessary elementtes to the use of without the whiche we coulde not lyve: yet by the meanes of t many a miserable deed hath been done and perpetrated. By hath ben consumed and devoured whole Cities and Countries. water, swalowed and drowned infinite men, shippes, yea, and w regions. Agayne," he continues, "meate and drynke, to the mod users thereof, doth minister and mayntayne lyfe: And, contra the unmeasurable and unsaciate gourmauntes and gluttons, it full many thousand times brought surfeetes, sicknesse, and at laste, death." * * * * But he argues, "shoulde men, for avoydyng al these foresayde inconveniences, and for the reasons ab sayde condempne and bannysh fyre and water, forsake their meate and drynke? * * * * No, it were but madnesse once to thinke it. Therefore, I say, the judgment of that eye can never be equall and indifferent, whiche hath more respecte and regarde alwayes to the displeasures and hurtes possible to happen (only through the misuse of a thyng), than to the emolumentes and profites dayly and commonlye lyke to ensue to the well users of the same. That that of it selfe is good, is never to be disalowed for the sake of them that do abuse it. For * * * * to them that be good them selfe, everythyng turneth to good, whatever it be, is to them a sufficient matter and occasion therein to seeke the glory of God, and the onely profyte of theyr even Christen. And contrary, such as be of an yll disposition, in everythyng (be it never so good and salutarie), picketh out matter of maynteynuance to theyr lewdnesse, turning matters of sadnesse and discretion to foolyshe and pyvyshe pratyng contention."[1]

CHAPTER XI.

ANSWER TO THE OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY, ADDUCED BY PROFESSOR MEIGS OF PHILADELPHIA.[2]

STARBANK, BY EDINBURGH, 1st August, 1848.

MY DEAR SIR—A few days ago, I saw your excellent epistle to me on the use of anæsthesia in midwifery, extracted, in an abridged form, from the Philadelphia Medical Examiner of March last, into the London Medical Gazette and Lancet. It reminded me, that amid other avocations and work, I had hitherto indolently omitted to answer the objections contained in your able and kind letter. And I feel that I am the more to blame for this neglect—on one account—namely, that as in your own country, so also in ours, there are few or no living obstetricians, whose opinions and name carry, and deservedly carry, more weight with them than yours. Be so good then as bear with me now for a few minutes, while I endeavor to state in what respects I am inclined to demur to your arguments against anæsthetic midwifery.

On reperusing, as I have just done, your esteemed letter, it appears to me that in it you ground your opposition to the adoption of anæsthesia in midwifery upon four or five different arguments, although you do not specialize them. I shall notice each of these arguments separately. You have not placed them in any particular order. I shall begin first with the one which you placed last.

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[1] Prologue to Raynalde's Birth of Mankinde, Edition of 1565, p. 9.

[2] From the Association Medical Journal, July, 1853, p. 582. Read before the Dublin Obstetric Society in February 1849.

1. *You object to anæsthesia in deliveries requiring "chirurgical intervention," and especially in forceps operations, on the ground that the sensations of the patient afford us our best aid for the introduction of the instrument.*

In order to introduce the forceps with the greatest safety to the mother, you state that—to quote your own words—"the best guide of the accoucheur is the reply of the patient to his interrogatory, 'Does it hurt you?' The patient's reply, 'Yes or No,' are," you observe, "worth a thousand dogmas and precepts. I cannot therefore," you continue, "deem myself justified in casting away my safest and most trustworthy diagnosis, for the questionable equivalent of ten minutes' exemption from pain, which, even in this case, is a physiological pain."

In answer to this novel objection, you will excuse me when I say, for I say it most conscientiously, that I think every man who ventures to use the forceps, in any midwifery case, ought to know the anatomy of the parts implicated, a thousand-fold better than you here presuppose. You would have the accoucheur guide his instrument, not so much by his own anatomical knowledge, as by the feelings and sensations of his patient. In this, as in other points relative to any novel question in practice, we can often, it appears to me, best perceive the soundness or unsoundness of our views upon it, by considering and contrasting them with our established views on other analogous questions, regarding which the opinions of the profession have been long ago fixed and determined. Now what would the surgical world, at this time of day, think of an operator, who in making a ligature of a large artery, such as the humeral, placed his chance of discriminating the attendant nerve from the bloodvessel which he wished to tie, by appealing not to his own anatomical knowledge, but to the feelings of his patient, as he touched the suspected structures, "Does it hurt you?—Yes, or No." Would not our surgical brethren denounce and decry the capabilities of any man who, in operating, required to have recourse to such imperfect and incompetent means for his anatomical direction and diagnosis? Would it be right and moral in a surgeon to deny to his patients the advantage of anæsthesia, in order that their sensations and sufferings should make up for his want of anatomical and operative knowledge?

But in saying this, do not, I pray you, for one moment suppose that I fancy that the argument which you adduce betrays any want whatever of the highest degree of operative skill on your part.

Nothing could be further from my thoughts. And to confess the truth, I do sincerely believe that you yourself, while using the forceps, do not require to have recourse to any such rude rule as you here propound—and that, in fact, the rule itself, and the objection to anæsthesia in operative midwifery which it contains, is an *afterthought* on your part, which has only sprung up since the practice of anæsthesia was proposed. For, in looking over the excellent precepts which you have given, relative to the use of the forceps, in the valuable work on Midwifery which you published a few years ago, viz., the Philadelphia Practice of Midwifery, I find no trace or mention whatever of such a rule as you have quoted above, in your letter to me. If that rule really formed, as you now state, the "safest and most trustworthy" guide in the operation, you would certainly at least have noticed it, or alluded to it in some way. In the precepts which you laid down in your work, you would assuredly not have forgot that one rule which, you say, is worth a "thousand other dogmas and precepts." And it would, I think, have been only the more incumbent upon you to have mentioned it, seeing that all other authors omit the notice of it.

I feel assured that when you come to reconsider, "dispassionately," your opinions regarding the non-employment of anæsthesia in operative midwifery, you will alter these opinions. And when you come to employ anæsthesia in actual practice, in cases in which the forceps are used, you will find that, instead of impeding the application of instruments, the anæsthetic state very greatly facilitates it. It enables you to guide the forceps far more safely to their destination, because it enables you, without any pain to the patient, to introduce your fingers for this purpose far more deeply between the head and maternal structures than you could do if the patient were awake, and in her usual sensitive state. You yourself state, in your published work on midwifery, that care should be "taken to direct the point (of the forceps) by the two fingers, as far as they can reach" (p. 300). "If," you again observe, "any difficulty occurs in getting the second blade forward enough, the two left fingers that are guiding it will serve to guide it edgeways into the proper position." Now, the state of anæsthesia, I repeat, gives you (as I have several times found) the power of fulfilling these and other most important rules, to an extent that never can be attained without it; and I am sure you will find them worth any "thousand dogmas and precepts" derivable from the mere sensations of the patient.

Besides, these sensations, or rather the expression of them, would constantly betray you if you *did* place any dependence upon them. Under the same amount of pain, scarcely any two women would give you exactly the same expression of suffering. What one woman would loudly complain of, another would declare to be nought.

Before interfering instrumentally with the forceps, the labor ha generally been allowed to endure for twenty or thirty long hour After a poor patient has undergone such a protracted ordeal of pai and suffering, her mind is not, I fear, in general in a very fit stat to guide the operator by her sensations or directions.

At page 302 of your published work on midwifery, you stat that when the forceps are used, the patient's mind is naturall wound up to a state of great anxiety; "it is strained," you observ "to the highest tension, by the mere thought that she is under th operation." Now putting entirely out of view, for the moment the propriety of our saving our patients the increased corpore agony attendant upon instrumental delivery, is it not, let me as our right and our duty, as medical men, to save her, as we can d from this trying state of mental anxiety at the time of operating In most cases, she will have been suffering and struggling on fo many hours previously. Why, then, thus needlessly and greatl intensify both her mental anxieties and physical sufferings at th time of our instrumental interference, when her strength, alike mind and body, are perhaps little calculated to bear any increase suffering; and, above all, when the resources of our art furnish u with simple and certain means of saving her from the unnecessar endurance of the one state and of the other?

But in instrumental delivery, besides greatly facilitating th application of the forceps, and relieving the patient from endurin the pains of the operation, and that "highest tension" of min which is present during it, the state of anæsthesia saves her, believe, also, in a great measure, from the effects of the shock the operation, and thus gives her a better chance of recovery. I we omit it, we omit, I believe, not only a means of saving her fro the sufferings attendant upon the operation, but a means of savin her from some of the dangers attendant upon it. When first publishing on the subject of anæsthesia in midwifery, in Februar 1847, I offered one or two observations on this point, which subsequent surgical statistics have amply fulfilled. In allusion to som cases of operative delivery, which I recorded, I observed: "Th cases I have detailed sufficiently show its value and safety in case of operative midwifery. And here, as in surgery, its utility is certainly not confined to the mere suspension and abrogation of conscious pain, great as, by itself, such a boon would doubtlessly be. But, in modifying and obliterating the state of conscious pain, the nervous shock otherwise liable to be produced by such pain—particularly whenever it is extreme, and intensely waited for and endured—is saved to the constitution, and thus an escape gained from many evil consequences that are apt to follow in its train."

The observations which I have hitherto made refer entirely to your opinion of anæsthesia in instrumental delivery. But,—

2. *You object to anæsthesia in natural labors, because you hold that the pain of natural labor should not be annulled, and that it is calculated to promote the safety of the mother.*

You regard, you say, "the pain of a natural labor as a state not by all possible means and always to be eschewed and obviated,"—"a labor-pain being," you declare, "a most desirable, salutary, and conservative manifestation of life-force."

In the above expressions, you make no distinction between the two separate and distinct elements of which so-called labor-pain consists, viz.: 1. The contractions of the uterus, and 2. The sensations of pain resulting from these contractions. If you apply the language I have quoted to the first of these elements, the uterine contractions (which contractions are not annulled by anæsthetics), I decidedly and entirely agree with you. If you apply it, however, to the sensations of pain produced by the uterine contractions (which sensations are annulled by anæsthetics), I most decidedly and entirely dissent from your opinion.

In your work on midwifery, you make, correctly, the important distinction to which I refer. You state (p. 148) that "the word (labor) is highly expressive of the violent and painful struggles and efforts of the woman." You add that "the essential element of labor is the contraction of the muscular fibres of the womb." And, at page 303, in speaking of the strength of these uterine contractions, you observe, "let it be well borne in mind that the expulsive powers of the womb are enormously great." In more than one place in your work, you allude to the intensity of the sensations of pain, "the pangs and agonies of travail," as you term them (p. 155); and at page 153 you speak of the "painful sensations" of the mother, in the last part of labor, as so great in degree "as to be absolutely indescribable and comparable to no other pain." In your still later work on Female Diseases, speaking of these pains—the pains of parturition—you observe, "Men cannot suffer the same pains as women. What," you continue, "do you call the pains of parturition? There is no name for them but Agony."

The muscular contractions of the uterus form, you say, the "essential element" of labor. In that opinion you and I are at one, and further, I quite agree that this cannot safely be "eschewed and obviated" in natural labor; nor are they "eschewed and obviated" under the proper use of chloroform.

But the pain, the second element, is a non-essential element in the process. It is non-essential, because—1. Labor, that is the uterine contractions, is occasionally, though very rarely, in the course of practice, seen to accomplish the full expulsion of the child with little or no pain; 2. In whole tribes of the human race, as in some of the black tribes, comparatively little or no pain seems to be endured, if we may believe various authorities; and 3. Hundreds of women have, during the last year, been delivered with perfect safety, but without any pain, while placed under the influence of anæsthetic agents.

I hold the pain to be *non-essential*, and I protest against the truth of your opinion, that "the pain of a natural labor is a state not by all possible means to be eschewed and obviated." On the contrary, I maintain that we omit and forego a mighty part of our professional duties whenever we forget the axiom of Bacon, that "it is the office of a physician not only to restore health, but to mitigate pain and dolors." And if, as medical men, we are called upon to mitigate and remove pain of any degree in our fellow-beings, we are surely called upon to mitigate and remove those "pangs and agonies of travail," as you term them, which in degree are, in your own language, "absolutely indescribable and comparable to no other pain,"—"pains for which there is no other name but Agony."

In your practice, you, like other medical men, constantly use measures to mitigate and relieve the pains of headache, of colic, of sciatica, of pleurodyne, of gout, rheumatism, and all the other innumerable "dolors" that flesh is heir to. Like other physicians, you deem it, I doubt not, your duty to wield the powers of your art, in order to free those that submit themselves to your medical care, from these and from other similar sufferings. But if it is right for you to relieve and remove these pains, why is it not right for you also to relieve and remove the pains accompanying the act of parturition? I cannot see on what principle of philosophy, or morality, or humanity, a physician should consider it his duty to alleviate and abolish, when possible, the many minor pains to which his patients are subject, and yet should consider it improper to alleviate and abolish, when possible, pains of so aggravated a character, that, in your own language, they are "absolutely indescribable and comparable to no other pains,"—"pains for which there is no other name but Agony."

3. *You object to anæsthesia in natural labor, because you deem the pain of natural labor "a physiological pain."*

"The sensation of pain in labor is," you observe, "a physiological relative of the power or force," and "to be in natural labor is the culminating point of the female somatic forces."

Now, for the reasons that I have already stated, I entirely doubt if we should look upon the severe sensations of pain endured by our patients as truly "physiological," for, as I have just stated, they are *not* essential to the mechanism and completion of the process in the white races of mankind, and they are absent, to a great degree, in the black. The severity of them could, I think, be easily proved to be the result of civilization, and, as I believe, of that increased size of the infantile head which results from civilization. Parturition is always physiological in its object, but not in some of the phenomena and peculiarities which attend upon it in civilized life.

But, waiving this point, or the discussion of it, let me state, that even if I allowed all the intense pains of parturition to be "physiological pains," I cannot conceive that to be any adequate reason for our not relieving women from the endurance of them. Because nature has fashioned any particular physiological function in any particular manner, that, I opine, is no reason why the science and art of civilized life should not, when possible, alter and amend its workings. If it were improper for us, for instance, to intermeddle with the functions of the hair of the head, or of the skin generally, then all hats and other coverings for the scalp, all clothings and coverings for the body, should be at once abandoned and unconditionally condemned. If it were improper for us to alter and amend the functions of the eye, then all optical glasses, the telescope, the microscope, &c., must be thrown aside. And, indeed, no later than the seventeenth century it was held and argued so in England. For, in his history of the first beginning of the Royal Society of London, Sprat tells us that it was generally believed that this "new experimental philosophy, namely, the philosophical papers laid before the Society, was subversive of the Christian faith," and many, he adds, mortally hated the newly invented optical glasses, the telescope, and the microscope, as *atheistical inventions*, which perverted our organs of sight, and made everything appear in a new and false light (D'Israeli). You argue as if we should not use means to eschew the pains of parturition *because* that pain is physiological. When Columbus first discovered your mighty American continent, a large portion of the inhabitants were unprovided with any kind of dress or covering. "To most of them," says Robertson, "nature had not even suggested any idea of impropriety in being altogether uncovered." And I do think that men living in such a state, could, against the fashion of dressing, use with far greater propriety and consistency than you or me, your own argument against anæsthetics in labor. Chloroform and ether should not be used in labor, you argue, because the pain against which they protect us is natural and physiological. No kinds of clothing or dress should be used, the original Americans might have equally argued, because the cold or heat against which they protect us are natural and physiological.

I have a letter lying before me on the subject of anæsthetics in midwifery, by a very highly and very justly esteemed teacher of midwifery in Dublin. "I do not," he writes, "believe that any one in Dublin has as yet used ether in midwifery; the feeling is very strong against its use in ordinary cases, and merely to avert the ordinary amount of pain which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural labor; and in this feeling I heartily and entirely concur."

The argument thus used, and so very well expressed by my Irish correspondent, is one which has been often adduced and repeated during the course of the past year. Some minds at first gave immense weight and importance to it. For my own part, I must confess that I never could view it as possessing any great force. Look at it as applied to any other practice which happens to be sufficiently old and established; and then we shall see it in its true import. Supposing, for example, it referred to the *first* introduction of carriages into use; it would then read thus:—"I do not believe that any one in Dublin has as yet used a carriage in locomotion; the feeling here is very strong against its use in ordinary progression, and merely to avert the ordinary amount of fatigue which the Almighty has seen fit—and most wisely we cannot doubt—to allot to natural walking; and in this feeling I heartily and entirely concur."

Nay, this frequently repeated argument against new innovations becomes not only, I think, ridiculous, but really almost irreverent, when we look far backward into the march of civilization, and apply it to any practices that are so very long established as to be very antiquated, and with which, therefore, the human mind has been long and intimately familiarized. Some one, but who I cannot pretend to say, no doubt first introduced the practice of wearing hats or bonnets, or some covering for the head. Supposing this practice, however, stoutly resisted, as doubtlessly it was at first, then the argument of my Dublin friend against this innovation would read somewhat as follows: "I do not believe that any one in Dublin has as yet used a hat to protect his head; the feeling here is very strong against its use in ordinary weather, and merely to avert the ordinary amount of wetting and cold which the Almighty has seen fit—and most wisely we cannot doubt—to allot to mankind; and in this feeling I heartily and entirely concur." Some day a canal will, in all probability, be made through the Isthmus of Panama. It has, you are well aware, long been proposed to cut one; and there and thus unite the Atlantic and Pacific Oceans. When it was proposed in the sixteenth century, a priest of the name of Acosta brought forward the following reasons against it. "I am," said he, writing in 1588, "of opinion that human power should not be allowed to cut through the strong and impenetrable bounds which God has put between the two oceans, of mountains and iron rocks, which can stand the fury of the raging seas. And, if it were possible, it would appear to me very just, that we should fear the vengeance of Heaven for attempting to improve that which the Creator in his almighty will and providence has ordained from the creation of the world." The arguments which are here brought forward by the earnest Spanish priest, against man meddling with and altering the impediments to navigation caused by the natural mechanism of the Isthmus of Panama, are essentially the same as those lately brought forward against man meddling with and altering the agonies caused by the natural mechanism of parturition in the civilized woman. We can all, perhaps, at this time of day, see through and smile at the character of the old priest's argument with regard to the supposed impropriety of changing and cancelling, if possible, the natural obstruction produced by any isthmus. Some years after this, perhaps, our descendants will equally see through and smile at the analogous modern argument in regard to the supposed impropriety of changing and cancelling, when possible, the physical suffering produced by a physiological function.

The truth is, all the tendencies of man, in a civil state of society, are to intermeddle with and change, and, as he conceives, improve the action of almost every function in the body. And each such improvement has, at the time of its introduction, been, like the ractice of anæsthesia, very duly denounced as improper, impious, c. &c. I might refer to numerous such cases. Let me cite only ne example. The human fingers are admirably constructed by ur Creator for the function of seizing and lifting objects. The ite Sir Charles Bell wrote a whole octavo volume—a Bridgewater 'reatise—on the mechanism of the human hand, as beautifully lapted for this and other functions. In the reign of the earlier tuarts, forks were introduced from the continent to assist our ands in the act or function of seizing and lifting the divided ortions of meat, &c., that we wished to eat. But this was a very id and uncalled for innovation upon the old and established physiogical functions of the human fingers; and, at the time, it was as loudly opposed and decried as the modern employment of anæsthetics in aiding the physiological function of human parturition D'Israeli tells us that the use of forks was so much reprobated i some quarters, that some uncleanly preachers denounced it "as a insult on Providence not to touch our meat with our fingers.' Nature herself has provided us with fingers of flesh, and bone, and nerve, and consequently is it not unnatural and impious in man to attempt, in his human pride and arrogance, to substitute for these artificial metallic fingers of silver and steel?

I repeat—all our tendencies and workings, in the present state o civilization, are attempts to intermeddle with and change and improve the action of almost every function in the economy. And assuredly, if we use means in regard to the function of parturition with the view of ameliorating and abolishing the unnecessary, but as you call them, the "absolutely indescribable" pains that attend upon it, we should be doing nothing more than what you and I and all of us are ever doing in relation to most of the other natural o physiological functions of our own bodies.

Let me illustrate this last remark by one more example, for, as I have already said, it is only in this way that we can properly judge of the soundness or unsoundness of our views of novel points in theory or practice. You are well aware that the act of parturition has been often familiarly compared, as the late Professor Hamilton expressed it, "to the toils of a journey," and like it divided into stages. "The sufferings of the mothers," says he, "have been in most languages compared to those of travellers." Now let us for a moment continue this natural simile between the function of parturition and the function of progression. You maintain that "labo is the culminating point of the female somatic forces." One of the most illustrious Presidents of your great American Republic—Thomas Jefferson—makes in his Memoirs a remark of precisely the same import regarding walking or progression. He describes the act of walking, but not exactly in the same words, as the kind o "culminating point of the human somatic forces."[1]

Few, or none, perhaps, will question the abstract truth of Jefferson's observations on this point. But, because walking or progression is a "physiological" function, and the practice of it i reputed salutary, would this be, with you, a proper and sufficient reason for never setting aside or superseding in any way this "physiological" state, in the same way as you insist, on the same grounds, that the physiological pains of labor should not be set aside or superseded? Because progression is a natural condition, would this be any adequate reason for your medical advisers adopting your own arguments against anæsthesia in midwifery, and insisting upon this, that, the next time you travelled from your own city of Philadelphia to the cities of Baltimore or New York, you should walk the distance on foot instead of travelling it by railway or other conveyance? What opinion would you form of the judgment of any medical adviser to whom you intrusted your own health, if, on going next time to the New York or Baltimore railway station, he should gravely and solemnly repeat to you, as his patient, what you tell your midwifery patients, and, in your own language, advise you to try to accomplish the intended journey on foot as (to quote your own words) "a desirable, salutary, and conservative manifestation of life-force?" And yet this really would be nothing more than making your *argumentum ad fœminam* an *argumentum ad hominem.*

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[1] Since writing the above, I have turned up Jefferson's Memoirs to get his own words. "Walking," says the American President, "is the best possible exercise; habituate yourself to walk very far. The Europeans," he continues, "value themselves on having added the *horse* to the uses of man; but I doubt whether we have not lost more than we have gained by the use of this animal. No one has occasioned so much (as the horse) the degeneracy of the human body. Our Indians go on foot nearly as far in a day, for a long day, as an enfeebled white does on his horse; and he (the Indian) will tire the best horses." —Memoirs, vol. i. p. 287.

You state, in a passage which I have already quoted, that even the agony accompanying instrumental delivery by the forceps is a "physiological pain." I do not, I confess, see why the suffering attending the use of the forceps, when the head is impeded by any cause of obstruction, should be regarded as a "physiological pain," any more than the suffering attending the use of the catheter, in obstruction from the prostate gland, or other morbid conditions of the urethra, should be regarded as a "physiological pain." They are both operations intended to remove the natural contents of the respective viscera, when their operative removal becomes necessary.

But let us waive this point, and return again to the analogy between the functions of progression and parturition. Suppose you plead with your medical adviser that, instead of insisting on your going on foot, he should allow you *for once* to take advantage of artificial assistance, and proceed on your journey from Philadelphia to Baltimore or New York by railway, because you were unable to walk the distance in consequence of being incapacitated by a rheumatic knee, or a sprained ankle, or an inflamed or blistered toe, and he replied to you that you should not care for this, but still proceed and suffer, because the pain you might thus suffer was (to use again your own language) still only a "physiological pain." Would that argument be any adequate philosophic consolation under the endurance of your suffering? or would you not laugh at the logic of your medical adviser, and take your seat in the railway in spite of his doctrine? And I have a fancy that betimes, in midwifery, patients *will* learn to adopt exactly the same line of practice under the analogous circumstances, and think, and act, too, exactly in the same way.

4. *You object to anæsthesia in labor, because the mother, in escaping by it from the "pangs and agonies of labor," may, in a few rare cases, be thus made to encounter danger to her own life.*

"Should I," you observe, "exhibit the remedy for pain to a thousand patients in labor, merely to prevent the physiological pain, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the life or the death of one in a thousand in a questionable attempt to abrogate one of the general conditions of man?" Let me add that I have seen this argument of yours already repeated from your letter, and strongly insisted upon by the opponents of anæsthesia in this country.

And, indeed, in a new practice such as that of anæsthesia, and with which the mind is yet not at all familiarized, the above forms one of that kind of apparently strong statements which it is impossible to answer directly, or, indeed, by any other way than by taking, as I have already said, a corresponding illustration and simile from some other matter with which the mind is already familiarized. Let us for a moment longer, then, adhere to the familiar comparison which I have already taken up, under the last head, between the physiological function of human parturition, and the physiological function of human progression. Suppose, then, that you and I were standing at the Philadelphia station on the first day of the opening of the railway to Baltimore or New York. I wish the passengers to Baltimore or New York, or the shorter and intermediate stations, to proceed thither by railway; but you argue with them, like President Jefferson, that "progression is the culminating point of the human somatic forces," and that "walking is a desirable, salutary, and conservative manifestation of life-force," and that progression being a "physiological function," and fatigue a physiological pain, they ought to proceed on foot. I say "No. Place yourself in a railway carriage, and thus eschew and obviate all the great fatigue and useless over-exertion of foot-travelling." Then comes that answer and argument of yours which I have quoted, and which runs as follows. "But should I exhibit, sir, the remedy for fatigue (*a railway carriage*) to a thousand travellers, merely to prevent the physiological exertion and fatigue of walking, and for no other motive, and if I should, in consequence, destroy only one of them, I should feel disposed to clothe me in sackcloth, and cast ashes on my head for the remainder of my days. What sufficient motive have I to risk the death of one in a thousand in a questionable attempt to abrogate one of the general conditions of man—viz., his power of progression by walking?"

I shall not stop to inquire whether, among our supposed lady passengers or patients (uninured, as most of them are, either to long pain or long walking), more than one in a thousand would not be worn out and destroyed by taking the journey on foot. A less proportion, I believe, would be found to be ultimately destroyed by the perils and dangers of the journey by railway than by the exertion and fatigue of the journey on foot, and the walk would shake and damage, both temporarily and permanently, many more constitutions than the railway carriage. I have a firm conviction that, on the great scale, there would be found a more absolute saving both of human life and of human health by adopting the means invented by art than the means provided by nature. And I most firmly believe that yet a similar difference will be found to hold good between the two corresponding practices of allowing women to pass through labor afflicted with all their usual physiological "pangs and agonies," and carrying them through that process without their being subjected to the endurance of these pangs and agonies.

But I proceed to remark, that if your supposed theory with regard to the function of parturition were carried out in regard to the other functions of the human body, it would produce a vast and mighty revolution in the practices of civilized life. Follow it out, for instance, with regard to any one of them, as, for example, with regard to the one we have already spoken of, viz., progression, and see what would be the results. Ever and anon our newspapers contain paragraphs, telling us of one or more human lives being lost by collisions on railways, explosions of steamboats, upsettings of stage-coaches, &c. Consequently, according to your doctrine, the featherless biped pedestrian man, should no longer, when travelling, fly in railway cars, ply in steamboats, ride in coaches, &c., for these are evidently all so many questionable attempts to abrogate what you call "one of the general conditions of man, viz., his original pedestrianism."

In the great government and police of nature, disease and death are among the most certain "general conditions of man." If your theory were true, the practice of medicine itself should, I fear, be at once and summarily abandoned, for perhaps, in your own language, it is, at best, a questionable attempt to abrogate one of the general conditions of man, and I am sure you will agree with me, that in this "questionable attempt" human lives are often lost from the mistakes, or the passiveness, or the want of knowledge and skill, on the part of the physician. In England and Wales, in 1840, there were, according to the returns of the Registrar-General, above 100 persons publicly and officially reported as having died from the effects of one drug alone, opium. But would this be any reason, or any ground of reason, for abandoning in medicine the use of opium, perhaps, in itself, the most valuable of all the remedies in our pharmacopœia? Would this be any adequate argument for refusing to relieve, by a dose of opium, the next appropriate case of pain that you are called to? Or because chloroform or ether, in a very rare case, now and again produces deleterious or even fatal consequences, should we refuse, in a thousand other persons, to mitigate and annul their agonies by its use?

In your esteemed letter to me, you quote some remarks from the celebrated old work—Raynalde's Birth of Mankinde, the first book on Midwifery printed in English. Look at the Prologue to the work. It is excellent in reference to the very matter we are discussing, viz., whether the rare accidents, from abuse or otherwise, to which any good gift may occasionally subject those who use it, should be a reason for repudiating the general use of that gift. "There is not anything," says Raynalde, "so absolute and perfecte, but by the occasion of the abuse thereof at one tyme or other, may and doth ensue greate daunger and damage to mankynde." He instances fire and water, "two right necessary elementtes to the use of man, without the whiche he could not lyve," yet sometimes "by fyre hath ben consumed and devoured whole cities and countreys. By water swalowed and drowned infinite men, shippes, yea and whole regions. Agayne," he continues, "meate and drynke, to the moderate users thereof, doth minister and maynteyn life; and contrary, to the unmeasurable and unsatiate gourmauntes and gluttons, it hath full many thousand tymes brought surfettes, and sickenesse, and at the laste, death. . . . But," he argues, "shoulde men, for the avoydyng al these forsade inconveniences, and for the reasons abovesayde, condempne and bannysh fyre and water, forsake theyr meate and drynke? No, *it were but madnesse once to thinke it.*"

Before passing from these, your supposed dangers of anæsthetics, let me add two remarks—1st. I do believe that if improperly and incautiously given, and in some rare idiosyncrasies, ether and chloroform may prove injurious or even fatal—just as opium, calomel, antimony, and every other strong remedy and powerful drug will occasionally do. Drinking cold water itself will sometimes produce death. "It is well known," says Dr. Taylor, in his excellent work on Medical Jurisprudence, "it is well known that there are MANY cases on record, in which cold water, swallowed in large quantity, and in an excited state of the system, has led to the destruction of life."[1] Should we, therefore, never allay our thirst with cold water? What would the disciples of Father Matthew say to this? But, 2dly. You and others have very unnecessary and aggravated fears about the dangers of ether and chloroform, and in the course of experience you will find these fears to be, in a great measure, perfectly ideal and imaginary. But the same fears have, in the first instance, been conjured up against almost all other innovations in medicine, and in the common luxuries of life. Revert again to our old simile regarding travelling. Cavendish, the Secretary to Cardinal Wolsey, tells us, in his life of that prelate, that when the Cardinal was banished from London to York by his master—that regal Robespierre, Henry VIII.—*many* of the Cardinal's servants refused to go such an enormous journey—"for they were," says Cavendish, "loath to abandon their native country, their parents, wives, and children." The journey, which can *now* be accomplished in six hours, was considered *then* a perfect banishment. We travel now between London and Edinburgh (some four hundred miles) in twelve or fourteen hours. A century ago the stage-coach took twelve or fourteen days. And in his Life of Lord Loughborough, Lord John Campbell tells us that when he (the biographer) first travelled from Edinburgh to London, in the mail-coach, the time was reduced to three nights and two days; "but," he adds, "this new and swift travelling from the Scotch to the English capital was wonderful, and I was gravely advised," adds Lord John, "to stop a day at York, as several passengers who had gone through without stopping, had died of apoplexy from the rapidity of the motion."[2]

Be assured that many of the cases of apoplexy, &c. &c., alleged to arise from ether and chloroform, have as veritable an etiology as this apoplexy from rapid locomotion; and that a few years hence they will stand in the same light in which we now look back upon the apoplexy from travelling ten miles an hour. And as to the supposed great moral and physical evils and injuries arising from the use of ether and chloroform, they will by and by, I believe, sound much in the same way as the supposed great moral and physical evils and injuries arising from using hackney-coaches, which were seriously described by Taylor, the water poet, two or three centuries ago, when these coaches were first introduced. In his diatribe against hackney-coaches, Taylor warned his fellow-creatures to avoid them, otherwise, to quote his own words, "they would find their bodies tossed, tumbled, rumbled, and jumbled without mercy." "The coach," says he, "is a close hypocrite; for it hath a cover for knavery; they (the passengers) are carried back to back in it like people surprised by pirates; and, moreover, it maketh men imitate sea-crabs in being drawn sideways," and altogether "it is a dangerous carriage for the commonwealth." Then he proceeds to call them "hell-carts," &c., and vents upon them a great deal of other abuse, very much of the same kind and character as that lavished against anæsthetics in our own day.

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[1] Loc. cit. p. 8.

[2] Lives of the Lord Chancellors.

In the course of your remarks, you imply, I think, though you nowhere explicitly state, another objection to anæsthetics in midwifery, viz.:—

5. *You object to anæsthesia in labor, because you do not consider that the mother encounters danger to her health or life from the endurance of the pains.*

"I have been accustomed," you observe, "to look upon the sensation of pain in labor as a physiological relative of the power of force, and notwithstanding I have seen so many women in the throes of labor, I have always regarded a labor-pain as a most desirable, salutary, and conservative manifestation of life-force."

If you hold, as your language appears to me to imply, that the sensation of pain, even when, as in labor, the degree of the pain is "absolutely indescribable," has no morbid or deleterious influence upon those who endure it, then I most decidedly disagree with you. On the contrary, I sincerely believe that the human constitution is so constituted that it cannot endure pain, particularly when that pain is long in duration, or severe in degree, without being more or less affected and injured by it. I know of many medical and obstetric authors, from the time of Ambrose Paré down to the time of Travers, Gooch, Alison, Burns, &c., who have stated and explained the common and hitherto unchallenged opinion of our profession in all ages, that pain was, in itself, deleterious and destructive, causing depression of the heart, syncope, and even, when in excess, sometimes producing speedy and sudden death. But, till the late discovery in your own country of the possibility of annulling the pains of surgical operations by the inhalation of ether, I know of no writer in medicine, in surgery, or in midwifery, who held that pain, when "absolutely indescribable" in degree, was a matter of no importance in regard to the life or health of the sufferer, and should not be relieved even when we had the complete power of relieving it.

If the mere pain of the labor were, as you state, a "desirable, salutary, and conservative manifestation of life-force," its long continuance, the very length of it, would insure, more certainly, the health and safety of the patient, than its shortness. Anything "salutary and conservative" to the constitution, should manifestly be safe in proportion to the length, and dangerous in proportion to the shortness of the duration. But as far as regards the life and health of the mother, the pain of labor is perfectly the reverse of all this. It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labor, more subsequently died than when their pain did not exceed two hours; of those who were eight hours in labor, more subsequently died than of those that were four hours ill; of those that were twelve hours in suffering, more died than of those that were eight; and so on in a regular progression. The longer this supposed "salutary and conservative manifestation of life-force," as you term it, the greater became the mortality; so that, in the long run, the maternal mortality was fifty-fold greater among the women that were above thirty-six hours ill, than among those who were only two hours in labor; one in every six of the former dying in childbed, and only one out of every three hundred and twenty of the latter.

Some time ago, I published a long series of statistics, tending to show, that out of a large collection of cases of the same operation, performed with and without anæsthesia, those who were operated on under anæsthesia, and consequently without the usual suffering, recovered in a much larger proportion than those who had the same operation performed without anæsthesia, and whose constitutions were subjected to the endurance of the usual pains and agonies of the surgeon's knife.

The same result holds good, I believe, in Midwifery as in Surgery. Save the maternal constitution, either by natural or artificial anæsthesia, from the endurance of the pains connected with parturition, and you will enhance both the chances of her recovery, and the facility of it. Among your red Indian and other uncivilized tribes, the parturient female does not suffer the same amount of pain during labor, as the female of the white race; and in consequence of this escape, they recover far more rapidly from the effects of parturition; nor are fatalities at all common among them. So easy is the convalescence among uncivilized tribes, that Strabo, Marco Polo, and other historians and travellers, tell us of whole communities in which the husband immediately went to bed for a number of days, upon the birth of a child, and the wife watched and nursed him. "They that write the history of America," says Guillemeau, "tell of the women in that country, that, as soon as they be delivered, they presently rise up, and lay their husbands in their room, who are used and attended like women in childbed."

Among the patients who have been delivered in Scotland under anæsthesia, the rapidity of the stage of convalescence has, as a general rule, been increased in a degree that seems often to surprise the patient herself, as much as her escape from the labor-pains themselves. Many of my obstetric brethren have remarked this circumstance to me. In fact, on awaking after delivery, the patient does not encounter and endure the usual feelings of exhaustion and fatigue. Some have declared to me, that they have felt as if they had awoke from a refreshing sleep. And when we consider the capabilities for the enduring of suffering and exertion, among the class of patients in civilized life upon whom you and I attend, perhaps the propriety for employing anæsthesia during labor may appear more evident. Unaccustomed by their mode of life to much pain and fatigue, patients in the higher ranks of life are not fitted to endure either of them with the same power or the same impunity as the uncivilized mother, or even as females in the lower and hardier grades of civilized society; and hence there is the greater propriety and necessity in the physician employing all the means of his art, so as to save them, as far as possible, from their sufferings. To illustrate the point, let us revert again to our old comparison between the physiological functions of progression and parturition. Let us compare, for a moment, our ideas of the effects of fatigue from walking, and of pain from parturition upon the female constitution; and surely the comparison is not an unfair one for your views, as far as the severity of the effects of the two influences, physical fatigue and physical pain, are concerned, for surely the effects of pain, of "absolutely indescribable" pain, should be greater upon the constitution than mere muscular fatigue. Suppose then that our patients, at the end of the ninth month of pregnancy, had to walk on foot a continuous journey of one, two, three, six, or a dozen or more hours' duration, that is, of five, ten, twenty, or thirty miles, or upwards, instead of passing through a continuous journey of recurring labor-pains of the same duration, the pains gradually becoming stronger, and latterly becoming "absolutely indescribable, and comparable with no other pains"—what would be the result, with, say one hundred ladies of the upper classes of society? Some of them might be little or not at all affected by the journey; others, weak perhaps when they began, would suffer more or less severely from it. Not a few would be inclined sooner or later to stop, and beseech you, if you were the medical attendant upon them, to save them from further exertion and fatigue, by allowing them to be carried or coached the required distance. In answer to their solicitations, would you console them by telling them, that, after all, progression was a "conservative manifestation" of life-force, and free from danger, or would you take the other view, and give them means of travelling the required distance by carriage or rail? I am sure you would have recourse not to the former but to the latter, for you would fear and dread the effects of fatigue upon the fragile constitutions of your lady patients. And I repeat, that certainly the effects of the endurance of pain are as great, if not greater, upon the constitution, than the effects of the endurance of fatigue. But if you would allow your patients to ride the supposed journey, instead of unnecessarily forcing and compelling them to walk it on foot, equally, I think, should you allow them to escape what you term the "pangs and agonies of travail," by saving them by chloroform, or other anæsthetic agents, during their travail, from all the unnecessary endurance of these pangs and agonies.

You state, "I have not yielded to several solicitations, as to the exhibition [of chloroform], addressed to me by my patients in labor." If, when driving out into the country, you perchance meet one of your fair patients, a few miles from Philadelphia, walking homeward, but so tired and way-worn that every five or ten minutes she stopped and groaned for fatigue, "absolutely indescribable, and comparable to no other fatigue," I am sure you would consider yourself bound, on the principles of common humanity, not to withstand her "solicitations" to be driven home in your carriage, and thus relieved of her present anxieties and suffering. And I cannot see why, if you do this (and who would not do it?), to relieve a patient from the mere effects of fatigue, you could refuse to relieve the same lady when in "the pangs and agonies of travail," from the endurance of pains which are, in your own words, absolutely "indescribable, and comparable to no other pains."

"Perhaps," you observe, "I am cruel in taking so dispassionate a view of the subject." Of course, it would ill become me to pass any such judgment upon you; but I feel this, that you and I, and other teachers of midwifery, are placed, in reference to this question, in a position far more fearfully responsible than ordinary medical practitioners. The ordinary obstetric practitioner has little or no power, except over the relief or the perpetuation (according as he may choose it) of the sufferings of his own immediate patients. But you and I, as obstetrical teachers, may, through our pupils, have the power of relieving or of continuing the sufferings of whole communities. If, perchance, you persist for some years longer in your present opinion, it will have the effect of inflicting a large amount of what I conscientiously believe and know to be altogether unnecessary agony and suffering upon thousands of our fellow-beings. If you review and alter your opinions (which I earnestly hope you will do), and make yourself sufficiently acquainted with the peculiarities in the mode of action and mode of exhibition of chloroform during labor, a vast proportion of human suffering may, even within the next few years, be saved by your happy instrumentality and influence.

Feeling, as I do deeply, the great responsibility, in this respect, of your situation and of mine, I trust you will kindly pardon and excuse me, if, anywhere in the preceding pages, I may have appeared to defend my views with too much earnestness. If I had to rewrite or revise the observations, I would perhaps have stated them more accurately; but I must send them as they are. And along with them I beg to send, also, the most sincere esteem and reiterated respects of, my dear sir, yours very faithfully,

J. Y. SIMPSON.

CHAPTER XII.

REPLY TO OTHER MINOR OBJECTIONS TO ANÆSTHESIA IN MIDWIFERY.[1]

DR. CUMMING having directed the attention of the Society to an objection raised by Mr. Gream against the employment of anæsthesia in midwifery, on the ground of certain alleged indecencies committed by patients under its influence, Dr. Simpson observed,[2] that the special alleged objection to chloroform to which Dr. Cumming's remarks referred, was by no means a new objection. At the time when various of our now common articles of food and medicine were first introduced into use, these innovations were frequently opposed and decried on exactly the same ground. As a general rule, this was an argument always resorted to by weak and impure minds, when they could find no stronger arguments against any proposed innovation in our customs or habits. For instance, at the present day, no one imagines or argues that the eating of potatoes is liable to cause in those who eat them any indecency of word or action. And yet that was one of the principal arguments maintained by the unscrupulous against the use of potatoes for long after their introduction. In his "Traditions," Mr. Chambers states that, even as late as the last century, there was a prejudice against the potato for this, among other reasons—"that it was a provocative to incontinence." A hundred years after this, our successors in the profession will probably feel as much surprised at the idea of the use of chloroform exciting improper words and actions, as we are all now at the old idea of the use of potatoes being attended with the same alleged consequences. Dr. Simpson had now used upwards of 2000 oz. of chloroform, and had never witnessed any such effects, and never expected to witness them. In the circular letter of Mr. Gream to the English practitioners, to which Dr. Cumming alluded, Mr. Gream did not ask for any beneficial effects from chloroform that his correspondents might have seen; he asked only for the bad effects; he wanted to see only one side of the picture. And, doubtless, he would be gratified by some of the reports given him; for Dr. Simpson believed, from what he occasionally heard from some parts of England, that in many districts and towns there, the use of chloroform was still greatly misunderstood; a bad and dangerous article was often used under the name of chloroform; the article improperly exhibited; and no proper care and precaution used in its exhibition. But still the profession would take any report from Mr. Gream on the subject, *cum grano salis permagno.* In a former amusing pamphlet on the subject, Mr. Gream proceeded so far as to alter the official report in a case of death from chloroform at Aberdeen, in order to prove to his readers that those who used chloroform may have morbid adhesions of the heart to the pericardium. In the report given in to the Law or Government authorities in this case, it was stated that there existed "old adhesions" between the heart and pericardium which required to be forcibly torn asunder with the finger. Mr. Gream pretended to quote this report verbatim, and in inverted commas; but he carefully omitted the word "old," &c., for he wished to prove to the ladies who read his pamphlet that these pericardial adhesions were quite recent, and he *effects* of the chloroform. Our grandchildren will, no doubt, wonder at such pathological ratiocination, even with the omission of he word "old;" but at the present day it is calculated to teach us his circumstance, that one who could tamper with an official and ublic document for the purpose mentioned, will have little hesitaon in following the same course with any private documents inrusted to him.

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[1] From Proceedings of Edinburgh Obstetric Society, Feb. 14, 1849.

[2] See Edinburgh Monthly Journal of Medical Science, May, 1849, p. 767.

In Edinburgh, chloroform has now (1849) been exhibited, he elieves, to 40,000 or 50,000 persons, without a single accident or eleterious result, traceable to its use. Perhaps as many doses of pium, or antimony, or calomel, or Epsom salts even, or any other otent medicine, would not have been followed by results equally nocuous. Some two or three hundred die in England and Wales every year, of the effects of opium and other medicines, given in improper doses, or in particular idiosyncrasies; and when the proper use of chloroform came to be better understood, it would, Dr. S. believed, stand less high in that list than most other medicines used; while, besides saving pain, it would ultimately be found to save human life to no small extent in surgery and midwifery. One means by which its proper mode of employment would become more widely spread in a few years, was in consequence of our medical students, who daily saw its use, and the rules for using it, practised in the hospitals of Edinburgh and London, leaving their studies, and commencing practice with all the knowledge necessary for its employment, and without any of the fears naturally shown by those who were unacquainted with its phenomena. It had been often repeated that in Harvey's time no physician who was above forty years of age, would believe in the doctrine of the circulation; and new medical theories and practices were very seldom adopted by men beyond that term. Some beyond this age of conversion have argued and reasoned on the subject in the most extraordinary way. For instance, an old but excellent accoucheur, Dr. James Reid,[1] in a late discussion on the subject of anæsthesia, at the Westminster Society, stated that he had used chloroform in three midwifery cases, and that it had *not* relieved the pains of labor. Of course Dr. Reid's argument merely and simply showed one thing, that he was ignorant of the mode of giving it, for no one here or elsewhere, who knew anything of the matter, had met with such results. And the most strange results were often ascribed to it. In fact, if *anything* whatever happened to the mother or child for months, it mattered not what, it was by some of our English friends directly ascribed to chloroform. It produced all manner of medical, and some surgical diseases. If the child had a dislocation or fracture, the cause was the chloroform. The same gentleman, Dr. Reid, some time ago mentioned, at the same Society, the case of a child dead-born, in the practice of Dr. Smith, after a very long labor (forty or fifty hours), and where the mother was chloroformed. The child died in consequence of dislocation or fracture of the lower parts of the parietal bone against some projecting point or other in the interior of the maternal pelvis. But the death, Dr. Reid argued, was from chloroform; and as the immediate cause of death was the above, hence chloroform could produce dislocation or fracture, and a hundred deleterious results besides. He (Dr. S.) had heard it accused, on equally logical grounds, of being the cause of a monstrosity on the part of the fœtus, when it happened to be given at the birth of the malformed child. Certainly not many years would elapse ere the profession must see the weakness and folly of this mode of argument. But, as it stands, no newly introduced

[1] Lancet, January 27, 1849.

practice had ever, he believed, made so great and steady progress in so short a time as anæsthetic midwifery; and of its ultimate and entire success and adoption everywhere, no one here, where all witnessed and practised it, had any kind of doubt.

CHAPTER XIII.

REMARKS ON THE OCCASIONAL FATAL RESULT OF THE ADMINISTRATION OF CHLOROFORM.

(Letter to the Editor of the Medical Times and Gazette, June 19, 1852.)

SIR—In the last number of the Medical Times and Gazette, Dr. Snow asks me to state the cases of death from chloroform which have occurred in Scotland.

He mentions two cases, both at or near Glasgow, one under the operation of extracting a toe-nail, the other while sounding for stone. I do not know any particulars whatever as to the former case. The common current account of the latter is given in the last number of the Monthly Medical Journal, under some observations on chloroform, to which I beg to refer your correspondent.[1]

In addition to these two cases, I have heard only of one more instance of a fatal result of the employment of chloroform in surgery, and that also near Glasgow. In this instance chloroform was given by the practitioner for tooth extraction; but, I am sorry to add, none of the parties present were at the time in a condition to give any very satisfactory evidence.

These are the only cases, so far as I know, of death in Scotland from chloroform, among the many thousand cases in which it has now been exhibited in connection with surgical proceedings; and I believe, that if any others had occurred, I should have been certain to have heard of them.

Before chloroform was introduced, sulphuric ether was reported to have produced the death of one patient in Scotland, who had amputation of the thigh performed, in consequence of a severe railway injury. One of the surgeons, however, afterwards assured me, that in the dark hovel in which the operation was performed, it was found impossible to secure the vessels sufficiently quickly with the ligatures to prevent the collapsed patient dying from hemorrhage after the leg was removed; but the ether was a better apology than the hemorrhage for the man's sudden sinking.

[1] Monthly Journal of Medical Science, 1852, p. 554.

I know of one medical patient who died south of the Tweed, when using, or shortly after using, chloroform, to procure sleep in *delirium tremens.* But, as Dr. Snow is well aware, death often enough occurs suddenly in that disease; and the last thing done is always apt to be blamed for the result. A medical friend of mine had, some time ago, a patient suffering under *delirium tremens.* Opiates, &c., had all failed to produce quietude, or induce sleep. At last, the practitioner, who was watching by his patient, went home to his own house, which was in the immediate neighborhood, to obtain some chloroform for the purpose of using it as a hypnotic. During the few minutes of his absence in search of the chloroform-bottle, his patient died.

A somewhat similar coincidence happened at the very first introduction of chloroform here. After discovering the anæsthetic effects of chloroform, I was, of course, anxious to get it tried in a surgical operation. The first surgical cases in which it was used, were operated upon in the Royal Infirmary here, on 15th November, 1847. Two days previously, an operation took place in the Infirmary, at which I could not be present, to test the power of chloroform; and, so far, fortunately so; for the man was operated upon for hernia, without any anæsthetics, and suddenly died after the first incision was made through the skin, and with the operation uncompleted. I know of another case in Edinburgh, where death instantaneously followed the use of an abscess-lancet, without chloroform; the practitioner, in fact, deeming the case too slight to require any anæsthetic.

While, since the anæsthetic effects of chloroform were known, we have thus had two patients dying in Edinburgh on the operating-table, who did not use chloroform, we have had no death under the same circumstances in the vast number who have now taken chloroform here for surgical operations and other purposes.

Some time ago, I was informed of an instance in which a practitioner urged a patient to use chloroform for the purpose of allowing a tooth to be removed that had worn her out with pain. She postponed it for a few hours; and, in the meantime, went to bed to procure, if possible, some rest. On going to her room an hour or two subsequently, she was found dead. In this, as in other similar instances, the reputation of chloroform had a narrow escape.

In the different discussions that have taken place, in this country and on the Continent, regarding chloroform as the supposed cause of death in various surgical patients, it appears to me, that it has very generally been forgotten, that patients have ever and anon died during, or immediately after, operations, long before the time of the use of any anæsthetics. But, when such cases occur now, and anæsthetics do happen to be employed in them, the latter are always naturally, though perhaps not always justly, blamed. I have already alluded to two such cases which have happened in Edinburgh since the time chloroform was known here; and doubtless, if chloroform had been employed in these cases, it would have been taxed with the fatal result. Shortly before the first of these cases, my friend, Dr. Robertson, had shaved the groin of a patient, and was about to proceed to perform the operation for hernia, when the patient fainted, and died before any incision was made. I have been told of various cases by other surgeons, where the patient died on the operating-table before the days of ether and chloroform, and where the result now would be considered by many as the palpable and indisputable effect of any anæsthetic the patient might chance to use.

In making this remark, I do not of course, by any means, wish to argue, that chloroform may not, and has not proved fatal when used in surgical operations. Nothing could be further from my thoughts or intentions. In the very first paper which I published on the subject, in the Monthly Medical Journal, I attempted to warn my professional brethren, that too great or too long a dose "would doubtless produce serious consequences, and even death;" and at the same time I ventured to hope, that "its great potency would be one great safeguard against its abuse."[1]

The druggists of Edinburgh have sold, I believe, during the last four or five years, as much chloroform to the medical practitioners and to the public of this city, as would produce anæsthesia in one or two hundred thousand separate instances; and, indeed, most of the practitioners here use it daily for diagnostic and other purposes, surgical, obstetrical, and medical.

I am sure you will doubt with me, whether an equal amount of full doses of antimony, or of opium, or even of Epsom salts, would not have been followed by a greater number of deaths, occurring perhaps more slowly, but, probably, also more surely. And, on the contrary, this agent, while, like all other medicinal agents, proving njurious now and again in a rare exceptional case, has, I rejoice to hink, been already the means of saving, during the last few years, a vast amount of human suffering, and by that means no small amount also of human life.

I am not aware of any death in Scotland or elsewhere from the use of chloroform in midwifery, out of the many thousand cases in which it has now been employed in the old and new world. Nor, indeed, does the obstetric patient run anything like the risk of the surgical patient; for, in midwifery, though the anæsthetic is required to be given for a far longer period, it does not require to be given so deeply as in surgery.

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[1] See a subsequent page.

Since November, 1847, I have only attended twelve cases of labor in which chloroform was not used during delivery. In all my other cases I have employed it; and none of those patients, I venture to say, who have used it, would again choose to suffer the unnecessary pains attendant upon labor without it. Most of my obstetric brethren employ it as frequently as I do. After once beginning its use in an obstetric case, I generally leave its exhibition to be continued by the nurse, or by any intelligent friend of the patient who may be in the room. Some of our midwives use it in the cases which they themselves attend. Two weeks ago one of them told me that she had now employed it herself in her own practice in above fifty cases, with nothing but the happiest results, and without meeting with any circumstances to give her the slightest feeling of uneasiness in its employment.

Chloroform is manufactured to a large extent by three or four chemical establishments in Edinburgh; and as its high price in England has been, I believe, one great cause of its slow introduction into general practice among you, let me state that with us it is usually sold wholesale to apothecaries at six or seven shillings per pound weight; and it is retailed to medical men at eightpence, and to the general public at one shilling the ounce per weight. It ought to be as cheap in the South.

I am not aware that in Scotland, where chloroform is employed so very extensively and so very successfully by medical men and others, it is ever exhibited by any one except on a handkerchief, towel, or the like; no kind of formal apparatus is used. Doubtless, one principal point consists in diluting its vapor sufficiently freely with air, and this can always be readily accomplished when the handkerchief is employed. Some American and English practitioners have proposed to render the vapor of chloroform less strong by diluting it when used with the vapor of sulphuric ether, or of alcohol, as in the so-called chloric ether. But alcohol, &c., often leave headache and excitement, which chloroform does not. And it is surely a thousand-fold better to dilute it with the vapor of "common atmospheric air," than with any other diluting medium.

Perhaps I should add, that twice or thrice chloroform has been used in Scotland by medical men and others to commit suicide; but, of course, Dr. Snow's inquiry does not extend to these cases of its deliberate abuse. He alludes to one of those cases, in which a druggist at Aberdeen destroyed himself by "breathing chloroform for amusement when no one was present;" and he proceeds to speak of a fatal accident "from this proceeding" as a "matter of course." I am not aware of any other similar case in Scotland.

I am, &c. J. Y. Simpson.

SECTION III.

ON THE NATURE AND POWERS OF VARIOUS ANÆSTHETIC AGENTS.

CHAPTER I.

ACCOUNT OF A NEW ANÆSTHETIC AGENT AS A SUBSTITUTE FOR SULPHURIC ETHER IN SURGERY AND MIDWIFERY.[1]

"I esteem it the office of a physician, not only to restore health, but to mitigate pain and dolors."—BACON.

FROM the time at which I first saw ether-inhalation successfully practised in January last, I have had the conviction impressed upon my mind, that we should ultimately find that other therapeutic agents were capable of being introduced with equal rapidity and success into the system, through the same extensive and powerful channel of pulmonary absorption. In some observations, which I wrote and published in March last, relative to the inhalation of sulphuric ether in midwifery, I stated that, in several obstetric cases, I had used ergot of rye in this way, along with ether.[2]

With various professional friends, more conversant with chemistry than I am, I have, since that time, taken opportunities of talking over the idea which I entertained of the probable existence or discovery of new therapeutic agents, capable of being introduced into the system by respiration, and the possibility of producing for inhalation vaporizable or volatile preparations of some of our more active and old-established medicines: and I have had, during the summer and autumn, ethereal tinctures, &c., of several potent drugs, manufactured for me, for experiment, by Messrs. Duncan, Flockhart, and Co., the excellent chemists and druggists of this city.

Latterly, in order to avoid, if possible, some of the inconveniences and objections pertaining to sulphuric ether—(particularly its disagreeable and very persistent smell, its occasional tendency to irritation of the bronchi during its first inspirations, and the large quantity of it occasionally required to be used, more especially in protracted cases of labor)—I have tried upon myself and others the inhalation of other different volatile fluids, with the hope that some one of them might be found to possess the advantages of ether without its disadvantages. For this purpose, I selected for experiment, and have inhaled, several chemical liquids of a more fragrant or agreeable odor, such as the chloride of hydro-carbon (or Dutch liquid), acetone, nitrate of oxide of ethyle (nitric ether), benzin, the vapor of iodoform, &c.[1] I have found, however, one infinitely more efficacious than any of the others, viz., Chloroform, or the Perchloride of Formyle, and I am enabled to speak most confidently of its superior anæsthetic properties, having now tried it upon upwards of thirty individuals. The liquid I have used has been manufactured for me by Mr. Hunter, in the laboratory of Messrs. Duncan, Flockhart, and Co.

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[1] Communicated to Edinburgh Medico-Chirurgical Society, 11th Nov. 1848.

[2] See a previous page; also for successful inhalation of opium to arrest the vomiting of pregnancy, see vol. i. p. 315.

Chloroform was first discovered and described at nearly the same time by Soubeiran (1831) and Liebig (1832); its composition was first accurately ascertained by the distinguished French chemist, Dumas, in 1835.[2] It has been used by some practitioners internally; Guillot prescribed it as an antispasmodic in asthma, exhibiting it in small doses, and diluted 100 times.[3] But no person, so far as I am aware, has used it by inhalation, or discovered its remarkable anæsthetic properties till the date of my own experiments.

It is a dense, limpid, colorless liquid, readily evaporating, and possessing an agreeable, fragrant, fruit-like odor, and a saccharine pleasant taste.

As an inhaled anæsthetic agent, it possesses over sulphuric ether the following advantages:—

1. A greatly less quantity of chloroform than of ether is requisite to produce the anæsthetic effect; usually from a hundred to a hundred and twenty drops of chloroform only being sufficient; and with some patients much less. I have seen a strong person rendered completely insensible by six or seven inspirations of thirty drops of the liquid.

[1] In talking over, with different chemists, what fluids might be sufficiently volatile to be respirable, and hence deserving of being experimented upon, Mr. Waldie first named to me the perchloride of formyle, as worthy, among others, of a trial;—Dr. Gregory suggested a trial of the chloride of hydrocarbon, &c. I have been deeply indebted to Dr. Gregory and Dr. Anderson, for their kindness in furnishing me with the requisite chemical agents for these experiments; and also to my assistants, Dr. Keith and Dr. Duncan, for the great and hearty zeal with which they have constantly aided me in conducting the inquiry.

[2] See the Annales de Chimie et de Physique, vols. xlviii, xlix, and lviii.

[3] See Bourchardat's Annuaire de Therapeutique, for 1844, p. 35.

2. Its action is much more rapid and complete, and generally ore persistent. I have almost always seen from ten to twenty full spirations suffice. Hence the time of the surgeon is saved; and at preliminary stage of excitement, which pertains to all narcotizg agents, being curtailed, or indeed practically abolished, the tient has not the same degree of tendency to exhilaration and lking.[1]

3. Most of those who know from previous experience the sensaons produced by ether inhalation, and who have subsequently reathed chloroform, have strongly declared the inhalation and inuence of chloroform to be far more agreeable and pleasant than ose of ether.

4. I believe that, considering the small quantity requisite, as mpared with ether, the use of chloroform will be less expensive an that of ether; more especially, as there is every prospect that e means of forming it may be simplified and cheapened.

5. Its perfume is not unpleasant, but the reverse; and the odor f it does not remain for any length of time, obstinately attached the clothes of the attendant—or exhaling in a disagreeable form om the lungs of the patient, as so generally happens with sulphuric her.

6. Being required in much less quantity, it is much more portble and transmissible than sulphuric ether.

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[1] In practice I have found that any such tendency, even with ether, is avoided by—1st, ving the patient from the first a large and overwhelming dose of the vapor, and 2dly, by eping him perfectly quiet and still, and preventing all noise and talking around him. I ve elsewhere insisted on the importance of these points. (See the numbers of the Monthly urnal of Medical Science, for March, 1847, p. 726, and for September, p. 154.) In the pers last referred to, I took occasion, when discussing the conditions requisite for insuring ccessful etherization, to observe, "*First*, The patient ought to be left, as far as possible, a state of absolute quietude and freedom from mental excitement, both during the inducon of etherization, and during his recovery from it. All talking and all questioning should strictly prohibited. In this way any tendency to excitement is eschewed, and the proper fect of the ether inhalation more speedily and certainly induced. And *Secondly*, with the me view, the primary stage of exhilaration should be entirely avoided, or at least reduced the shortest possible limit, by impregnating the respired air as fully with the ether vapor, the patient can bear, and by allowing it to pass into the lungs both by the mouth and strils, so as rapidly and at once to superinduce its complete and anæsthetic effect; * * * very common but certainly a very unpardonable error being to exhibit an imperfect and citing, instead of a perfect and narcotizing dose of the vapor. Many of the alleged ilures and misadventures are doubtless entirely attributable to the neglect of this simple le;—not the principle of etherization, but the mode of putting it in practice being altogether blame. But, *Thirdly*, whatever means or mode of etherization is adopted, the most imortant of the conditions required for procuring a satisfactory and successful result from its nployment in surgery, consists in obstinately determining to avoid the commencement of e operation itself, and never venturing to apply the knife until the patient is under the *full* fluence of the ether vapor, and *thoroughly and indubitably soporized by it.*" In fulfilling ll these indications, the employment of chloroform evidently offers great and decided adantages, in facility and efficiency, over the employment of ether.

7. No special kind of inhaler or instrument is necessary for its exhibition. A little of the liquid diffused upon the interior of a hollow-shaped sponge, or a pocket-handkerchief, or a piece of linen or paper, and held over the mouth and nostrils, so as to be fully inhaled, generally suffices in about a minute or two to produce the desired effect.[1]

I have not yet had an opportunity of using chloroform in any capital surgical operation, but have exhibited it with perfect success in tooth-drawing,[2] opening abscesses, for annulling the pain of dysmenorrhœa, and of neuralgia, and in two or three cases where I was using deep and otherwise very painful galvano-puncture for the treatment of ovarian dropsy, &c. I have employed it also in obstetric practice with entire success. The lady to whom it was first exhibited during parturition, had been previously delivered in the country by perforation of the head of the infant, after a labor of three days' duration. In this, her second confinement, pains supervened a fortnight before the full time. Three hours and a half after they commenced, and ere the first stage of the labor was completed, I placed her under the influence of the chloroform, by moistening, with half a teaspoonful of the liquid, a pocket-handkerchief, rolled up into a funnel shape, and with the broad or open end of the funnel placed over her mouth and nostrils. In consequence of the evaporation of the fluid, it was once more renewed in about ten or twelve minutes. The child was expelled in about twenty-five minutes after the inhalation was begun. The mother subsequently remained longer soporose than commonly happens after ether. The squalling of the child did not, as usual, rouse her; and some minutes elapsed after the placenta was expelled, and after the child was removed by the nurse into another room, before the patient awoke. She then turned round to me and observed that she had "enjoyed a very comfortable sleep, and indeed required it, as she was so tired,[1] but would now be more able for the work before her." I evaded entering into conversation with her, believing, as I have already stated, that the most complete possible quietude forms one of the principal secrets for the successful employment of either ether or chloroform. In a little time she again remarked that she was afraid her "sleep had stopped the pains." Shortly afterwards, her infant was brought in by the nurse from the adjoining room, and it was a matter of no small difficulty to convince the astonished mother that the labor was entirely over, and that the child presented to her was really her "own living baby."

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[1] When used for surgical purposes, perhaps it will be found to be most easily given upon a handkerchief, gathered up into a cup-like form in the hand of the exhibitor, and with the open end of the cup placed over the nose and mouth of the patient. For the first inspiration or two, it should be held at the distance of half an inch or so from the face, and then more and more closely applied to it. To insure a rapid and perfect anæsthetic effect—more especially where the operation is to be severe—one or two teaspoonfuls of the chloroform should be at once placed upon the hollow of the handkerchief, and immediately held to the face of the patient. Generally a snoring sleep speedily supervenes; and when it does so, it is a perfect test of the superinduction of complete insensibility. But a patient may be quite anæsthetic without this symptom supervening.

[2] A young dentist who has himself had two teeth extracted lately—one under the influence of ether, and the other under the influence of chloroform—writes me the following statement of the results:—"About six months ago I had an upper molar tooth extracted whilst under the influence of ether, by Mr. Imlach. The inhalation was continued for several minutes before I presented the usual appearance of complete etherization; the tooth was then extracted; and although I did not feel the least pain, yet I was conscious of the operation being performed, and was quite aware when the crash took place. Some days ago I required another molar extracted on account of toothache, and this operation was again performed by the same gentleman. I inhaled the vapor of chloroform, half a drachm being poured upon a handkerchief for that purpose, and held to my nose and mouth. Insensibility took place in a few seconds; but I was so completely *dead* this time, that I was not in the very slightest degree aware of anything that took place. The subsequent stupefying effects of the chloroform went off more rapidly than those of the ether; and I was perfectly well and able again for my work in a few minutes."

Perhaps I may be excused for adding, that since publishing on the subject of Ether Inhalation in Midwifery, some time ago,[2] and then for the first time directing the attention of the medical profession to its great use and importance in natural and morbid parturition, I have employed it, with few and rare exceptions, in every case of labor that I have attended; and with the most delightful results. And I have no doubt whatever, that some years hence the practice will be general. Obstetricians may oppose it, but I believe our patients themselves will force the use of it upon the profession.[3] I have never had the pleasure of watching over a series of better and more rapid recoveries; nor once witnessed any disagreeable result follow to either mother or child; whilst I have now seen an immense amount of maternal pain and agony saved by its employment. And I most conscientiously believe that the proud mission of the physician is distinctly twofold—namely, to alleviate human suffering, as well as preserve human life.

CHEMICAL CONSTITUTION OF CHLOROFORM.

Formyle is the hypothetical radical of formic acid. In the red ant (*Formica rufa*) formic acid was first discovered, and hence its name. Gehlen pointed it out as a peculiar acid; and it was afterwards first artificially prepared by Doebereiner. Chemists have now devised a variety of processes, by which formic acid may be obtained from starch, sugar, and, indeed, most other vegetable substances.

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[1] In consequence of extreme anxiety at the unfortunate result of her previous confinement, she had slept little or none for one or two nights preceding the commencement of her present accouchement.

[2] See Monthly Journal of Medical Science for February, 1847, p. 639; for March, pp. 718 and 721; and April, p. 794, &c.

[3] I am told that the London physicians, with two or three exceptions only, have never yet employed ether inhalation in their midwifery practice. Three weeks ago I was informed in a letter from Professor Montgomery of Dublin, that he believed that in that city, up to that date, it had not been used in a single case of labor.

A series of chlorides of formyle are produced when chlorine and the hypochlorites are brought to act on the chloride, oxide, and hydrated oxide of methyle (pyroxylic or wood spirit). In the same way as formic acid may be artificially procured from substances which do not contain formyle ready formed—so also are the chlorides of this radical capable of being procured from substances which do not originally contain it.

Chloroform, chloroformyle, or the perchloride of formyle, may be made and obtained artificially by various processes—as by making milk of lime, or an aqueous solution of caustic alkali, act upon chloral—by distilling alcohol, pyroxylic spirit, or acetone, with chloride of lime—by leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c. The preparation which I have employed was made according to the following formula of Dumas:

"℞ Chloride of lime in powder, ...	℔. iv.
Water, ...	℔. xii.
Rectified Spirit, ...	f ℥ xii.

"Mix in a capacious retort or still, and distil as long as a dense liquid, which sinks in the water with which it comes over, is produced."[1]

The resulting perchloride of formyle consists of two atoms of carbon, one of hydrogen, and three of chlorine. Its specific gravity is much greater than that of water, being as high as 1·480. It boils at 141°. The density of its vapor is 4·2. It is not inflammable; nor changed by distillation with potassium, potash, sulphuric, or other acids.[2]

It is now well ascertained that three compound chemical bodies possess, when inhaled into the lungs, the power of superinducing a state of anæsthesia, or insensibility to pain in surgical operations, &c., namely, nitrous oxide, sulphuric ether, and perchloride of formyle. The following tabular view shows that these agents are entirely different from each other in their chemical constitution, and hence that their elementary composition affords no apparent clue to the explanation of their anæsthetic properties:

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[1] Gray's Supplement to the Pharmacopœia, 1816, p. 633.

[2] See Turner's Elements of Chemistry, 8th edition, p. 1009; Gregory's Outlines of Chemistry, part ii. p. 401; Fownes' Manual of Elementary Chemistry, p. 419; Thomson's Chemistry of Organic Bodies, p. 312; Loewig's Organische Chemie, vol. i. p. 498.

	Propor. of Nitrogen.	Propor. of Oxygen.	Propor. of Carbon.	Propor. of Hydrogen.	Propor. of Chlorine.
Nitrous Oxide, .	1 Atom.	1 Atom.	....	....	....
Sulphuric Ether, .	....	1 Atom.	4 Atoms.	5 Atoms.	....
Chloroform, . .	....	....	2 Atoms.	1 Atom.	3 Atoms.

It is perhaps not unworthy of remark, that when Soubeiran, iebig, and Dumas engaged, a few years back, in those inquiries nd experiments by which the formation and composition of chlororm was first discovered, their sole and only object was the invesgation of a point in philosophical chemistry. They labored for the ure love and extension of knowledge. They had no idea that the bstance to which they called the attention of their chemical rethren could or would be turned to any *practical* purpose, or that possessed any physiological or therapeutic effects upon the animal conomy. I mention this to show, that the *cui bono* argument gainst philosophical investigations, on the ground that there may e at first no apparent practical benefit to be derived from them, as been amply refuted in this, as it has been in many other intances. For I feel assured, that the use of chloroform will soon ntirely supersede the use of ether; and, from the facility and pidity of its exhibition, it will be employed as an anæsthetic gent in many cases, and under many circumstances, in which ether ould never have been had recourse to. Here then we have a subance which, in the first instance, was merely interesting as a atter of scientific curiosity and research, becoming rapidly an bject of intense importance, as an agent by which human suffering d agony may be annulled and abolished, under some of the most ying circumstances in which human nature is ever placed.

Postscript.—Since the above observations were sent to the press, have, through the great kindness of Professor Miller and Dr. uncan, had an opportunity of trying the effects of the inhalation chloroform, to-day, in three cases of operation in the Royal Inrmary of Edinburgh. A great collection of professional gentleen and students witnessed the results, and among the number as Professor Dumas of Paris, the chemist who first ascertained d established the chemical composition of chloroform. He hapened to be passing through Edinburgh, engaged along with Dr. ilne Edwards, who accompanied him, in an official investigation r the French government, and was in no small degree rejoiced to itness the wonderful physiological effects of a substance with hose chemical history his own name was so intimately connected. I append notes obligingly furnished to me by Professor Miller and Dr. Duncan, of three cases of operation. The first two cases were operated on by Professor Miller; the third by Dr. Duncan. In applying the chloroform in the first case, I used a pocket-handkerchief as the inhaling instrument; in the last two I employed a hollow sponge.

Case I.—"A boy, four or five years old, with necrosis of one of the bones of the fore-arm. Could speak nothing but Gaelic. No means, consequently, of explaining to him what he was required to do. On holding a handkerchief, on which some chloroform had been sprinkled, to his face, he became frightened, and wrestled to be away. He was held gently, however, by Dr. Simpson, and obliged to inhale. After a few inspirations he ceased to cry or move, and fell into a sound snoring sleep. A deep incision was now made down to the diseased bone, and, by the use of the forceps, nearly the whole of the radius, in the state of sequestrum, was extracted. During this operation, and the subsequent examination of the wound by the finger, not the slightest evidence of the suffering of pain was given. He still slept on soundly, and was carried back to his ward in that state. Half an hour afterwards, he was found in bed, like a child newly awakened from a refreshing sleep, with a clear merry eye, and placid expression of countenance, wholly unlike what is found to obtain after ordinary etherization. On being questioned by a Gaelic interpreter, who was found among the students, he stated that he had never felt any pain, and that he felt none now. On being shown his wounded arm, he looked much surprised, but neither cried nor expressed the slightest alarm."

Case II.—"A soldier who had an opening in the cheek—the result of exfoliation of the jaw—was next made to inhale. At first he showed some signs of moving his hands too freely; but soon also fell into a state of sleep and snoring. A free incision was made across the lower jaw, and from this the dense adhering integuments were freely dissected all round, so as to raise the soft parts of the cheek. The edges of the opening were then made raw, and the whole line of incision was brought together by several points of suture. This patient had previously undergone two minor operations of a somewhat similar kind; both of them had proved unsuccessful, and he bore them very ill—proving unusually unsteady, and complaining bitterly of severe pain. On the present occasion, he did not wince or moan in the slightest degree; and, on the return of consciousness, said that he had felt nothing. His first act, when apparently about half awake, was suddenly to clutch up the sponge with which the chloroform was used, and readjust it to his

›uth, obviously implying that he had found the inhalation from ınything but a disagreeable duty.

"This case was further interesting as being one of those opera- ns in the region of the mouth, in which it has been deemed im- ssible to use ether—and certainly it would have been impossible have performed the operation with any complicated inhaling paratus applied to the mouth of the patient."

Case III.—"A young man of about twenty-two years of age, ving necrosis of the first phalanx of the great toe, and ulceration the integuments, the consequence of injury. The ulcerated sur- :e was exceedingly tender to the touch—so much so, that he nced whenever the finger was brought near to it; and the ghtest pressure made him cry out. After the removal of the essings, which caused some pain and fretting, the inhalation was mmenced, and the patient almost immediately[1] became insensible, d lay perfectly still, while the diseased mass was being removed by ıputation of the toe through the middle of the second phalanx. ıe inhalation was now stopped. The edges of the wound were en brought together with three stitches, and the wound dressed. ıe patient shortly afterwards awoke, looked round him, and atefully declared his entire and perfect freedom from all pain and easiness during the operation."

The whole quantity of chloroform used in these three operations l not exceed half an ounce—and, as Professor Miller afterwards served to the students that were present, if ether had been used reral ounces of it would have been requisite to produce the same ıount of anæsthetic effect.

The following case occurred also to-day, to Mr. Miller, in private ıctice. The notes of it and the subsequent remark are in his own ›rds.

Case IV.—"A young lady wished to have a tumor (encysted) ısected out from beneath the angle of the jaw. The chloroform ıs used in small quantity (about a drachm) sprinkled upon a piece operation sponge. In considerably less than a minute she was und asleep, sitting easily in a chair, with her eyes shut, and with r ordinary expression of countenance. The tumor was extirpated, d a stitch inserted, without any pain having been either shown felt. Her sensations throughout, as she subsequently stated, d been of the most pleasing nature; and her manageableness ıring the operation was as perfect as if she had been a wax doll or lay figure.

Dr. Christison, who was watching the result, informs me that this patient was affected half a minute.

"No sickness, vomiting, headache, salivation, uneasiness of chest, in any of the cases. Once or twice a tickling cough took place in the first breathings."

I have, up to this date, exhibited the chloroform to about fifty individuals. In not a single instance has the slightest bad result of any kind whatever occurred from its employment.

EDINBURGH, 15th November, 1847.

CHAPTER II.

ANÆSTHETIC AND OTHER THERAPEUTIC PROPERTIES OF CHLOROFORM.[1]

AT the first winter meeting of the Edinburgh Medico-Chirurgical Society, 10th November, I directed the attention of the members to a new respirable anæsthetic agent which I had discovered a short time previously, viz., Chloroform, Chloroformyle, or Perchloride of Formyle. In this limited notice, I shall state briefly some of the principal facts pertaining to its history, composition, effects, &c.

Chemical History and Composition.—Chloroform was discovered at nearly the same time by Soubeiran (1831), and Liebig (1832). Its chemical composition was first ascertained by Dumas and Peligot (1835). It consists of 2 atoms of carbon, 1 of hydrogen, and 3 of chlorine; or, to express it otherwise, of 1 atom of formyle, and 3 of chlorine. Hence its chemical formula is C_2HCl_3; or $FoCl_3$.

Modes of Preparation.—It may be obtained by various processes. 1. By the distillation of a mixture of diluted spirit, pyroxylic or wood spirit, or acetone, and chloride of lime (bleaching powder); or 2. By making milk of lime, or an aqueous solution of caustic alkali, act upon chloral; 3. By leading a stream of chlorine gas into a solution of caustic potass in spirit of wine, &c.

Physical and Chemical Properties.—It is a clear, limpid liquid, as heavy as 1·480; not inflammable; very volatile; and boils at 141°. It has a fragrant, fruit-like odor; and a sweet saccharine taste.

Therapeutic History.—It has been used internally. Guillot employed it in asthma, diluted with water one hundred times (1844). My friend, Dr. Formby, of Liverpool, told me, about two years ago, that he used it often in a diluted form as a diffusible stimulant; and I have, since that period, frequently prescribed it instead of valerian, camphor, &c.[2] But I am not aware that any person has used chloroform by inhalation, or discovered its remarkable anæsthetic properties, till the date of my own experiments.

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[1] From Edinburgh Monthly Journal of Medical Science, December, 1847, p. 415.

[2] Since first publishing on the subject of chloroform, Dr. Glover, of Newcastle, has pointed out to me, that, in an essay on Bromine, in the 152d number of the Edinburgh Medical and

Advantages as an Anæsthetic Agent.—In producing insensibility to pain in surgical and obstetric practice, chloroform possesses various important advantages over sulphuric ether. 1. A greatly less quantity of chloroform is required; 2. Its action is much more rapid, more perfect, and generally more persistent; 3. Its exciting or exhilarating stage is far shorter, insensibility commonly supervening in a minute or two, or less; hence, 4. The time of the surgeon is saved; 5. The inhalation and influence of it are more agreeable and pleasant; 6. Its odor is evanescent; 7. No special instrument is required for its employment.

Dose and Mode of Exhibition.—A fluid drachm or two of the liquid, diffused upon the interior of a pocket-handkerchief, arranged in a concave or cuplike form in the hand of the exhibitor, and applied over the nose and mouth of the patient, generally suffices to produce rapid and complete anæsthesia. A few patients may require more, others less. The *edges* of the cup or cone are not to be wetted, or the patient's face will be irritated. To keep up its action, when that is necessary, the handkerchief must be again besprinkled with the fluid when the first quantity is evaporated. The moistened handkerchief should be at first held at the distance of about half an inch from the face, and gradually approached nearer. The patient should, if possible, be placed easily and upon his back, and advised previously to take full inspirations. All noises and excitement around the patient should be strictly and peremptorily forbidden.[1]

Physiological Effects.—After the first two or three full inspirations, feeling of warmth and excitation, radiating from the chest to the xtremities; followed by whirring noises in the ears; a sensation f vibratory thrilling and benumbing throughout the body; with, etimes, rapid loss of sensation and of motion, and, at last, of conciousness. Often before total unconsciousness supervenes, the atient, guided by instinct rather than by volition and reason, akes an effort to get rid of the inhaling vapor and handkerchief, s if it interfered with free respiration. This temporary effort must e resisted by the exhibitor. During the full anæsthetic sleep pro-

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urgical Journal, he mentions having poisoned several animals with chloroform, by injectg it into their bloodvessels, stomach, and the cavity of the peritoneum, and has investigated s physiological mode of action.

[1] Nothing can be more absurd than the way in which some dentists proceed. The uscles of the jaws often close under the use of chloroform and ether; and they try to open e mouth partly by persuasion, partly by force—irritating and rousing the patient. A cork r gag placed between the teeth, *before* the inhalation is commenced, saves all this, and xpedites and facilitates the whole process.

duced by chloroform, sometimes no mental action goes on, o least is remembered; in many others, the mind is active a dreams. The respiration is usually at first soporose; the p sometimes natural, in others slightly contracted, in others dila The pulse is usually quickened ten or twenty beats at first, afterwards falls to its normal rate, and if the vapor is exhibited long in very powerful doses, it comes down more and more b the natural standard; muscles of voluntary motion in genera laxed; more rarely cataleptic; still more rarely clonically contra as happens also occasionally with ether.

In small doses, given slowly, its effects are exhilarating, an actly like those generally following the inhalation of nitrous gas. Of course, when exhibited in this way, the patient is in a of excitement quite unfit for a surgical operation. When give surgical operations, it should be exhibited rapidly in large and the patient sent over into a deep soporose or stertorous *before* the incisions are begun.[1]

Uses in Surgery.—1. To relax the muscles in reducing di tions, &c.; 2. To avert the sufferings attendant on deep pro and other painful but necessary modes of diagnostic examin and dressing; and 3. and principally, To annul the pain of o tions by the caustic, ligature, or knife.

Examples.—I. A child of ten weeks old had a very large n behind the ear. Dr. Duncan destroyed its internal organizatio passing large red hot needles in different directions throug While the tumor was hissing and decomposing under their a the infant lay quietly and placidly asleep on my knee, unde influence of chloroform. This is the youngest subject to wh have given it. II. A boy of four or five had a necrosed radiu down upon and removed by Mr. Miller. He slept soundly d the operation; and, without moving, he was carried out o operation theatre of the hospital—still fast asleep. When v some time afterwards, he was found awake in bed, with a b merry eye, as if just out of a refreshing sleep. No pain even III. A nervous woman, a patient of Mr. Miller's, was to un partial amputation of the foot in the hospital—afraid both o operation, and of being carried in before a crowd of medical for the purpose. I apathized her with chloroform in the cons room of the hospital, and had her carried into the operation in that state, and did not allow her to awake till the amput was performed, and she was removed back again to bed. Sh thus entirely spared both the moral shock and physical pain which he dreaded. IV. A boy had his elbow-joint excised by Mr. Syme. The operation, which is always a very painful one, was prolonged n consequence of the very diseased state of the parts operated on. He slept soundly, and remained perfectly and passively still during he whole operation, &c. &c.

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[1] I believe all the reputed failures and misadventures are attributable to two caus 1. Using an impure and imperfect variety of chloroform; and, 2. Not giving it in suf large and rapid doses.

Uses in Midwifery.—To diminish and annul the physical pains ttendant on labor, and more especially those which accompany the assage of the child's head through the pelvic cavity and outlet—the second stage of Denman).

Examples.—I. The lady to whom it was first exhibited had been reviously delivered in the country by craniotomy, after a very long abor. Her second confinement took place a fortnight before the ull time. Chloroform was begun to be inhaled when the os uteri was becoming well expanded, and the pains very severe. In twenty-ive minutes the child was born. The mother did not awake till fter the placenta was removed; and was perfectly unaware that her child was born and alive. She stated her sensations to be those f awaking from "a very comfortable sleep." II. I exhibited it, with Mr. Carmichael, to a patient who had, at her preceding con-inement, been in severe labor for twenty hours—followed by flood-ng. She began the inhalation when the dilatation of the os uteri was half completed. The child was born in fifty minutes afterwards. he was kept under its influence for a quarter of an hour longer, ll the placenta was removed, and the binder, body, and bed-clothes ll adjusted. On awaking, she declared she had been sleeping re-eshingly; and was quite unconscious that the child was born. No ooding. An hour afterwards, she declared she felt perfectly un-tigued, and not as if she had borne a child at all. III. Patient nmarried. A first labor. Twins. The first child presented by e pelvis; the second with the hand and head. The chloroform as exhibited when the os uteri was fully dilated. The passages eedily became greatly relaxed (as has happened in other cases laced under its influence), and in a few pains first child was born, ssisted by some traction. I broke the membranes of the second, ushed up the hand, and secured the more complete presentation f the head. Three pains expelled the child. The mother was hen bound up; her clothes were changed; and she was lifted into nother bed. During all this time she slept on soundly, and for a ull hour afterwards; the chloroform acting in this, as in other cases f its prolonged employment, as a soporific. The patient recol-ected nothing from the time of the first inhalations, and was reatly distressed when not one, but two living children were rought by the nurse to her. Dr. Christison saw this case with me. I have used it in several operative deliveries with similar success.

In labor it does not require to be given in such large doses as in surgery. After the first *full* dose, a few inhalations before each returning uterine contraction is generally sufficient. It should be made more deep as the head is passing the perineum and vulva. If the state is extremely and unnecessarily deep, it will no doubt diminish and even temporarily stop uterine contractions; and I have taken advantage of this, in one case, to facilitate the operation of turning, &c. Besides thus:—1. Diminishing or annulling the more severe part of the sufferings attendant on natural labor, it will, 2. Abolish those more agonizing pains which accompany the use of the forceps, and other modes of operative delivery; 3. Enable us to extract the placenta artificially when required, without resistance or suffering; 4. Give us the power of making an accurate and full examination of the presentation, when necessary early in labor, as in placenta prævia, preternatural presentations, &c.

Uses in Medicine.—1. As an *antispasmodic;* as in asthma, laryngismus, tetanus, and other spasmodic diseases, &c.,[1] I have used successfully the inhalation of ether to arrest the paroxysms of hooping-cough, dysmenorrhœa, colic, and the pains attendant on the passage of biliary calculi. In a case of the most severe, at the same time painful, spasmodic twisting and convulsions of the extremities attending a second attack of chorea, I allowed the patient ether inhalation; and sometimes she lay under its influence for hours, with relief while its action lasted, but generally without sleep. Latterly the chloroform has both relieved the spasms and their attendant pain, and procured sleep. 2. As an *anodyne* or *narcotic*. In neuralgia, I have seen chloroform stop the fit at once; in two other cases the pain remained absent only while the chloroform acted. A patient suffering under severe delirium tremens had remained awake for about seventy hours; a half ounce of laudanum, given at a single dose, failed to produce rest; ten hours afterwards, the inhalation of chloroform was immediately followed by several hours of critical sleep. What cases of insanity would it benefit? I have exhibited it in full doses in some cases of dementia, combined with excitement and wakefulness. They were all asleep in about a minute—and remained so for some time. In nothing does chloroform differ from ether more than in its soporific effects—when given in full doses, and continued for some time. 3. In small doses as a *diffusible stimulant;* to arrest the first commencement of ague, ephemera, &c.; in hysteria, &c. Perhaps it may be used by inhalation in small quantities when the stomach will not bear wine or other stimulants; in severe vomiting, fevers, &c. I have seen its inhalation at once dispel a sick headache. 4. As a *contra-stimulant* in inflammatory diseases,[1] especially those of a painful character? ? Acute rheumatism, &c. ? ?

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[1] In various trials at Morningside, at which Dr. Christison, Dr. Skae, and Dr. Wingett were present, Dr. S. had sent violent patients over into a soporose sleep in a minute or less. As to what its therapeutic effects, if any, might be in insanity, we had he believed no power yet of judging. It could not be expected to be of any marked service—at least in any short time—in such chronic cases, as it had been tried. But it had this effect: the patients could be kept asleep under, it for a long series of hours. In this way, it had already apparently cured some cases of delirium tremens, and, he believed, also of puerperal insanity, and might yet be found useful in other forms of acute mania. At all events, it was a means of restraining a furious maniac; as powerful as, and, perhaps, it would be found far more safe than a strait-jacket or the grasp of a number of keepers. Once set a patient over (and that was the work of a minute), and a nurse with a pocket-handkerchief and some chloroform might keep him under perfect and complete restraint. Nor need he add how useful the same means might be in enabling a riotous and resisting patient to be removed to an asylum, or from one place to another. It would be tedious to discuss all the other diseases in which it had already been tried. Dr. S. was most anxious to observe its effects in acute local inflammations, but he had, as yet, had few opportunities of doing so. He mentioned a case he had seen with Dr. Dease (apparently an extra-uterine conception bursting into the abdomen), where the accompanying severe abdominal pain, and its dreadfully depressing effects, were kept at bay by the anæsthetic and contra-stimulant effects of the chloroform. He mentioned a case of extremely severe *cholera*, where, after all things had failed and the patient was apparently sinking, the inhalation of chloroform had induced sleep—dispelled the spasms and vomiting—and restored the patient.—(From the Discussion at the Medico-Chirurgical Society of Edinburgh, December 15, 1847. See Monthly Journal of Medical Science for January, 1848.)

Cautions.—The liquid used should be sufficiently strong. Its proper sp. gr. (as I have said) 1·480. I have seen some specimens perfectly unfit for use from their weakness; others perfectly unfit for their want of purity, for although of the proper specific gravity, they contained free chlorine, irritating the throat, and making the patient cough. It is certainly far too powerful an agent to be intrusted to nurses or unprofessional individuals. I have given it, up to this date, to above eighty persons, without the slightest bad result of any kind whatever in any one of them. The power, however, which we have with it, of bringing down the pulse, &c., shows that, if exhibited in *too strong* a dose, given *uninterruptedly* for too great a length of time, it would doubtless produce serious consequences, and even death. But, certainly, all its *full* anæsthetic and other influences may be perfectly obtained without allowing it to produce such depression as would be in any degree dangerous. I have, in obstetric cases, kept patients under its influence for several consecutive hours, without any resulting harm of any kind to either mother or child—renewing its application from time to time after the first full dose. Like many other agents, it may be powerful for evil as well as for good. I believe its great potency will be one great safeguard against its abuse.

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[1] For the results of its employment in Pneumonia, see a previous page of this volume.

Its influence upon the blood, &c., the counter-indications to use, &c. &c., remain still to be ascertained.

EDINBURGH, 25th November, 1847.

CHAPTER III.

NOTES ON THE ANÆSTHETIC EFFECTS OF CHLORIDE OF HY CARBON, NITRATE OF ETHYLE, BENZIN, ALDEHYDE, AND BI PHURET OF CARBON.[1]

DURING the last few months two or three different substa have been brought forward as anæsthetic agents; but our me journals have afforded little or no detailed notice of their ef The few following notes, however imperfect, may therefore no uninteresting; more particularly as they are the result of d experiments upon myself and others with the agents in ques In most of these experiments, I had the kind and able assistan Dr. Keith and Dr. Duncan.

When first publishing, in November, 1847, upon the anæsth properties of chloroform, I stated that, "in making a variety o periments upon the inhalation of different volatile chemical liqu I have, in addition to perchloride of formyle, breathed chloric hydrocarbon, acetone, nitrate of oxide of ethyle, benzin, the v of iodoform, &c. I may probably," I added, "take another op tunity of describing the result."[2]

Three of the substances which I named in the preceding list, duce, when inhaled, a state of anæsthetic insensibility, viz., chl of hydrocarbon, nitrate of oxide of ethyle, and benzin.

CHLORIDE OF HYDROCARBON.

Chloride of Hydrocarbon, or *Dutch liquid*, as it is often terme consequence of its being first discovered by the Dutch chemis the last century, is one of the various fluids to which the nan *Chloric Ether* was for some time given.

When equal parts of olefiant gas and chlorine are mixed toge the two gases rapidly disappear, and produce a colorless oily li of a peculiar sweetish taste and ethereal odor. Its specific gr is 1·247. It boils at 148°. It is composed of four atoms of ca four of hydrogen, and two of chlorine. Hence its formu $C_4H_4Cl_2$.

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[1] From Edinburgh Monthly Journal of Medical Science, April, 1848, p. 74

[2] See Lancet for 20th November, 1847, p. 549.

When its vapor is inhaled, the chloride of hydrocarbon causes so great irritation of the throat, that few can persevere in breathing it for such a length of time as to induce anæsthesia. I have latterly, however, seen it inhaled perseveringly until this state, with all its usual phenomena, followed, and without excitement of the pulse, or subsequent headache. When I myself attempted to inhale the chloride of hydrocarbon, it produced an extreme degree of acrid irritation in the throat, which did not disappear entirely for many hours afterwards.

NITRATE OF ETHYLE.

When two parts of alcohol, and one part of pure nitric acid, are distilled together, with the addition of a small quantity of urea, *Nitrate of Ethyle*, or, more properly, *Nitrate of Oxide of Ethyle*, is produced. It is a transparent colorless liquid, with a sweet taste, and very agreeable odor. Its specific gravity is 1·112; it boils at 185°. It is a compound of four proportions of carbon, five of hydrogen, six of oxygen, and one of nitrogen; and its formula is $(C_4 H_5)$, O, NO_5; or Ac O, NO_5.

Nitrate of ethyle is easy and pleasant to inhale, and possesses very rapid and powerful anæsthetic properties. A small quantity, such as fifty or sixty drops, when sprinkled on a handkerchief and inhaled, produces insensibility after a few inspirations. But during the brief period which elapses before the state of complete anæsthesia is induced, the sensations of noise and fulness in the head are in general excessive; and much headache and giddiness have usually followed its employment, and persisted for some time.

BENZIN.

Benzin or *Benzole* was first discovered by Faraday, as one of the products in his experiments on compressing oil gas, and was designated by him *Bicarburet of Hydrogen*. Mitscherlich afterwards obtained it by distilling, at a high temperature, benzoic acid with an excess of slaked lime.

It is a clear colorless liquid, of a peculiar ethereal odor; with a specific gravity of 0·85; and boils at 186°. It is believed to be composed of two proportions of carbon and one of hydrogen. Its formula is, $C_2 H$; or perhaps, more properly, $C_{12} H_6$. It is polymeric with the hypothetic radical formyle.

In my experiments with benzin I found it capable of producing anæsthesia; but the ringing and noises in the head accompanying and following its inhalation, were so excessive, and almost intolerable in the case of myself and others, as to seem to us to render its

practical application impossible, even had there been no other
jections to its use. Latterly, Dr. Snow has tried its employm
upon some patients for tooth-drawing; and in one instance of am
tation. In this last case it produced convulsive tremors.[1]

ALDEHYDE.

Aldehyde, or *Hydrate of Oxide of Acetyle*, was first noticed
Doebereiner in distilling together sulphuric acid, alcohol, and
oxide of manganese; but it was left for Liebig to fix and determ
everything about its chemical nature. It is a colorless limpid liq
of specific gravity 0·791. It is very volatile, boiling at 72°.
spontaneously changes when long kept, and is converted into
substances, a solid and a fluid, metaldehyde and elaldehyde. Lie
found it to be composed of four atoms of carbon, four atom
hydrogen, and two of oxygen; and its formula is $C_4H_3,O + Aq$.

Professor Poggiale, of Paris, has lately made some experim
with dogs on the inhalation of the vapor of aldehyde, and from th
has concluded that its anæsthetic effects will be found more pro
and energetic than those of sulphuric ether or chloroform. It
tainly possesses, like some of the preceding agents, well-mar
anæsthetic properties; but it assuredly will never come into use
very few will be found capable of inhaling a sufficient dose of
vapor. In fact, out of five of us that attempted to inhale aldeh
very carefully prepared and purified, four were driven to susp
the respiration of it in consequence of the coughing, and insu
able feelings of dyspnœa which it immediately induced. The se
tions of difficult respiration and constriction in the chest which
vapor produced, resembled precisely those of a severe fit of s
modic asthma. In the fifth case, the experimentalist, after pers
ringly breathing the aldehyde for a minute or two, became enti
insensible; the state of anæsthesia lasted for two or three minu
during it, the pulse became excessively small and feeble. On
covering, the bronchial constriction and coughing, which had
appeared as the anæsthesia was induced, returned immediately,
was annoying for some time after.

BISULPHURET OF CARBON.

Bisulphuret of Carbon, or *Alcohol of Sulphur* (as it was at
termed),[1] was accidentally discovered in 1796, by Lampadius, w
experimenting on iron pyrites. Different opinions of its comp
tion were held by different chemists; but Berzelius and Marcet
1813, at last fully confirmed the previous idea of Clement

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[1] Lancet for 12th February, 1848, p. 130.

Desormes, Vauquelin, &c., that it consisted only of sulphur and carbon. It is composed of two atoms of the former to one of the latter; consequently its formula is $C\,S_2$.

The most easy method of procuring it is by transmitting the vapor of sulphur over fragments of charcoal heated to redness in a closed porcelain or iron tube. The resulting bisulphuret of carbon, when purified by distillation, is a clear, colorless liquid, of a pungent taste. Its specific gravity is 1·272. It is very volatile, boiling at 108°.

It has been stated in various literary journals, that bisulphuret of carbon has lately been used as an anæsthetic agent at Christiana; but no particulars regarding its employment in Norway have, so far as I know, been yet published.

I have breathed the vapor of bisulphuret of carbon, and exhibited it to about twenty other individuals, and it is certainly a very rapid and powerful anæsthetic. One or two stated that they found it even more pleasant than chloroform; but in several it produced depressing and disagreeable visions, and was followed for some hours by headache and giddiness, even when given only in small doses. In one instance I exhibited it, with Mr. Miller's permission, to a patient, from whom he removed a tumor of the mamma. It very speedily produced a full anæsthetic effect; but it was difficult to regulate it during the operation. The patient was restless in the latter part of it, but felt nothing. Like several others when under it, her eyes remained wide open. After the operation she was extremely sick, with much and long-continued headache; and, for fifty or sixty hours subsequently, her pulse was high and rapid, without rigor or symptoms of fever.

I tried its effects in a case of midwifery, in presence of Dr. Weir, Dr. Duncan, Mr. Norris, and a number of the pupils of the Maternity Hospital. It was employed at intervals during three quarters of an hour. The patient was easily brought under its influence, a few inspirations sufficing for that purpose; but it was found altogether impossible to produce by it the kind of continuous sleep attending the use of chloroform. Its action was so strong, that when given, as a pain threatened or commenced, it immediately affected the power of the uterine contractions, so as often to suspend them; and yet its effects were so transient that the state of anæsthesia had generally passed off within a minute or two afterwards. The patient anxiously asked for it at the commencement of each pain. During its use she was occasionally sick, and vomited several times. Latterly her respiration became rapid, and her pulse rose extremely high. I then changed the inhalation for chloroform, and, under it, the patient slept quietly on for twenty minutes, when the child was born. During these twenty minutes there was no mo sickness or vomiting, and the pulse gradually sank down to i natural standard. A few minutes after the child was expelled, a while the mother still slept, her pulse was counted at 80. Next d the mother and infant were both well, and she has made a go recovery.

While these experiments prove the strong anæsthetic propert of bisulphuret of carbon, they at the same time show its dis vantages. I have not alluded to another strong drawback upon use, viz., its very unpleasant odor. "It has," says Dr. Gregory, peculiarly offensive smell of putrid cabbage."[1] By dissolvi various essential oils in the bisulphuret, I tried to overcome t disagreeable defect, but without much success.

None of the five anæsthetics which I have mentioned in t present communication are, I believe, comparable with chlorofo or sulphuric ether, either in their manageableness or in their effe And the after-consequences which all of them tend to leave, are t severe and too frequent to admit of their introduction into practi They are more interesting physiologically than therapeutically.

EDINBURGH, March 20, 1848.

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[1] Outlines of Chemistry, p. 130.

SECTION IV.

ON LOCAL ANÆSTHESIA.

CHAPTER I.

HISTORICAL NOTICE ON THE PRODUCTION OF ARTIFICIAL LOCAL ANÆSTHESIA.[1]

A FEW months ago I published some remarks with the object of showing that the artificial production of a state of general anæsthesia before the performance of surgical operations was not altogether an idea of modern times. Several ancient writers aver that such a state of general anæsthesia can be produced by using mandragore—as Dioscorides, Pliny, Apuleius, &c. In the thirteenth century, Theodoric published a receipt for producing it by the *inhalation* of vapors arising from the watery extracts of various narcotic herbs; and in our own days, 1832, a receipt similar to this of Theodoric's has been, apparently, found quite sufficient for the purpose by Dauriol.

The ancients, also, seem to have entertained the idea of the possibility of producing a state of *local* and limited anæsthesia in any part to be operated upon. Dioscorides, who, as we have seen, repeatedly mentions the production of *general* anæsthesia by mandragore, states it as a matter of report that *local* anæsthesia in a part was capable of being produced by the Memphian stone. "The Memphian stone," says he, "is found in Egypt near Memphis, of the size of a calculus, fatty, and of different colors. They say that this, when bruised, and spread over parts that are to be cut or cauterized, produces in them a dangerless anæsthesia."

Whilst we may entirely doubt whether local anæsthesia was capable of being produced by such an apocryphal application as the above, the passage is curious, as evidence that the idea of obtunding a single part of the body against the pain of an operation, was not unknown or unthought of in former times. Nay, many old authorities believed that against the fire ordeal, any part of the body could be so protected and defended by previous applications, that the human hand, for instance, should not feel the contact of red-hot iron. The writings of Eusebe Salverte, and of Beckmann, contain ample notices on this disputed question. Upwards of half a century ago, our countryman, Dr. Moore, ingeniously proposed to effect a local anæsthesia of any limb that was to be operated upon, by previously compressing with tourniquets and pads the nervous trunks going to that limb; and he has left us an interesting account[1] of a case of amputation in St. George's Hospital, in which this plan was tried, apparently with partial success, by John Hunter.

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[1] Provincial Medical and Surgical Journal, July, 1848, p. 365; Lancet, July, 1848, p. 30; London Medical Gazette, July, 1848, p. 62.

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[1] See his "*Method of Preventing or Diminishing Pain in several Operations of Surgery*," London, 1784.—"I communicated," says Dr. Moore, p. 30, "the experiments I had made, and all my ideas on the subject, to Mr. Hunter, who was so obliging as immediately to offer me an opportunity of trying the effect of my compressor, at St. George's Hospital, on a man whose leg he was to take off below the knee within a few days. I went to the Hospital the day before the operation to try the instrument. The patient had lost all his toes, and had a large ulcer on his foot. This was so much inflamed and so irritable that dressing it in the gentlest manner gave him the most acute pain. I applied the instrument; after the compression had been continued for about half an hour, his limb became so insensible that rubbing pretty smartly with the finger upon the ulcer gave him no pain. Next morning, the patient being carried into the operation-room, I began the compression of the nerves at a quarter before eleven o'clock. The numbness of the limbs followed at the usual time. At a quarter before twelve I gave him one grain of opium to diminish the smarting of the wound after the operation, when the compression should be taken off. A few minutes after twelve the tourniquet was applied, and the amputation performed by Mr. Hunter, at the usual place below the knee.

"At the circular incision through the skin, the patient did not cry out, change a muscle of his face, or show any symptoms of pain. At the subsequent parts of the operation, particularly during the sawing of the bones, he showed marks of uneasiness in his countenance, but did not cry out. As it was thought necessary to take up no less than five arteries, the operation lasted a longer time than is usual, and towards the end he grew faintish, and desired to have some water, and afterwards asked if they were nearly done. When the operation seemed to be over, and the bleeding stopped, the tourniquet was relaxed, and I also removed the compressor; but a small vessel bleeding unexpectedly, it was thought necessary to tie it also. Here the patient showed very strong marks of pain, and afterwards declared that tying this last vessel gave him much more pain than all the others, although the great nerves had been included in the ligatures. When he was put to bed the wound smarted, as is usual after amputations. The compressor being now entirely removed, this was to be expected. But some time after, being questioned concerning the pain he had suffered during the operation, he declared that he had felt hardly any, except (as he himself expressed it), at the rasping of the bones, which, he added, had shaken his whole limb. This seems a little extraordinary, as sawing the bones is usually the least painful part of amputations." * * * "This trial had all the success I expected; there was evidently a most remarkable diminution of pain, particularly during the first incisions through the skin and muscles, which are generally by far the most severe parts of the operation, and I am convinced that what pain the patient felt was chiefly owing to some small branches of the lumbar nerves which extend below the knee, and were not compressed." Perhaps some of our modern surgeons will not consider inclusion of the great nerves in the arterial ligatures as any compliment to Mr. Hunter's surgery.

The possible production of local anæsthesia by this or other means is certainly an object well worthy of study and attainment. Surgeons everywhere seem to be more and more acknowledging the facility, certainty, and safety with which the state of general anæsthesia can be produced at will before operating, and the moral and professional necessity of saving their patients from all unrequisite pain. But if we could by any means induce a local anæsthesia without that temporary absence of consciousness which is found in the state of general anæsthesia, many would regard it as a still greater improvement in this branch of practice. If a man, for instance, could have his hand so obtunded that he could *see* and yet not *feel* the performance of amputation upon his own fingers, the practice of anæsthesia in surgery would, in all likelihood, advance and progress still more rapidly than ever it has done.

In the following remarks it is my object to state the results of a number of experiments which I have performed—1st, Upon the lower animals; and 2d, Upon man, with a view to the possibility of the production of such a state of local anæsthesia, by the local application of chloroform or other anæsthetic agents, to individual parts of the body.

CHAPTER II.

ON THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE LOWER ANIMALS.[1]

At a meeting of the Medico-Chirurgical Society of Edinburgh held on the 17th March, 1848, I took occasion to state that I had successfully chloroformed several of the lower animals—annelida, crustacea, fishes, &c.; that in some, especially in the common earthworm (*Lumbricus terrestris*), I had been able to produce local anæsthesia by applying the chloroform vapor locally; and had thus, at will, rendered anæsthetic, individual parts and portions of the worm, as the head merely, or the tail merely, or the middle part of the worm, merely, the head and tail remaining unaffected. At the same time I recapitulated what I had stated at one of the February meetings—that, in the human subject, local anæsthesia of a portion of the gums could be produced by rubbing the part with hydrocyanic acid. After the date of the above meeting, I was led to make some additional experiments upon the possible production of local anæsthesia in man; and in reporting the proceedings of the preceding sederunt of the Society, in the last number of the

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[1] Provincial Medical and Surgical Journal, July, 1848, p. 366.

"Monthly Journal of Medical Science," the editor has stated, in a short footnote, some of the results of those experiments upon the human subject, which I here intend to detail at greater length.[1]

Nothing could be more curious or satisfactory than the experiments alluded to on the production of local anæsthesia by the local application of chloroform vapor to different parts of the body of the earthworm. The resulting degree of local anæsthesia in the part exposed is generally, in the course of two or three minutes, most complete as regards both sensation and motion; in fact, after being sufficiently exposed, the chloroformed portion of the animal is quite flat and flaccid, does not move under any irritation, and can be doubled and twisted upon itself like a piece of loose wetted cord. If the part paralyzed by the chloroform is small, it will be dragged along by the movements of the other unaffected portions of the worm. Generally, in the course of a few minutes, it gradually regains its powers of motion, and its irritability and contraction under stimuli.

The easiest method of performing this experiment, is to place a small quantity of chloroform in the bottom of a tumbler, paste over the mouth of it a covering of paper, and make an aperture in this covering sufficient only to admit the portion of the animal that is to be chloroformed. When held in this position, the part of the animal below the paper and exposed to the vapor of chloroform, is generally thrown into violent movements for a minute or two before the state of anæsthesia supervenes. I have repeated the same experiments with the vapors of sulphuric ether, and of bisulphuret of carbon.

I have tried the same experiment with the same results upon the medicinal leech.

The results were, if possible, still more marked in another of the articulata that was submitted to experiment. A small myriapode or centipede (*Julus sabulosus ?*) was rendered completely anæsthetic and motionless in the posterior segments of its body, by exposing that part alone for a few minutes to the vapor of chloroform. The five or six last rings of the centipede, with the suspended and motionless feet attached to them, were, for a short time afterwards, dragged about in a kind of paraplegic state, by the brisk and lively movements of the anterior and unaffected portion of the animal. The animal soon and completely recovered, each segment with its corresponding feet regaining its power of motion; and this in regular order from before backwards.

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[1] See Monthly Journal, No. xci. p. 48.

[2] These experiments become the more interesting in a physiological and toxicological point of view, when we recollect, that in the articulata the vascular system is general and distributed *longitudinally*, while both the nervous and respiratory system of this class of animals is, in a great measure, segmentary and *transverse*, like the action of the anæsthetic.

In other centipedes experimented upon, a small quantity of fluid chloroform was applied by a slight brush to the head, or to two or three of the middle rings of the animal, or to the tail, and always with the effect of anæsthetizing and paralyzing the part or parts only with which the chloroform came in contact. Sometimes when the head and anterior rings were alone touched, the animal after vainly trying to push its motionless head forwards, suddenly reversed the movements of all its limbs for a time, and dragged the paralyzed head behind it. All of the centipedes experimented on recovered in a very short time from the effects of the chloroform.

By immersing the tail of the water-newt in chloroform vapor, the sensibility and motions of that part were rapidly destroyed, and returned in a few minutes afterwards. By a longer degree of immersion of the tail alone, the whole animal became anæsthetic; and in several experiments it was found possible, but difficult, to give the animal in this way a fatal dose of the vapor.

The hind leg of the frog becomes anæsthetic when exposed for four or five minutes to the vapor of chloroform. Immediately after, it drags the limb in progressing, and bears, apparently without feeling, pricking and irritation of it; but a galvanic current passed through it excites both sensation and motion. In one case, the motory power of the limb was not restored at the end of the third day. No effect appeared to result from keeping the hind leg of this animal immersed in strong tincture of Indian hemp.

One hind leg of a healthy, active rabbit, was confined in a large bladder containing the vapor of chloroform. At the end of an hour the common sensibility of the limb to pinching and squeezing was much impaired; but a current of galvanism passed through it produced crying and signs of pain. The power of moving the limb seemed unimpaired.

The hind leg of a guinea-pig, similarly treated, exhibited the same phenomena at the end of an hour; but the anæsthesia was more complete. The skin of the leg was red and congested.

The posterior extremities and pelvis of a strong guinea-pig, were enclosed in a bag containing the vapor of chloroform. At the end of an hour, no signs of pain could be extracted by pinching and squeezing either limb; and a current of electricity passed through a hind leg evidently caused much less pain than when the same current was passed through a fore leg. The whole hinder parts were very red and congested. The animal was also in some degree paraplegic, and dragged itself along by strong efforts with its anterior limbs.

In an interesting communication, addressed, on the 7th of June, 1848, to the Yorkshire Branch of the Provincial Association, and published in the number of the Provincial Journal for June 28, Mr. Nunneley of Leeds has stated that he has produced complete paralysis of the individual limbs of frogs and toads, by immersion or exposure of them for a few minutes, to the vapor of chloroform; that by a similar but longer immersion he had rendered the leg of the rabbit sufficiently anæsthetic to bear mutilation without pain; that he had immersed his own finger in anæsthetic fluids for about half an hour or an hour, and at the end of that time it was nearly powerless and insensible, nor was it entirely recovered for forty-eight hours; that in operating on the human eye he had rendered the organ anæsthetic by previously exposing it for about twenty minutes to the vapor of chloroform; and that, in his opinion, the action of all, or of most anæsthetic agents, might be produced locally by their local application, the sensorium being unaffected, consciousness retained, and the limbs not subjected to their influence being unaffected. This naturally leads us to the consideration of—

CHAPTER III.

THE PRODUCTION OF LOCAL ANÆSTHESIA IN THE HUMAN SUBJECT.[1]

Early in February, 1848, I was led to make the experiments to which I have previously alluded, relative to the artificial production of *local anæsthesia* in a portion of the human gums by friction with hydrocyanic acid, in consequence of being assured, on what I believed to be satisfactory evidence, that a dentist at Limoges, in France, M. Pernot, had the secret of extracting teeth with little or no pain, in consequence of previously applying some obtunding agent to the gums. I tried at the time a great variety of substances, in order to obtain this local anæsthesia, such as various ethers, bisulphuret of carbon, benzin, aconite, &c. Among all the agents employed, the effect of prussic acid was by far the most decided and complete; any part of the gum strongly rubbed by it, speedily became benumbed and insensible; but the resulting degree of anæsthesia was by no means sufficient for the purpose required. The results of these experiments were stated orally to the Edinburgh Medico-Chirurgical Society, at their meeting on the 16th February, 1848.

Before that date I had met with one instance in which local anæsthesia of the human hand had been produced in a young lady, in consequence of her accidentally holding in it for a considerable time a scent-bottle containing some chloroform. I tried at various times to reproduce a similar result in myself and in others, by keeping my hand wrapped in a napkin soaked in chloroform and other anæsthetic agents, but with little, or indeed, no decided success, until I used the vapor of chloroform raised by heat, the hand for the purpose having been immersed in a deep jar, into which a small quantity of chloroform was poured—the jar temporarily placed in a basin of water of the temperature of 130° or upwards, and the wrist or forearm being surrounded by handkerchiefs, so as to prevent the escape of the vapor.

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[1] Provincial Medical and Surgical Journal, July, 1848, p. 367.

A number of circumstances influence, however, the effect and the degree of the state of local anæsthesia; and as I have made a considerable variety of experiments, both upon myself and upon others, in order to ascertain these points, I will shortly state the results. Let me premise, that in the experiments upon which the following results are founded, the hands of the same individual were generally held simultaneously in two different jars, differently arranged in regard to material or otherwise, in order to make two different and comparative experiments at the same time; and the relative degree of anæsthesia in each hand was ascertained, during the experiment, by pinching the fingers with the thumb nail, without removing the hand from the jar. After they were removed, these and other more severe measures were used with the same view, as tests of the degree of anæsthesia. Most of the experiments referred to were repeated and tried upon several different individuals. The general results were the following:

1. When the hand is exposed to an anæsthetic vapor, it betimes presents the sensations of a limb benumbed by compression of its larger nervous trunks—the sensations, in fact, of partial or commencing paralysis. Usually, after a short time, a glowing or burning feeling is perceived in the parts most exposed, and gradually there supervenes a sensation of thrilling and tingling (like a limb asleep), which deepens more and more. The skin turns red, and the hand at last becomes stiff and clumsy, and feels as if enlarged, and painful impressions, as pricking, pinching, &c., are felt less and less than in the other unexposed hand. After the hand experimented upon is removed from the vapor, it is generally half an hour or more before its usual normal feelings are quite restored. The nerves of motion are usually apparently as much affected as the nerves of sensation.

2. When the jar containing the chloroform or other anæsthetic fluid experimented upon, was immersed in warm water, so as to

raise the vapor of the fluid more fully and quickly, the resulting anæsthetic effect was always greatly increased, both in rapidity and intensity.

3. The vapor of chloroform proved stronger than any other that was tried. When one hand, for instance, was immersed in a jar containing the vapor of sulphuric ether, and the other in a jar containing the vapor of chloroform, both jars containing similar quantities, and being subjected to the same degree of heat, the hand in the chloroform jar was both more speedily and more deeply affected than the other. In addition to the vapor of chloroform and ether, I have tried comparative experiments with the vapor of aldehyde, bisulphuret of carbon, iodide of methyle,[1] &c. The aldehyde had little or no effect of any kind. The iodide of methyle produced a very severe burning sensation, and left the hand intensely red and erythematous for a day or two afterwards, but with no marked anæsthetic influence. Among several of us that tried the vapor of bisulphuret of carbon, only one bore it for any length of time (about an hour), and in him it did not render the hand anæsthetic in any very appreciable degree; in myself and others the sensation of heat and burning became so utterly intolerable, as to force us to withdraw the hand in two or three minutes. Immersion of the hand for half an hour in Dr. Fleming's very powerful tincture of aconite,[2] or in a strong tincture of opium, or of Indian hemp, or in a strong solution of belladonna, produced no appreciable anæsthetic effect.

4. The hand, when plunged in liquid chloroform, is usually somewhat more deeply apathized than the other hand simultaneously held in the vapor of chloroform. This was the more general result with those who tried the experiment; but in some the chloroform vapor was as anæsthetic, or more so, than the liquid. Few persons can keep the hand for any adequate length of time in liquid chloroform; the sensation of burning becomes so intense and insufferable as to force them to withdraw it in a very few minutes.

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[1] Two or three months ago I began a series of experiments with the intention of testing the anæsthetic or other therapeutic effects of the various respirable compounds of amethyle, acetyle, &c. I was prevented from proceeding far in the inquiry, in consequence of the effects following the inhalation of the compound mentioned in the text, viz., iodide of methyle. I found it very powerfully anæsthetic, but dangerously so. After inhaling a very small quantity of it for two or three minutes, I remained for some seconds without feeling much effect; but objects immediately began to multiply before my eyes, and I fell down in a state of insensibility, which continued for upwards of an hour. I did not completely recover from the effects of it for some days.

[2] Some other forms or preparations of this and the other substances may possibly produce different results, as we know that aconite, when chewed, causes a numbness and tingling in the lips and tongue, lasting for some hours. See "Christison on Poisons," p. 2. In making this experiment with aconite, it appeared to me that the sensibility of the tongue, &c., was not decreased to pinching, &c., when the part was tingling from the effects of the chewing of the aconite.

On one occasion I held my hand for upwards of an hour in liquid chloroform without the part being more deeply apathized than it would have been by exposure to the vapor. One of my pupils, Mr. Adams, perseveringly retained his hand in the liquid chloroform for upwards of two hours; no great or very marked degree of local anæsthesia resulted. In these cases, in which the hand was long steeped in liquid chloroform, the sensations of burning returned severely, from time to time, as if in waves, during the experiment; and on removing it from the jar, the feelings of heat were temporarily aggravated. The normal sensibility of the parts speedily returned, and was completely restored within an hour or two in all, but the skin sometimes remained red and injected for a longer period; occasionally for several hours.

5. The anæsthetic effect of chloroform, &c., is increased, both in rapidity and in degree, by immersing the hand, with the cuticle softened and moist. When one hand, for instance, is immersed without any preparation, and the other is prepared by being bathed and fomented for ten or twenty minutes previously, the latter almost immediately begins to tingle under exposure to the vapor, the dry hand not for some minutes. The degree of anæsthesia is also ultimately deeper in the moistened hand.

6. The degree of delicacy of skin in the person or part exposed to the anæsthetic vapor influences the result. In females I have seen the degree of the local anæsthesia of the hand that was produced, much greater and deeper than I could ever render it in the male subject. In applying the vapor of chloroform in small cupping-glasses, &c., to different parts of the body, as the insides of the arms, &c., the resulting degree of local anæsthesia seemed, in a great measure, regulated by the tenuity of the skin of the parts experimented upon. The skin of the axilla seems too tender to allow of the vapor being applied for a length of time sufficient to produce anæsthesia. One of my students, who kept both of his lower extremities enveloped in strong chloroform vapor for three continuous hours, felt no appreciable local anæsthetic effect from it.

When strong chloroform vapor is locally applied to mucous surfaces, the attendant sensations of heat and smarting are too severe to allow of its sufficient continuance; at least, this is the result that I have obtained in applying it with small glasses to the inside of the lips, the tongue, and eye. Mr. Nunneley states, as we have already mentioned, that before operating on a difficult case of artificial pupil, he had applied for twenty minutes a small quantity of the vapor of chloroform to the eye, by means of a small jar, which accurately filled the orbit, with the effect of rendering the parts nearly insensible. Dr. Duncan and myself have repeatedly tried this experiment upon ourselves; but in none of the ti we made, with the eye either shut or open, could we burning action of the vapor upon the part above two nutes, and with no other result than always rendering experimented upon red and injected, and suffused with

7. The degree of anæsthesia produced in the hand by of it to the strong vapor of chloroform, does not, in ge ceptibly increase after fifteen or twenty minutes. The tions continue if the hand is still retained in the jar; but length of exposure does not, after a time, produce a con increased degree of local insensibility.

But, finally and specially, I would observe that the local anæsthesia produced in the human hand or skin, by it to the local action of the vapor of chloroform, has ne experiments, been by any means so deep and complete racter as to give the chance of annulling the pain of operation, such as the deep incision, or amputation of a compared with the other non-exposed hand, the chlorofo is generally rendered to a marked amount, *less sensitiv* insensibility is never, I fear, so entire and perfect as wi patient from the pain of the surgeon's knife. In short, I doubt, whether, in the human subject, we shall ever reduce the knowledge of this possible production of p anæsthesia to any practical purpose. It is principally in a toxicological and physiological point of view, and to the doctrine of the mode of action of anæsthetic ag

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[1] I have tried the application of various anæsthetic gases and vapors to cases of vaginal irritation and neuralgia. The stronger forms cannot be bor duced to try them in consequence of the following curious statement rega acid, published by Dr. Pereira ("Materia Medica," vol. i. p. 155).—"A lady fered a considerable time from some uterine affection, and had derived no treatment adopted, was advised to consult a physician in Italy (Dr. Rossi). examined the condition of the uterus, he assured her there was no organ merely a considerable degree of irritation, for which he proposed to apply c a sedative. This was done by means of a pipe and tube, communicating wi situated in another room. The patient obtained immediate relief, and alth been obliged to be carried to the doctor's house on account of the pain experi ing, she left it in perfect ease. On her return to England, she had a relap plaint, and applied to Dr. Clutterbuck to know whether she could have the applied in London, in order to save her the necessity of returning to Italy."

[2] The distinction which exists between the structure and functions even the lower animals, as compared with the skin of the human subject, perha explains the differences in the degree of local anæsthesia, capable of being the local application of anæsthetic vapors and fluids in the one and in t animals," says Dr. Jackson, see Amer. Cyclopædia of Prac. Med. and Surger 115, "whose skin is moist, and which possess a very delicate cuticle, cutane is a constant and important function. Such are frogs, salamanders, and The experiments of Edwards have established the skin in them to be enti and instrumental in their support."

bearings are more upon the theory than upon the practice of anæsthesia.

These remarks relate to local anæsthesia as capable of being produced by the anæsthetic agents with which we are at present acquainted. Others may no doubt yet be detected, much more powerful than any we at present know,[1] and their local application may enable us to effect the local anæsthesia desired. At the same time, this consummation, even, seems doubtful; for, perhaps, any agent possessing a deeper and more rapid anæsthetic local power, would, by absorption, affect the system generally, and it may be, dangerously, before complete local insensibility of a part could be effected. Some time ago, in attempting to produce local anæsthesia in my hand, by exposing it to the vapor of hydrocyanic acid, ere the hand was much, or very perceptibly benumbed, I began to feel the constitutional effects of the poison; my respiration became irregular, and I felt giddy and faint, when my assistant removed my hand from the jar. All due care was taken to prevent my breathing any of the vapor, and I sat during the experiment in a current of air. I felt the benumbing influence of the acid extending from the hand upwards along the arm a minute or two before the experiment was stopped.

In addition to the liquids and vapors experimented upon, I have tried long immersion of the hand in various gases, as carbonic acid and common coal gas (both of them powerful general anæsthetics when inhaled), without any effect. Chaptal, however, alleges that he had felt the limbs plunged in carbonic acid much benumbed; and Collard de Martigny[2] found that, when the general surface of his body was immersed in carbonic acid (arrangements being made so that none of it was breathed), giddiness, ringing in the ears, and the other symptoms produced by the action of this gas, supervened in eight or ten minutes, proving that it was absorbed. Davy[3] felt the premonitory exhilaration of nitrous oxide gas by exposing the surface of his body to it in the same way.

Chaussier[4] enclosed the leg of a dog in a bag containing sulphuretted hydrogen, and found that he could in this way, in a few minutes, induce the poisonous effects of this gas; and similar experiments were repeated by Lebkuchner[1] and Nysten,[2] and my friend Dr. Madden,[3] on the rabbit, &c., with similar results. I have held my hand enclosed in a bag filled with the constantly renewed vapor of sulphuretted hydrogen for about half an hour, without feeling any local effects.

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[1] Perhaps some special modification in the application of electricity, galvanism, or magnetism to the part to be locally anæsthetized, may be yet found capable of effecting this object. I have tried several experiments of the kind, but, as yet, without much success. The possibility of deep *local* anæsthesia existing in diseased states (as in colica pictonum, hysteria, hypochondriasis, &c.), without the general sensibility or consciousness being affected, is well shown in a late interesting essay of M. Beau, in the "Archives Générales de Médecine," for January, 1848. He relates one case of an insane patient who broke his fibula, and continued to walk about without pain until the limb became inflamed and gangrenous. It was then amputated, and still without the patient apparently suffering in any degree during the operation.

[2] Archives Génér. de Méd. vol. xiv. p. 209.

[3] Researches on Nitrous Oxide, 1800, p. 485.

[4] Sedillot's Journal de Méd. vol. xv. p. 25.

And even supposing that we could, by any means, so benumb the sensibility of a part to be operated upon as to render it anæsthetic, I doubt much if this state of local anæsthesia would place the parts in a condition at all favorable for being subjected to operation. It is quite possible, nay, probable, that other more powerful agents than those which I have experimented upon, may, by their local application, produce a greater and deeper local anæsthesia on the human subject than I have yet witnessed; but all, or almost all, of those that I have used, have, along with the anæsthesia, led to such a congested and injected condition of the part, as to give every likelihood, both of a greater tendency to hemorrhage at the time of operating, and of a greater tendency to inflammation in it immediately afterwards.

Other and more simple agencies than any which I have mentioned are capable of producing a certain amount of local anæsthesia. A mere exposure of the hand, for instance, to great changes of temperature, either in the way of increase or diminution, has the power of deadening the sensibility of the part. I have tried, and seen others try, to hold the hand, with a view to this, immersed in broken ice, or dipped in ice-water, and with the effect of inducing a degree of anæsthesia in the part, as deep, if not deeper, than exposure of the same part for a greater length of time to the local action of chloroform would have effected. In his admirable and classical "Lectures on Inflammation," the late Dr. Thomson remarks, that "the sensibility to external impressions of the parts exposed to cold, is always more or less impaired, and the diminution in the sensibility of the nervous system seems to admit of degrees, from the slightest perceptible numbness, to that of the most complete insensibility."[4] But he also correctly adds—"This diminution, however, of the sensibility to external impressions, is not unfrequently accompanied with severe degrees of pain." In fact, in making the experiments to which I have above adverted with the pounded ice, few of us could allow the hand to be retained in it above two or three minutes, in consequence of the intense pain which ensued; but still that brief period produced, as I have said, a most distinct and well-marked degree of local anæsthesia.

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[1] Diss. Inaug. utrum per viventium adhuc animalium membranas materiæ ponderabiles permeare queant, p. 10, &c.

[2] Nysten. Recherches Physiologiques, p. 137, &c.

[3] See his excellent Experimental Inquiry into the Physiolcgy of Cutaneous Absorption, p. 13.

[4] Thompson's Lectures, p. 617.

Keeping the hand immersed in water, of as high a temperature as it can conveniently bear, has also the same effect of rendering it, in five or ten minutes, partially anæsthetic and benumbed, as compared with the other unexposed hand; and this without the pain and suffering connected with the other extreme of temperature.

It is not my object here to inquire in what relative diseased or other conditions of a part, heat or cold are respectively calculated to act anæsthetically. But I would beg to make this general remark, that the action of the above, and of other applications, which we use to inflamed, burned, neuralgic, and pained surfaces and parts (such as preparations of opium, conium, aconite, belladonna, tobacco, &c. &c.), still require to be studied, which they have not yet been, in another and a new light, viz., as LOCAL ANÆSTHETICS. Therapeutical writers will, I believe, betimes consider and describe them in this novel point of view; and when attention comes to be directed to them with this object, some new facts and precepts may be elicited that will enable both the physician and surgeon, to exhibit and apply local anæsthetics with more science, precision, and success, than hitherto.

The experiments and observations which I have detailed in the preceding pages, perhaps entitle us, in the present state of our knowledge, to draw the following conclusions:—[1]

1. In animals belonging to the class of Articulata, complete local and limited anæsthesia can be produced by the local and limited application of the vapor or liquid of chloroform to individual parts of the body of the animal.

2. In Batrachian reptiles, the tail, or an individual limb, can be affected in the same way with local anæsthesia, by the local application of the chloroform; but, in addition, general anæsthesia of the animals usually results in a short time, in consequence of the chloroform absorbed by the exposed part coming to affect the general system.

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[1] Through the kindness of Professor Balfour, I have had various opportunities of trying the effect of chloroform in vapor upon the sensitive plant (*Mimosa pudica*). When the vapor was either too strong or too long continued, the plant was destroyed. When it was weaker and applied only for a few minutes, the leaflets in some plants closed as when irritated, and did not expand again for an unusual length of time. In other plants under exposure to the chloroform vapor, no closure of the leaflets took place, and, in a few minutes the plant became so anæsthetized, that the mechanical or other irritation of the leaflets or stalk did not produce any of the common movements; nor did their irritability become restored for a considerable time afterwards.

3. In the smaller mammalia a single limb, or even the whole lower or pelvic half of the body, can be rendered anæsthetic by local exposure of these parts to the influence of chloroform.

4. In the human subject partial, and, perhaps, superficial, local anæsthesia of a part, as the hand, can be produced by exposing it to the strong vapor of chloroform; but the resulting degree of this local anæsthesia is not sufficiently deep to allow the part to be cut or operated upon without pain.

5. Any agent possessing a stronger local benumbing, or an anæsthetic influence, would probably be dangerous, by its acting too powerfully on the general economy, before the local anæsthesia was established to a depth sufficient for operating.

6. Artificial local anæsthesia, from any known anæsthetic agents, seems objectionable in any part intended to be operated upon, in consequence of the vascular congestion and injection which attend upon and accompany this local anæsthesia.

7. There are few operations in which there is not previously a local broken surface; and the application of chloroform, &c., to such a surface, would be far too painful to be endured, no small degree of suffering sometimes arising from even the exposure of the unbroken skin to their action.

ON THE

PRACTICAL APPLICATION OF CHLOROFORM

AS A TOPICAL ANÆSTHETIC TO MUCOUS AND CUTANEOUS SURFACES.

In 1848, the preceding essay on local anæsthesia and its artificial production by chloroform, &c., was printed in two English medical journals.[1]

In 1853, Dr. Hardy, of Dublin, published in the November number of the Dublin Quarterly Journal of Medical Science, an interesting communication on the same subject, entitled, "On the Local Application of the Vapor of Chloroform in the Treatment of various Diseases."

The principal peculiarity in Dr. Hardy's essay consisted in the proposal of a special valved instrument—the Anæsthetic Douche as he termed it[1]—for the purpose of applying in an intermittent stream, the vapor of chloroform to any part or surface that was wished to be affected.

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[1] See Lancet and Medical Association Journal for July, 1848.

But in projecting a stream of chloroform vapor upon any point, I have generally made use merely of a common enema syringe; and it will be found, I believe, to serve as well, if not indeed better, than any of the complex and expensive special instruments invented for the purpose. In fact, a larger and more powerful stream of vapor can be kept up by an enema syringe than by any of the special anæsthetic douches which I have seen.

Any of the usual forms of pea-valve enema syringe will answer the purpose, provided their lower or receiving extremity be immersed in the vapor of chloroform, and the instrument worked in the usual way employed for the transmission of water or other liquids. The vapor of chloroform, &c., or rather of air loaded with the vapor, passes readily through the canal of the syringe, and is projected in an intermittent stream from its orifice.

The syringe which I have generally used for this purpose is the barrel syringe of Mr. Higginson. It consists of three pieces of caoutchouc tubing, the middle or thickest portion being provided at either extremity with the common pea or ball valve, and altogether forms, in my opinion, by far the simplest, most durable, and at the same time the cheapest description of syringe yet suggested for injecting fluids into the rectum or vagina. When used for the transmission of chloroform vapor, it requires to be worked in the usual way for the transmission of liquids, but with its lower or inferior extremity placed in air loaded with the vapor of chloroform. In order to effect this last arrangement, all that is necessary is to place this lower extremity of the instrument in the neck of a phial or bottle containing liquid chloroform. The lower extremity of the barrel enema syringe is generally made of the size and form of the two last joints of the little finger; and the tube is encircled with a projecting ridge or shoulder above this point. When employed as an anæsthetic douche, this finger-like end of the instrument is passed into the neck of a chloroform bottle sufficiently large to admit it easily; whilst at the same time the circular projecting ridge of the tube rests on the mouth of the phial. For this purpose the comm six-ounce phial or bottle, with a mouth four or five lines wide, swers perfectly. An ounce of chloroform placed in the bottom the phial will enable it to serve as an anæsthetic douche for a l time. Before using it, the shaking of the bottle will impregn the air in it more thoroughly with chloroform vapor. W patients themselves employ the syringe and bottle, perhaps it be found necessary to explain to them that they are not to in the liquid chloroform through the tube, but only the vapor of it rather air loaded with the vapor.

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[1] Dr. Hardy's original Anæsthetic Douche "was formed of a caoutchouc bottle, having attached to one side of it a metallic chamber and egress pipe provided with two valves, to regulate the admission and the egress of air and vapor. The metallic chamber was perforated at the side to admit a sponge sprinkled with chloroform, and this perforation was closed with a screw stopper." See figure of the instrument in Dublin Journal of Medical Science for November, 1853. Subsequently Dr. Hardy invented and used another Anæsthetic Douche, far more complex and expensive, of which he has given a description and drawing in the Dublin Medical Press for April, 1854. It was proposed to take out a patent for the Douche.—(See Medical Press for April 26, p. 268.)

The preceding simple arrangement converts a common enem vaginal syringe into an anæsthetic douche, equally, or, indeed, n powerful than the ingenious instrument specially invented by Hardy for the purpose. As a proof of this, let me merely st that in various trials upon various individuals, I have never the stream of vapor from Dr. Hardy's instrument, when f charged, produce a state of general anæsthesia when the jet fro was projected into the mouth; but I have found that result to follo some instances when the same experiment was made with a stro and more sustained stream of chloroform vapor sent through common syringe.

When the inferior end of the enema syringe employed is of a shape that it will not pass into the neck of a bottle contai chloroform, other arrangements may be required to supply it chloroform vapor. For this purpose the lower end of the syr may be placed upon the hollow of a concave sponge bedewed chloroform; or a piece of lint, flannel, or the corner of a h kerchief, or other such material, freely wetted with it, may be lig rolled around the lower extremity of the instrument. Someti with the same view, I have placed the end of the syringe in bottom of a cup or tumbler in which there was a bit of spong lint soaked with chloroform; for the vapor of chloroform b nearly four times heavier than atmospheric air, fills always lower part of such a vessel. By any of these means a suffic quantity of chloroform vapor can be supplied to fill the instrum and to make a stream of it pass from its superior orifice, when syringe is worked in the usual manner for transmitting liquids.

I have used the injection of chloroform vapor into the vagin the preceding method in many cases of painful and neuralgic ditions of the uterine and pelvic organs. In most instances, the first sensations of warmth produced by the injection have pa away, relief has been found to follow for a greater or less lengt time; and to sustain this state of freedom from suffering, the jection has generally required to be repeated by the patient after lapse of a few hours. This treatment has appeared to me more particularly useful in neuralgic states of the uterine organs and passages; in those organic diseases that are occasionally accompanied with suffering, as carcinoma uteri; in some cases of severe feelings of bearing down, and incapacity to stand and walk, complicated with displacements and enlargements of the uterus; and in various spasmodic conditions of the uterus attended with pain, as in threatened abortions; in after-pains; and most markedly in severe dysmenorrhœa. But at the same time I would beg to remark that in various instances in which the preceding morbid states were present, and in which I fully expected the usual anodyne effect of the vapor to be experienced, the treatment has failed to give the usual relief; probably because the mere superficial anæsthesia which results from the anæsthetic vapor was not sufficient in depth or in degree to produce an anodyne effect. In other instances, on the contrary, in consequence, perhaps, of the peripheral extremities of the nerves distributed to the genital mucous surface being specially affected, or having a special reflex influence upon the deeper-seated parts and pains, the chloroform vapor has succeeded not only in producing temporary relief, but in producing even a speedy and permanent cure, under circumstances where the previous duration and severity of the symptoms seemed, a priori, to forbid the hope of a restoration to health by this means alone. I had, for example, lately under my care, a patient who, in consequence of severe pelvic or uterine pain, had been obliged to keep the supine position upon the bed or sofa for nearly six months previously. All attempts at standing or walking brought on renewed paroxysms of suffering. The uterus was slightly retroverted, but otherwise apparently healthy. After being brought with some difficulty to Edinburgh from a distant part of England, the only treatment to which she was subjected consisted of the injection of chloroform vapor several times a day into the vagina, which at once relieved, and ultimately altogether removed the uterine pains. Within a week, the morbid sensibility of the parts entirely disappeared. There was, about a month subsequently a short relapse, in consequence of indiscretion in exercise and exposure to cold, but the attack speedily yielded to the same treatment. I never had the pleasure of watching such a speedy and perfect restoration to health and happiness from that state of hysteralgia which so often entails upon patients misery and suffering for long months and years.

I have repeatedly applied chloroform to the maternal passages during labor in cases of rigidity of these passages, and particularly in rigidity of the cervix uteri when coexisting with morbid irritability and sensibility of the parts. In these instances I have used sometimes the chloroform vapor injected by the usual means; sometimes a few drops of fluid chloroform mixed up with oil, or with a small solid mass of butter or ointment. The practice has appeared to me to be very often followed by two beneficial results —first, the abatement of the supersensibility of the maternal canals; and secondly, very often also with an increased secretion of mucus, and apparently an increased susceptibility to relaxation and dilatation in the rigid structures.[1]

In the preceding remarks I have hitherto spoken of chloroform when applied as a local anæsthetic to the genital mucous canals. Its local anæsthetic action on other mucous surfaces has not yet been much studied. I have seen, however, the injection of the vapor of chloroform into the rectum, useful also in some instances of morbid irritability and sensibility of the lower end of the intestinal canal, in tenesmus, &c. The mucous membrane of the eye seems in most individuals—especially in its diseased states—too irritable to bear the contact of very concentrated chloroform vapor, such as I employed in some early experiments alluded to in the last chapter; but in cases of photophobia and supersensibility to light connected with scrofulous ophthalmia, &c., the vapor of chloroform diluted with air or aqueous vapor, acts sometimes very markedly and beneficially as a local anæsthetic. I have seen the intolerance of light connected with ulcerative corneitis at once relieved by exposing the eye to the vapor of chloroform, raised by pouring a small quantity of the fluid into a cup of warm water. The patient will thus sometimes immediately be enabled to open the eye freely and without pain; and the chloroform vapor often serves also as the best possible medicinal application to the ulcerated surface.[1] The dentist can occasionally relieve the pain of toothache, by the local anæsthesia resulting from the application of a drop of fluid chloroform to the exposed interior of the tooth; or by directing a stream of chloroform vapor upon it. In painful and spasmodic states of the respiratory canals, when chloroform is applied to their mucous surfaces by inhalation, it is difficult, or indeed, impossible, to tell always whether the resulting relief is the effect of local or of general anæsthesia. In some cases of spasmodic asthma, relief is occasionally obtained by doses too slight to have acted by any general anæsthetic effects; but I have seen other instances of the same disease where the paroxysm was not effectually arrested till a complete state of general anæsthesia was produced. A similar observation holds true with regard to different cases of laryngismus. Sometimes that troublesome affection, hysterical or spasmodic aphonia, is at once cured by a few inhalations of chloroform vapor, acting perhaps as much upon the principle of a local as of a general anæsthetic. The irritability of the cough in cases of phthisis, bronchitis, pneumonia, &c., is often effectually relieved by doses apparently too small to have acted otherwise than as local anæsthetics. Lastly, in reference to the topical anæsthetic influence of chloroform upon mucous membranes, let me add, that the swallowing of a few drops of fluid chloroform in oil, cream, soda water, or any other convenient vehicle, sometimes speedily abates nausea, vomiting, obstinate hiccup, &c.—perhaps upon the principle of its acting as a local and limited anæsthetic upon the walls of the stomach.

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[1] *Note on the mode of dilatation of the maternal passages in labor.*—During parturition the maternal canals, viz., the cervix uteri, vagina, and vulva, are no doubt dilated principally by the results of muscular uterine action and mechanical pressure. But they evidently become also dilatable and relaxed by another and an additional process, which is so far independent both of muscular action or mechanical pressure. In proof of this we find the whole length of the canal of the vagina relaxing and widening during a protracted labor, *before* the head has yet passed the brim, or fully opened the os uteri. This vital process of dilatation seems to me to consist of a rapid development of cells within the tissues of the walls of the maternal canals—just as the thick mucous secretion thrown out upon the free surface of these canals during labor (and indicative when present in great quantity of great dilatability in these canals) is essentially, and in its ultimate physiological analysis, a rapid development of cells upon the free surface of their mucous coat. The application and stimulus of various substances, as simple warm water, of warm aqueous vapor, oils, simple or stimulant, &c., apparently promotes the dilatability of the tissues of the cervix uteri and vaginal canals, by promoting probably the more rapid formation of these cells. And from various cases which I have seen, I am led to believe that chloroform, both in the form of vapor, or of fluid diluted with oil or lard, will be found specially successful in producing this result, or at least—be the explanation what it may—in producing the required relaxation in cases of anormal or morbid rigidity.

The preceding observations are limited to the local anæsthetic effect of chloroform upon mucous surfaces and canals. On the skin it produces a topical action, similar in principle, but far less in degree. When the epidermis is removed, or when the skin itself is destroyed, the surface of any existing sore, such as an irritable abrasion, an excoriated nipple, or a benign or carcinomatous ulcer, can be very remarkably anæsthetized and benumbed by the local application of chloroform vapor; but the feelings of great heat and pain, which in the first moments accompany its application, more than counterbalance, in most subjects, the subsequent sedative effects derivable from its use. The various experiments which I have detailed in the preceding essay, show that chloroform fluid or vapor, when applied to the unbroken human skin, produces a degree and depth of local anæsthesia, that is sufficiently great to be sometimes useful in medicine, while it is not sufficiently great to be useful in operative surgery. In medicine, for example, the local anæsthetic effects of chloroform often prove most beneficial in local neuralgia, local rheumatism, &c.; and chloroform mixed with equal, or with varying parts of olive oil, according to the sensitiveness of the patient's skin, is sometimes, in such cases, the most efficient form of cutaneous topical anodyne which we can employ. The amount of local anæsthesia, however, capable of being produced, is not, as I have just stated, by any means deep enough to enable the patient to endure any operative or surgical procedure. In the earlier part of 1854, however, a variety of experiments were made in the Parisian hospitals, under the full belief that a stream of chloroform vapor projected against the skin might produce such an amount of local anæsthesia in any given part of the cutaneous surface, as would allow that surface to be cut or operated upon by the surgeon, without pain to the patient. Dr. Hardy's anæsthetic douche, or some modification of it, was the instrument usually employed in these experiments. Several alleged cases of the perfect success of this local cutaneous anæsthesia were published in the French journals. It was averred, for example, that M. Dubois had opened with the knife, and without pain, an abscess in the axilla, that M. Nelaton opened an abscess in the foot—the vapor of chloroform having in each case been previously applied to the skin; and that M. Danyan also without pain, made a caustic issue on the neck—the skin being prepared by the anæsthetic douche. But additional trials very speedily proved the inutility of the practice, as far at least as the possibility of producing by it immunity from the pain of surgical operations was concerned. At the end of these trials, in commenting upon the subject in the Parisian hospitals, M. Latour, the learned editor of the Union Médicale, observes—"I have felt, I avow, distressed and humbled with all the noise that has been made, and with the recital of all the numerous experiments that have been tried in this matter. I have not desired to accumulate the record of them in this journal; and I would wish that all trace of these facts were, for the honor of French physiology, blotted out as speedily as possible."[1]

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[1] The vapor of prussic acid carefully applied to the eye, by means of a proper cup or glass, acts probably upon the same principle. It was much recommended some years ago by Dr. Turnbull; but indiscriminately in almost all ocular diseases. From what I have seen in practice, I believe that the use of dilute chloroform vapor and of carbonic acid, will become common in affections of the cornea and conjunctiva connected with intolerance of light and supersensibility. They both act not as powerful local anæsthetics merely, but also as excellent medicinal applications to any existing ulcers, &c.

In fact, the whole of these experiments and inquiries into the possibility of producing a sufficient amount of local anæsthesia for surgical purposes, by applying chloroform to the unbroken skin, resulted in the conclusion which I had already ventured to publish several years previously, namely, that "in the human subject, partial, and perhaps superficial local anæsthesia of a part, as the hand, can be produced by exposing it to the strong vapor of chloroform; but the resulting degree of the local anæsthesia is *not* sufficiently deep to allow the part to be cut or operated on without pain."[1]—(Feb. 1856.)

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[1] L'Union Médicale, for 4th March, 1854.

ON CARBONIC ACID GAS

AS A LOCAL ANÆSTHETIC IN UTERINE DISEASES, ETC.[2]

CARBONIC ACID is usually recognized by toxicologists as a very powerful narcotic poison, when inhaled in sufficient quantity. The rapidity and potency with which it acts as a general anæsthetic, has long been described by travellers in the experiment of temporarily plunging a dog into an atmosphere of carbonic acid in the Grotto del Cane, at Pozzuoli.

Carbonic acid gas acts also as a local anodyne or anæsthetic. The anæsthetic effect of a stream of carbonic acid, when applied locally, is easily proved by holding any exposed portion of the surface of the body where the skin is thin—as the wrist or forearm—over a jet of the gas, escaping from a common gas receiver. In a minute or two, the surface of the exposed part will be found benumbed, as when it is exposed to the vapor of chloroform; and pinching or irritating it is not followed with so much pain as a similar amount of pinching and irritation applied to the opposite and unexposed wrist or forearm. The degree of local anæsthesia capable of being produced in this way by the vapor of chloroform applied to a portion of the cutaneous surface, is, as I have already stated, not by any means deep and complete; but, in the relative experiments which I have made, it has generally appeared to me to be greater when a jet of carbonic acid gas was used, than when chloroform vapor was employed. And, like chloroform and other analogous agents, carbonic acid gas acts more powerfully when applied locally to mucous, than when applied to cutaneous surfaces.

In one respect, carbonic acid will be found preferable to chloroform vapor as a topical anæsthetic to the vagina and uterus. Though the application of the vapor of chloroform to the mucous membranes of the mouth, nose, pharynx, and bronchi, in the way in which it is usually inhaled, does not produce any very marked feeling of warmth in these mucous surfaces, yet its introduction into the genital mucous canals generally creates a disagreeable, and, in some instances, a very painful, though temporary feeling of heat and burning. The injection of carbonic acid gas into the vagina is not followed by any such painful sensation.

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[1] See this and other conclusions anteriorly.

[2] Addressed to the New York Academy of Medicine.

I have used carbonic acid as a local anæsthetic, principally in neuralgia of the vagina and uterus, in dysmenorrhœa, and in morbid states of the pelvic organs accompanied with pain, as in carcinoma, &c. I have found it also sometimes of use in irritable states of the neighboring organs. Two years ago I had under my care from Canada the wife of a medical gentleman, who was suffering much from that most distressing disease—dysuria and irritability of the bladder. Many modes of treatment had been tried in vain. The injection of carbonic acid gas into the vaginal canal several times a day at once produced relief, and ultimately effected a perfect cure. She has remained well since her return to America, and lately became a mother. Occasionally relief follows immediately. In two or three instances I have seen the use of the gas continued daily for months. I have notes of one case where the patient was invalided and almost entirely kept to the supine posture for years, from feelings of pain and bearing down in the uterus and neighboring parts, particularly on attempting to sit or walk. Many modes of treatment were tried by myself and others, with little or no benefit. She has, however, at last regained in a great measure the power of progression, and freedom from suffering in the erect posture—a result which she herself ascribes to the local application of carbonic acid gas, which I recommended to her some months ago; and in the use of it she has since regularly persevered.

Since using carbonic acid as a local anæsthetic application in uterine disease, I have met with two or three notices of this use of it by modern authors. In his work on Diseases of Females, published in 1835, the late Dr. Dewees, of Philadelphia, when discussing the treatment of carcinoma uteri, observes, "We have enabled several patients to derive much comfort, as well as temporary relief, from the extrication of carbonic acid gas within the cavity of the vagina, by means of a flexible tube, of sufficient length and size, attached to the mouth of a bottle, in which there is mixed diluted sulphuric acid and the carbonate of lime. This may be introduced into the vagina several times in the twenty-four hours. In two or three instances, this substance has relieved the severity of pain whenever it was employed, as well as diminished the offensiveness of the discharge."[1]

In cases of painful menstruation or dysmenorrhœa, Professor Mojon of Geneva has recommended the local application of carbonic acid gas.[1] Dr. Mojon states that in the disease in question he has employed the injection of carbonic acid gas into the vagina in a great number of instances, and generally with decided advantage, the pain being almost always relieved by this treatment. He directs the remedy to be used two or three times a day, and for five or six minutes each time. Like Dr. Dewees, he recommends the gas to be obtained by pouring dilute sulphuric acid on some pieces of chalk in a flask, from which a curved flexible tube carries it into the vagina.

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[1] Treatise on the Diseases of Females, p. 269.

In a paragraph which I have cited in a preceding chapter, from the second edition of the work of Dr. Pereira, on Materia Medica, carbonic acid is described as having acted speedily and beneficially in one case as a local sedative in uterine pain. "A lady," he observes, "who had suffered a considerable time from some uterine affection, and had derived no relief from the treatment adopted, was advised to consult a physician in Italy (Dr. Rossi). After he had examined the condition of the uterus, he assured her there was no organic disease, but merely a considerable degree of irritation, for which he proposed to apply carbonic acid as a sedative. This was done by means of a pipe and tube, communicating with a gasometer situated in another room. This patient obtained immediate relief, and although she had been obliged to be carried to the doctor's house on account of the pain experienced in walking, she left it in perfect ease. On her return to England, she had a relapse of the complaint, and applied to Dr. Clutterbuck to know whether she could have the same remedy applied in London, in order to save her the necessity of returning to Italy."[2] This case and paragraph, however, seemed to Dr. Pereira of so little value, that he has omitted the details of it in the last edition of his work. But from the time of reading it, I have tried at various times, and more particularly during the last two or three years, the local application of carbonic acid to the mucous membrane of the vagina and cervix uteri in different painful conditions of the uterus and neighboring parts; and, whilst it has failed in some instances to afford the expected relief, it has in others proved, as I have already stated, of great, and occasionally of almost instantaneous benefit.

In practice I have generally used a common wine bottle for the formation of the carbonic acid gas, and formed the gas by mixing in the bottle six drachms of crystallized tartaric acid with a solution of eight drachms of bicarbonate of soda, in six or seven ounces of water. A long flexible caoutchouc tube conducts the gas from the bottle into the vagina. The cork fixing this tube into the mouth of the bottle, should be adapted so as to prevent any escape of the gas by its sides. With this view the cork should be perforated by a metallic tube, and covered externally with a layer of caoutchouc.

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[1] Bulletin de Therapeutique.—American Journal of Medical Sciences, vol. xxii. p. 469.

[2] Elements of Materia Medica, vol. i. p. 155.

The use of carbonic acid as a local anæsthetic to the uterine mucous surfaces and to other parts of the body, is not a discovery of late times. I have found that in this, as in many other examples, what appeared to me at first novel, was, when fully investigated, a practice known previously in its essence, and perhaps in its more minute details also. Besides here, as elsewhere, when once we detect a principle, such as the anæsthetic power of carbonic acid gas when applied topically, we can explain by it the good effects of modes of practice, which previously perhaps we were inclined to ridicule and reject.

1. In some, for example, of our oldest works on female diseases, as in the Hippocratic writings,[1] and in the chapters of Paulus Ægineta,[2] Rueff,[3] Paré,[4] &c., referring to uterine affections, directions are given for the relief of local pains, &c., in the uterus, by a system of practice which at least included, and perhaps which essentially consisted of the local application of carbonic acid gas to the mucous membrane of the genital canals. I allude to the burning of various herbs, aromatic and medicinal, and the application of the fumes arising from their combustion, by appropriate tubes and instruments, to the interior of the vagina. It is now known to all that the combustion of dried plants and vegetable substances gives rise to the formation of carbonic acid; and the fumigations of the ancients, when they acted beneficially, probably acted much more by the mere topical application of this gas, than by anything aromatic or medicinal contained in the smoke of the burned ingredients.

2. Further, the knowledge of the marked local anæsthetic effects of carbonic acid gas appears to me to explain the good effects sometimes derived from a system of modern practice regarding which I formerly felt great skepticism. I have often been assured

[1] See Kuhn's edition of Hippocrates, vol. ii. De Naturâ Muliebri, pp. 567, 597, &c. De Morbis Mulierum, p. 859, &c. "Theophilus describes the process very minutely, but it will be readily understood, that it consisted in introducing the fumes of strong-smelling things, such as frankincense, spikenard, cassia, and storax, into the vagina by means of a funnel." Dr. Adams' edition of the works of Hippocrates, vol. ii. pp. 742 and 748. See also on the Hippocratic method of fumigating the mucous surface of the vagina, Halle and Nysten, in Diction. des Sciences Medicales, vol. xvii. p. 135.

[2] Adams' edition of Paulus Ægineta, vol. i. p. 642, &c.

[3] The Expert Midwife (1637), compiled in Latin, by James Rueff, part ii. p. 52, &c.

[4] Johnston's Translation of the Whole Works of Ambrose Paré, p. 944, with a description and figure of "a vessel made with a funnel or pipe for to fumigate the wombe."

by patients, of the soothing and sedative effects of the direct injection from the spring, of streams of various mineral waters, as practised at different German baths. It always seemed to me impossible that the slight amount of alkaline salts which these injection baths or streams contain, could be followed by the sedative effect so often ascribed to them. But from the inquiries that I have made, I believe it will be found that all the mineral waters which, when locally applied, produce this sedative or anæsthetic effect, contain a greater or less quantity of free carbonic acid, rapidly and constantly escaping from them; and the practice in its true therapeutic analysis probably consists only of the local application, in a somewhat clumsy form, of carbonic acid to the genital mucous surfaces. At some of the German watering-places, as Neuheim, Marienbad, &c., the large quantities of carbonic acid thrown off from the mineral wells has latterly been collected, and applied *per se* in the form of baths, jets, and streams, to different parts of the cutaneous and mucous surface—as to the uterus in neuralgia, &c., to the limbs when ulcerated, to the eye in irritable chronic ophthalmia, &c. &c. These uses of carbonic acid have been followed out, without, I believe, any tenable rationale having been suggested of the probable mode of action of the treatment. The utility of the practice, which I have been assured by my friend Dr. Funck of Frankfort, is most marked in some diseased states, will find, I believe, its true explanation in the local anæsthetic effect of carbonic acid. And if so, it is scarcely necessary to add, the remedy may be artificially made, and readily applied at any time, and at any place, and in the practice of any physician.

3. A knowledge of the topical anæsthetic effects of carbonic acid serves perhaps also to afford an explanation of other points in common therapeutics. There are, for example, circumstances in medical practice in which we either incidentally or intentionally apply carbonic acid to the gastric and intestinal mucous membrane. In gastric irritability and nausea, physicians, since the time of Riverius,[1] have constantly been in the habit of prescribing effervescing saline draughts, or artificial aerated waters, which throw off a great quantity of carbonic acid after their introduction into the stomach. In some instances the antacid action of the alkali may explain this effect; but is not the sedative action of these draughts, in most instances, dependent upon the local anæsthetic effect upon

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[1] The celebrated anti-nauseant and anti-emetic potion of Riverius was composed of a solution of a scruple of salt of wormwood (an impure carbonate of potass), mixed with a table-spoonful of lemon-juice. He describes it as a "remedium præstantissimum, præsertim in vomitu qui febribus malignis solet contingere."—Praxis Medica, lib. ix. cap. 7; et Centuria Observationum, obs. 15.

the mucous surface of the stomach of the large quantities of carbonic acid which they eliminate?

Dr. Pereira refers to Mr. Parkins' late recommendation[1] of the topical application of carbonic acid gas as a clyster in dysentery and diarrhœa. But neither Dr. Pereira nor Mr. Parkins seem to be aware that the practice is not altogether novel. As long ago as 1772, in a case of diarrhœa, complicating continued fever, Mr. Hey of Leeds threw up into the rectum, on two successive days, injections of carbonic acid gas. This practice had, as he supposed, the best effects—both the frequency and fœtor of the stools being apparently diminished by it.[2] Dr. Percival of Manchester also published in Dr. Priestley's work "two similar instances of the salutary effects of mephitic air thus administered as an enema." The same practice was also adopted in the last century by Dr. Warren of Taunton, and Dr. Rotheram.[4] "May we not," asks Dr. Percival, "presume that the same remedy, viz., enemata of carbonic acid gas, would be equally useful in dysentery?"[5] Dr. Henry subsequently recorded two cases of dysentery in which the use of carbonic acid clysters was, in his opinion, followed by very marked relief of the abdominal pains and swelling, and a diminution of the fœtor of the discharges.[6]

4. Carbonic acid, when applied to the surface of the body—the skin having been previously removed—acts as a local anæsthetic. A practice which probably depends for its utility on this principle has long been in vogue. The old yeast poultice (*Cataplasma cerevisiæ*) exhales from its surface a quantity of carbonic acid gas; and, perhaps, the beneficial and anodyne effects which surgeons formerly ascribed to it in the treatment of irritable and sloughing sores was in a great degree owing to the carbonic acid eliminated from it serving as a local anæsthetic and antiseptic. The constantly recurring motion, however, of points in the surface of the poultice, from bubbles of the gas mechanically raising and bursting it, does, no doubt, more than counteract, in some instances, any sedative effect that is derivable from the topical anæsthetic application of the carbonic acid. And perhaps far simpler means could be easily devised to keep an open and painful ulcer or wound in contact with a sufficient quantity of carbonic acid. Dr. Percival states that

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[1] On the Efficacy of Carbonic Acid Gas in the Diseases of Tropical Climates, &c.—London Medical Gazette, vol. xviii. p. 777.

[2] See A Letter from Mr. Hey concerning the Effects of Fixed Air applied by way of Clyster, in Dr. Priestley's Work, vol. ii. p. 292.

[3] See Dr. Dobson's Medical Commentary on Fixed Air. London, 1787, p. 14. Dr. Priestley on Air, vol. ii. p. 375. Dr. Priestley's Experiments, &c., on the Different Kinds of Air, vol. i. p. 305.

[4] See Percival's Essays, vol. iii. p. 237.

[5] Priestley on Air, vol. i. p. 305.

[6] Experiments and Observations on Different Subjects, p. 125.

in the application of pure carbonic acid gas to a carcinomatous sore, the sanies of the cancer was "sweetened by it, the pain mitigated, and a better digestion produced."[1] Dr. Ewart, of Bath, applied it locally in two cases of ulcerated cancer of the mamma. In the first of these cases the ulcer, which was nearly five inches long, three inches broad, and about two inches long at its greatest depth, entirely, though temporarily, closed up and cicatrized in three months, under the constant local application of carbonic acid gas. In the second case the cancerous ulcer was larger, very irregular on its surface, and discharged a thin, mucous, fœtid matter. It filled up and contracted somewhat in its dimensions under the use of carbonic acid; and at the date of his report, viz., at the end of two months' application of it, Dr. Ewart observes that at least the gas "has kept a person in ease and comfort, who for so great a length of time before had known only agony and torture." "What," he elsewhere observes, "strikes us in the two preceding cases with the greatest astonishment, is the almost instantaneous relief of pain, which never failed to follow the application of the gas."[2] The celebrated Dr. Ingenhouz, who saw one of these cases, in which a "large cancerous ulcer of the breast" had temporarily cicatrized under the local application of carbonic acid, and then re-opened, states that still the ulcer "gives no pain when she (the patient) applies the air."[3]

The application of the carbonic acid gas was, however, speedily recognized as not capable of producing a cure, but as capable of producing alleviation only. "The public prints," observes the celebrated French chemist, Fourcroy, "contain accounts of several instances of the cure of cancer made in England by the application of the carbonic acid. We can nevertheless assert, that this means has been used by ourselves and others, without success, several times. After the first application, the cancerous ulcer exhibits a more favorable appearance; the sanies which commonly flows, becomes white, consistent, and laudable; the flesh assumes a lively color; but these flattering appearances do not continue; the ulcer soon returns to its former state, and passes through the usual changes with unabated violence.'[1]

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[1] Priestley's Work on Airs, vol. i. p. 302. Dr. Percival cites also the following case:—"A physician who had a very painful aphthous ulcer at the point of his tongue, found great relief, when other remedies failed, from the application of fixed air to the part affected. He held his tongue over an effervescing mixture of potash and vinegar, and as the pain was always mitigated, and generally removed by this vaporisation, he repeated it whenever the anguish arising from the ulcer was more than usually severe."

[2] Dr. Ewart's History of two cases of Cancer treated by Carbonic Acid. London, 1795, p. 48. On the Medicinal Effects of Factitious Air, part iv. By T. Beddoes, M.D., and James Watts, Engineer. Table of Cases, p. 4.

[3] See Part iii. of the Essay of Beddoes and Watts, p. 118; and Ingenhouz's Miscellanea, &c. 1795.

In reference to the effects of carbonic acid upon raw surfaces and wounds, Dr. Ingenhouz mentioned to Beddoes the following experiment:—"Blister your finger, so as to lay bare the naked and sensible skin. The contact of air will produce pain; put your finger into vital air (oxygene), and this will produce more pain; introduce it into fixed or azotic air (carbonic acid or nitrogen), and the pain will diminish or cease." In relation to this statement, Dr. Beddoes informs us that he made the following experiments on three different persons:—First, The raised epidermis of a blistered finger, after all action from the cantharides had ceased, was cut away in carbonic acid gas. No pain was felt. Secondly, A second blister being opened in common air, smarting pain came on. In a bladder of fixed air, this pain soon went off. Thirdly, After opening a third blister, the finger was instantly plunged into oxygene. It felt as when salt is sprinkled on a cut. In carbonic acid gas, the pain in two minutes quite subsided; but returned when the denuded skin was again exposed to the atmosphere.[2]

If there be no source of fallacy in these experiments, they certainly point to one kind of important improvement in the treatment of some painful burns, wounds, &c. For they appear to me to suggest the possibility of the suffering which is attendant on such injuries being controlled and cancelled by keeping the pained parts in contact with carbonic acid, or with some other gas or fluid, capable of acting as a local anæsthetic. If the reports of Ewart, Beddoes, and Fourcroy are correct, we ought also, indeed, to find carbonic acid an excellent application even as far as the mere healing and cicatrization of the broken surfaces are concerned.

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[1] Fourcroy's Elements of Chemistry and Natural History, Prof. John Thompson's translation, 5th edition, vol. i. p. 395.

[2] On the Medicinal Uses of Factitious Airs, pp. 43–45. *Note on painless extraction of teeth.*—I have previously stated, that it was reported that M. Pernot, a dentist at Limoges, in France, had a secret of extracting teeth with little or no pain, in consequence of previously applying some obtunding agent to the gums. More recently, I have been informed by several persons, some of them medical men who had practised at Hong-Kong, &c., that the extraction of teeth is sometimes performed in China without pain. The alleged secret of thus annulling the pain incident on tooth extraction seems to be possessed by a particular guild of dentists in the Celestial Empire. They are said to apply locally the anodyne, and to be able to pull out the tooth with very little force or pain some hours subsequently. The substance employed is generally reputed to be extracted from the head of a fish; and, if so, may probably be some form of phosphoric acid. Is it possible that any form of this acid can soften the tooth, or its bony socket, as acids generally soften bone, permitting its removal without pain, while the surrounding soft textures remain uninjured?

MMARY REGARDING THE THERAPEUTIC HISTORY AND EFFECTS OF CHLOROFORM.[1]

HISTORY OF ANÆSTHETICS.

E fluid to which the name of chloroform is given was nearly
ltaneously discovered in 1831–32 by Guthrie in America, Sou-
n in France, and Liebig in Germany. Its true chemical com-
ion, however, was not ascertained till Dumas and Peligot
ted their attention to the subject in 1834–35. It consists ulti-
ly of 2 atoms of carbon, 1 of hydrogen, and 3 of chlorine.
in accordance with the generally received opinion of chemists,
ay be more correctly described as composed of 3 atoms of
ine, and 1 atom of formyle; and hence the names given to it of
oform, chloroformyle, formychloride, or perchloride of formyle.
ther words, it is regarded as a compound of chlorine with
yle, while the latter is the hypothetic base of formic acid, or of
fluid capable of reducing the oxides of the noble metals, which
el Fischer long ago distilled from the body of the red ant, or
ica rufa (hence the terms formic and formyle), and which Marg-
Arvidson, Richter, Gehlen, and others subsequently examined
elaborately.

hen Guthrie, Soubeiran, and Liebig, first discovered chloroform
e course of their chemical experiments and inquiries, and when
as and Peligot subsequently worked out its true chemical com-
ion, their sole and only object was the investigation of a sub-
in philosophical chemistry. They labored for the pure love of
xtension of knowledge; they had no idea that the substance
hich they called the attention of their chemical brethren would
ould be turned to any practical purpose, or that it possessed
physiological or therapeutic effects on the animal economy.
e who use the *cui bono* argument against philosophical investi-
ns, on the ground that at first they may appear to yield no
tical benefit, will find that argument fully refuted in this as in
y other instances; for here we have a chemical compound
h for many years after its discovery was merely interesting as
tter of scientific curiosity and research, becoming latterly an
le of great importance and extensive manufacture, as a medi-
agent by which human suffering and agony may be annulled
r some of the most trying circumstances in which human
re is ever placed.

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ing the article "Chloroform," from the Encyclopædia Britannica, vol. ii. p. 627, 1855.

Chloroform may be obtained by various processes; as, 1, by passing a stream of chlorine gas into an alkaline solution of caustic potass; 2, by decomposing chloral with the agency of aqueous fixed alkalies; 3, by heating acetate of potass or acetate of soda with chloride of lime; 4, by boiling chloro-acetic acid with aqueous ammonia; 5, by decomposing chloride of methyle by chlorine, in a vessel exposed to the sun's rays; 6, by distilling alcohol, pyroxylic or wood spirit, or acetone, with chloride of lime, &c. &c. Some vegetable oils, as those of turpentine, lemons, bergamot, peppermint, &c., when heated with chloride of lime and water, yield chloroform. When manufactured on a large scale, chloroform is prepared by distilling a mixture consisting of six parts of chloride of lime, or common bleaching powder, in thirty parts of water, and one part of alcohol. The fluid which passes over separates into two layers, the lower of which is chloroform; and the upper layer consists principally of a solution of weak spirit. The chloroform, before it can be safely used, requires, however, to be much purified. For, 1st, in order to remove the water and alcohol mixed with it, it is shaken up with about half its volume of strong sulphuric acid slowly added; and, 2dly, to remove the acid, the chloroform is redistilled from milk of lime. Sometimes it is redistilled a third or even a fourth time, after being allowed to stand over quicklime, in order to render it completely pure, and of the requisite specific gravity. For other methods, and full details as to the manufacture of chloroform on the large scale, we need only refer to the pharmacopœias and dispensatories, and to the various text-books on chemistry.

It is a clear, colorless, limpid liquid; heavy, and of a specific gravity varying from 1·480 to 1·500; not inflammable; very volatile; it boils at 140° Fahr., and remains liquid and transparent at 4° Fahr. The density of its vapor is 4·13, or it is about four times heavier than atmospheric air. It has a fragrant, ethereal, fruit-like odor, and a slightly acrid, but at the same time intensely sweet, saccharine taste. It dissolves very sparingly in water, to which it imparts its sweet odor and taste; but it mixes in all proportions with alcohol. It is a powerful solvent, speedily dissolving camphor, gutta-percha, wax, resins, &c.

Chloroform is often found in the market very impure, and containing empyreumatic oils, aldehyde, chlorine, free muriatic acid, &c. When so contaminated, its medicinal use, by inhalation, is liable to be attended by headache, cough, nausea, &c. It deserves to be particularly known, that the purest chloroform will sometimes spontaneously decompose if left exposed for a length of time to the combined influence of heat and light. Hence, when the liquid is kept for medicinal purposes, and particularly in warm climates, it is a matter of importance to keep it in a dark and cool place.

When inhaled in small quantities only, and slowly, the influence of chloroform upon the system is exhilarating and intoxicating, like the influence of alcohol, or any diffusible stimulant.

When inhaled in larger and continuous doses, with the view of suspending pain in surgical, obstetrical, or medical practice, its effects are generally as follows:—After a few inhalations, there supervenes a feeling of warmth and exhilaration, radiating from the heart towards the extremities, but generally perceived first in the extremities themselves. In most persons, this feeling is speedily followed by a sensation of vibratory thrilling and benumbing throughout the body, and by affections of the organs of sense, as by loud whirring noises in the ears, or brilliant lights before the eyes. After one or two additional inhalations, there is a rapid loss of sensation and voluntary motion, and at last a total suspension of consciousness. The state induced is a condition of deep artificial sleep. During this anæsthetic sleep, the relation between the mental condition of the patient, and his unsusceptibility to the feeling of pain, differs much in different individuals, and is greatly regulated by the amount of dose that is used. When the dose is at last full and complete, and such as is now usually given before most great surgical operations, no mental action appears to go on, or, at least, none whatever is remembered. In other persons, however, and especially when the dose is not so great and complete, though all consciousness of pain is suspended, the mind is still active as in dreams; and occasionally, though rarely, this peculiarity is observable—that the same type of dream recurs to the same individual every time he is subjected to the anæsthetic vapor. Sometimes when the dose of chloroform is not sufficiently great, a patient will wince or cry out under the use of the surgeon's knife or cautery, and afterwards, on awaking, declare that he has felt nothing. In such cases, a certain amount of sensibility and consciousness appear to remain, but the memory of the patient subsequently retains no recollection whatever of any circumstances that have happened during his temporary anæsthetic sleep. Persons occasionally will talk ramblingly when subjected to an incomplete dose; but this rarely happens except some of those around are conversing and exciting the patient; and it can be readily arrested by an additional dose of the anæsthetic, and by enforcing quietude. In other persons, again, sometimes, when the dose is small, sensation is found to be suspended, and freedom from pain obtained, whilst still a greater or less amount of mental consciousness and intellectual activity and clearness remains. Their sensibility to suffering is more or less completely deadened, while their intelligence is preserved. The muscles of voluntary motion usually become more and more relaxed as the intensity of the anæsthetic effect increases, and at last their action can always be thoroughly suspended—a matter of great surgical moment in relation to the reduction of dislocations. But occasionally, before this total relaxation is produced, a state of spasm supervenes, particularly if the patient is held and restrained, or excited by talking and noises during the inhalation of the chloroform. Generally the pulse is increased in frequency during the first inhalations, but when the system is fully brought under the influence of the anæsthetic it decreases to its natural standard, or even becomes slower than natural; and few persons are able to bear with total impunity the wounds of a severe surgical operation until the anæsthetic vapor is given to such an extent as to have reduced the pulse to this degree. The respiration, though unaffected at first, becomes slower and deeper as the influence of the anæsthetic increases; and usually it is rendered soporose before any great surgical operation is begun. The temperature of the body decreases when the action of the chloroform is long continued.

The rapidity with which the effects of chloroform upon the mind and body disappear is perhaps not less wonderful than the rapidity with which they supervene. Generally the awakening from the anæsthetic sleep is in the course of a few minutes after the inhalation is arrested. When the patient is left undisturbed he usually awakes to a state of perfect consciousness; but sometimes, particularly if roused artificially or too early, a few minutes will elapse before he is perfectly master of his own state and situation. When the drug used is pure no headache follows. In some cases there is left a tendency to sleep.

The inhalation of chloroform is now extensively employed by professional men to fulfil various purposes in the practices of surgery, midwifery, medicine, and medical jurisprudence.

In *Surgery* it is employed with different indications, as 1. And principally, to annul and abolish the pain and agony attendant upon the various chirurgical operations, whether these operations are performed with the knife, caustic, ligature, or otherwise. But the surgeon finds great value in its use in other matters of practice, as, 2. In enabling him to make a far clearer examination and more accurate diagnosis in some difficult cases of injury and disease, such as fractures, dislocations, stone, &c. 3. By the total relaxation which a full dose produces, the reduction of dislocations, herniæ, &c., is much simplified and facilitated. 4. The removal of patients who have suffered severe injuries or wounds, to their homes or to hospitals (a matter often attended with much and exhausting suffering), has now, by the previous use of chloroform, been often accomplished without pain and without danger. 5. The agony attendant upon the daily dressing of large wounds, as those made by burns, amputations, &c., has been abolished by its employment. 6. Under the use of chloroform, amputations and other operations, required after severe injuries and wounds, are sometimes capable of being performed when, without it, the state of shock and depression would otherwise totally forbid operative interference. And lastly, the mortality accompanying surgical operations has been in a marked degree diminished and lessened since the general introduction of anæsthetics.

In *Midwifery* various important objects are gained by the accoucheur through the employment of chloroform. 1. By its use he is enabled to save the mother from the suffering attendant upon the process of common parturition in the human female, and that without placing his patient in a degree of anæsthesia by any means so deep, and hence so dangerous, as is required in surgery. 2. In morbid or difficult cases of labor, requiring manual or instrumental interference, the state of anæsthesia enables him to apply that interference without pain to his patient, and generally with much greater facility to himself. 3. By its aid the process of obstetric diagnosis is, in many circumstances of doubt and difficulty, very greatly improved and facilitated. 4. By its relaxing effects, it renders the dilatation of the maternal canals more easy, especially where these canals have any tendency to spasmodic rigidity and contraction. 5. In cases where arrestment of uterine action is a matter of great importance, as in the operation of version, &c., a very complete and very deep dose of chloroform enables us to attain this object far better and more speedily than by opium or other means. 6. Chloroform seems generally capable of reducing and keeping in abeyance one of the most common and most fatal complications in difficult labor, namely, puerperal convulsions. And, lastly, by saving the constitution of the patient from the pain attendant on the process of human parturition, it saves her strength and constitution, expedites her convalescence, and renders her proportionately less liable to the various affections which occur in the puerperal state.

In *Medicine* the inhalation of chloroform has been employed for various purposes:—1. As an anodyne or anæsthetic in severe and exhausting pains, whether inflammatory or neuralgic; as in earache, toothache, pleurodyne, tic, &c. 2. As a narcotic in cases of delirium tremens, of puerperal and other forms of mania, and in other diseases where there is wakefulness and excitement. 3. As an antispasmodic in colic, dysmenorrhœa, laryngismus, asthma, hooping-cough, the pains attendant on the passage of biliary and renal calculi, in chorea, tetanus, hysteria, and in infantile and other forms of convulsion. 4. In small doses, as a diffusible stimulant to arrest the first commencement of the rigôr in ague, in ephemeral fever, &c., and to support the excitement of the system where the stomach will not bear wine or other stimulants. Lastly, it has been used, particularly by some German and French physicians, in inflammatory affections alike of the head, chest, and abdomen. In the hospital of Frankfort, and elsewhere, small and frequently repeated inhalations of chloroform have been extensively employed in inflammation of the lungs, with the effect, it is alleged, of more marked relief to the cough, pain, and fever, and a more speedy resolution of the disease than under any other treatment with which it was contrasted. It will probably ere long be applied to other uses in medicine.

Chloroform, administered by the stomach and not by the lungs, is also used by physicians. The usual dose of the drug when swallowed is from five to twenty drops, dissolved in a mucilaginous or oily liquid. In this form it exerts in a less marked but in a more prolonged degree the same therapeutical effects as the inhalation of the drug by the lungs.

In the detection of feigned diseases, as in pretended paralysis and contractions of limbs, in simulated deafness, &c., chloroform has been successfully used by army surgeons and others.

Various chemical gases and vapors beside chloroform have been found when inhaled to possess the effect of producing, some in a more, others in a less marked degree, a state of anæsthetic insensibility and sleep. Among these we may enumerate protoxide of nitrogen (the "laughing gas" of Sir Humphry Davy), olefiant gas, light carburetted hydrogen, bisulphuret of carbon, chloride of hydrocarbon, or Dutch liquid, aldehyde, acetone, coal and rock naphtha, benzoine and various ethers, as the nitric, acetic, hydrochloric, formic, and other ethers, but particularly sulphuric ether.

It has also been supposed that the odor and vapors of some vegetable substances can exert an anæsthetic effect. We shall afterwards see that the ancients believed that the odor of mandragora, and that vapors arising from the concentrated juices of henbane, hemlock, &c., were capable of producing insensibility to surgical operations. Knowing the soporific effects of the common puff-ball (*Lycoperdon bovista*), upon the working bee, we some years ago made repeated trials of the effects of it, but without ever producing any very marked degree of anæsthesia in man. Since that time it has been tried by Mr. Richardson and others, but, we believe, without any greater success.

The advantages which chloroform possesses over the other anæsthetic agents hitherto discovered are various. A smaller, and hence a more portable quantity of chloroform than of other anæsthetics is required in order to produce the state of anæsthesia; its action is more perfect and more certain; it exerts in ordinary doses no such depressing effects on the heart and general system as most of those chemical compounds which we have enumerated do; it acts rapidly, and consequently with a comparatively short stage of excitement: its inhalation is infinitely more agreeable than the inhalation of most other anæsthetic gases or vapors; its odor is rapidly evanescent, and does not adhere to the clothes of those near it, giving it in this respect no small advantage with the busy physician over the persistent and disagreeable smell of sulphuric ether, &c.; it is cheaper than any other known anæsthetic; and, lastly, no special instrument requires to be used in its exhibition.

To produce such a complete anæsthetic effect as is required for most surgical operations, a larger and more rapid dose is necessary than in obstetric practice; whilst in midwifery the drug requires to be given in smaller quantities, but for a far greater length of time than in surgery. Numerous forms of instruments have been proposed for the exhibition of chloroform by inhalation, but they merely complicate the process; and certainly the simplest as well as the safest apparatus is a piece of sponge, or a towel or handkerchief.

Two or three fluid drachms of the liquid diffused upon the interior of a towel or pocket-handkerchief, arranged in a somewhat concave form in the hand of the exhibitor, and applied over the mouth and nose of the patient, generally suffice to produce speedy and complete anæsthesia. If such an effect does not follow, an additional dose ought to be poured on the handkerchief at the end of a minute or two, for the drug rapidly evaporates. At first the moistened handkerchief ought to be held at the distance of half an inch or an inch from the face of the patient, to allow free access of air to the mouth and nostrils, and afterwards it should be gradually approached nearer. At last it should touch the face except at one side where the fingers interpose between the face and it, to allow of a sufficient access of atmospheric air. In order that the patient may be brought speedily under the influence of the drug, and with as short an excited stage as possible, the vapor should be allowed to pass into the air-tubes by both the mouth and nostrils, and all means of compressing either of these two cavities by the fingers or instruments must be strictly avoided. It should always be remembered that the vapor of chloroform is about four times heavier than atmospheric air, and hence the handkerchief or towel should be held *in such a position* over the mouth and nostrils, or so adjusted about the lower part of the face as to allow the vapor, by its mere gravitation, to fall into the air-passages. Not unfrequently, at the moment when the patient is first becoming insensible, he will suddenly withdraw his face, or forcibly push aside the handkerchief with his hand; but the handkerchief should be instantly so reapplied as to allow the vapor to gravitate towards and be drawn into the mouth and nostrils; and a few additional inhalations will now, after this point of excitement, render the patient quite insensible. The tests of the patient being fully anæsthetized that are usually most relied upon in practice, consist of induced slowness of the pulse, or some degree of noise or sopor in the respiration. Either or both of these phenomena, viz., slowness of the pulse, or some noise in the respiration, indicate that the patient is sufficiently insensible for undergoing any surgical operation, and the chloroform-handkerchief must be withdrawn as soon as they supervene. Subsequently, only smaller and intermitting doses of the vapor are in general required in order to maintain an adequate state of anæsthesia during the performance of the operation. The principal error committed in using chloroform in surgery consists in giving it at first in such small doses, or so slowly, as to keep up a state of excitement instead of inducing a true state of anæsthesia; and a still more grievous error is sometimes committed in commencing the use of the knife before the pulse or respiration is affected, and consequently while the patient is not yet sufficiently anæsthetized.

In midwifery practice there are two leading peculiarities in regard to the exhibition of chloroform, viz., first, that it is given to the patient only when the parturient actions or pains are present, and is always totally or entirely withdrawn in the intervals between those contractions or pains. The neglect of this simple but all-important rule has led, on the part of some practitioners, to much error and misconception regarding the effects and utility of chloroform in midwifery. But, secondly, the drug does not require to be given in such large and full doses in midwifery as in surgery, except where severe obstetric operations are to be performed. And, as ample experience has now shown, it may be given in common obstetric practice with perfect safety for two, four, six, or more hours, if the two simple rules alluded to above be duly followed.

When used in the treatment of medical diseases, chloroform is sometimes employed for a much longer time than in either midwifery or surgery. Patients suffering under tetanus, peritonitis, &c., have sometimes been kept more or less under its influence, for two, three, or more continuous days. We have seen a child affected with infantile convulsions of such severity as to defy all other means, kept under the agency of chloroform with slight intermissions, to allow of food being taken, &c., for fourteen days, with the effect of arresting the fits, and ultimately saving the life of the little patient.

We have as yet found no human being capable of withstanding the anæsthetic effect of chloroform, though sometimes in exceptional cases a much larger dose is required than in others; and the actual amount necessary in any case can be judged of only by the actual effects of the drug, and not by any rules as to its mere measurement or quantity.

There are perhaps few morbid states which entirely contraindicate the use of chloroform when required as an anæsthetic agent. It is generally, however, believed that marked disease of the valves of the heart, or fatty degeneration of the walls of that organ, and diseased states of the brain, form reasons for avoiding its employment. It should not be given in large doses—as for surgical operations—shortly after meals; otherwise sickness and vomiting of the contents of the stomach are liable to follow. A little previous fasting usually prevents this complication.

In the way of caution in the employment of chloroform, the points that demand the principal attention are the following:—1. The drug employed should be as pure as possible, and free from those various deleterious ingredients that are sometimes found mixed up with it, and which are liable to produce cough, headache, &c. 2. In its exhibition there ought always to be allowed to enter with the vapor of chloroform a free intermixture of atmospheric air, the fingers of the exhibitor being for this purpose always kept placed at one side between the face of the patient and the chloroformed towel or handkerchief; and 3. Its action should always be suspended, and the handkerchief or instrument containing it instantly removed, whenever snoring and stertor supervenes in the respiration, or when the pulse becomes languid, and falls much below the natural standard; or when the face and lips greatly alter in their color either to pallor or lividity.

The exhibition of chloroform, as of every other potent drug, used in medicine, is liable to be attended with danger and death, provided it be given in too large or in too long continued doses. Like most other valued medicinal agents, it is powerful for evil as well as for good. But its occasional disagreement with, or deleterious influence upon one in 10,000 or 20,000 patients, is no sound argument against other patients benefiting from its employment. It has been calculated, from the returns of the registrar-general, that every year in England and Wales alone some 300 or 400 human beings are poisoned with opium; but certainly no one would argue that this is any reason why *opium*, the most valuable remedy in our pharma-

copœia, should not be given to other human beings in proper d
and in proper cases. Patients have often sunk under the depres
effects of calomel, antimony, digitalis, &c.; but such accide
while they teach us very strong lessons of caution, form no re
why these most useful drugs should be banished from the pha
copœia. Many persons are annually drowned in bathing; bu
reasonable man would argue from such unfortunate occurrences
this powerful means of maintaining and restoring health be t
fore abandoned and forsaken. Deaths certainly ever and anon c
in patients subjected to the influence of chloroform, but assu
only very rarely indeed when a pure drug and all proper precau
are used. Perhaps the exhibition of any other potent medi
agent in the materia medica, exhibited in equally full doses
many hundreds of thousands of patients as have now inhaled
doses of chloroform, would have been followed by more acci
and deaths than have been witnessed in the use of this anæst
agent. When we consider the immense extent to which chloro
has already been employed in all quarters of the world in medi
in surgery, and in midwifery, the frequent great impurity of the
and the little care which has sometimes been observed in its
the wonder perhaps really is, that so few accidents have happ
from its employment. And as a counterbalance to these accid
we know from statistical evidence the fact, that in the absolu
has been a great means, not only of saving human suffering
also of saving human life, by diminishing in a marked ratio
danger and fatality attendant upon surgical operations and dise
states. Thus, let us take amputation of the thigh as an exa
Out of 987 cases of this operation, collected by Mr. Phillips
proved fatal, or 44 in every 100 died. But out of 144 amputa
of the thigh performed upon patients in an anæsthetic state,
27 proved fatal, or 25 in 100 died. According to this comput
the number of persons saved from death in amputation of the t
by the patients being anæsthetized during the operation, amo
to 19 lives in every 100 operations performed, or to 190 lives o
every 1000 such operations.

All the patients that die under the hand of the operator
chloroform is used, do not necessarily die from the effects o
chloroform upon the constitution. In several of the recorded
the dose given was far too small to have had any such fatal e
Before the time that anæsthetics came to be used in surgery, d
on the operation table ever and anon occurred. Such cases
been recorded by Brodie, Cooper, Home, Travers, &c. &c., but
excited no marked share of professional attention, as they
generally supposed to be accidents against which no caution

of any use. Of late years, and since chloroform has been em-
oyed, they have usually been directly and at once ascribed to the
leterious action of the chloroform. The week after the anæsthetic
fect of chloroform was discovered in Edinburgh, a patient sud-
nly died upon the operating table in the infirmary of that city,
mediately after the first incisions for the reduction of a hernia
ere made, and before the operation was finished. Fortunately,
om special casual circumstances, chloroform was not used in this
se, or otherwise the drug would doubtlessly have been blamed for
e result. We know of two other cases in the same city in which,
nce the introduction of chloroform, patients have died during or
mediately after surgical operations, and in both of which, from
cident rather than any other cause, chloroform did not happen to
used, or to be at hand for use.

When in any case too powerful and large a dose of chloroform is
ven, the means of recovery which ought to be pursued are chiefly
e following:—1. The instant removal of the chloroform hand-
rchief or instrument, and of everything containing the liquid,
om the neighborhood of the patient. 2. The supine position. 3.
he free access of pure air to his face. 4. If necessary the per-
rmance and continuance of artificial respiration by alternate com-
ession and relaxation of the walls of the chest or other means,
king special care at the same time to pull forward the tongue in
e first instance, provided it has fallen backwards on the top of the
indpipe. Some authorities have recommended the use of galvan-
m if an apparatus be at hand, the inhalation of oxygen or ammonia,
version of the body, &c. No liquid should be poured into the
outh of the patient till he is able to swallow.

Instead of being inhaled so as to produce a constitutional anæs-
etic effect, chloroform is sometimes used locally in the form of a
uid or vapor with the view of obtunding the sensibility of that
dividual part of the body only, to which the agent is directly ap-
ied. The local application of chloroform alone, or mixed with
l, is one of the most powerful local sedatives which the materia
edica possesses; and as such often relieves rheumatic, neuralgic,
d other pains, when applied to the suffering part. But in the
man subject, the degree of partial and superficial local anæsthesia
hich is capable of being produced by chloroform liquid or vapor is
ver sufficiently great to allow of the part to be cut or operated
on without pain. All late experience has gone to prove the
ath of this observation in regard to man. But in some of the
wer animals, complete local and limited anæsthesia can be readily
duced by the local application of chloroform. For example, in
e articulata, as in the common earthworm and the centipede, the

application of chloroform to the head or tail of the animal or to individual medial rings will render the parts touched altogether anæsthetic, whilst the remainder of the body retains its natural state Latterly, in the human subject, it has been proposed by Dr. James Arnott, to produce local anæsthesia, before some minor surgical operations, by previously freezing or frostbiting the affected part by the application of a strong frigorific mixture. In most people no inconsiderable amount of pain attends this process of sudden local freezing; the part frozen is not in a condition admitting of easy surgical interference with common instruments, and the state of anæsthesia does not extend beyond the skin and subcutaneous cellular tissue.

The vapor of chloroform was first proposed by Dr. Simpson as an anæsthetic agent in surgery and midwifery in 1847. For a year previously, the vapor of sulphuric ether had been used to a considerable extent, both in America and Europe, for the purpose of inducing insensibility to pain in surgical operations. Sulphuric ether was first practically adopted for this purpose in 1846, by Dr. Morton, a dentist, at Boston, in America. Subsequently Dr. Charles T. Jackson of that city claimed the merit of having suggested it to Dr. Morton as an agent capable of producing insensibility to pain. But the power of producing, by the vapor of sulphuric ether, an insensibility exactly like that produced by the inhalation of nitrous oxide gas, had been long previously known. The fact had been already often published by several American authorities, as by Godwin (1822), Mitchell (1832), Professor Samuel Jackson (1833), Wood and Bache (1834). Richard Pearson was the first to suggest the inhalation of sulphuric ether in medicine in 1795; and he then described its employment in some cases of phthisis, asthma, hooping-cough, &c. The sedative effects of its inhalation in these affections have been noticed by almost every author who has written at any length on the Materia Medica during the first half of the present century. In 1816 Nysten proposed and described a special instrument for the inhalation of sulphuric ether.

The idea, however, of saving by some artificial means the human body from the pains and tortures inflicted by the knife of the surgeon, is by no means a thought either first broached or first acted upon in recent times. For the production of anæsthesia a variety of measures had been suggested, and some used, long before sulphuric ether and chloroform were applied to this purpose. In 1828 Dr. Hickman appears to have proposed the inhalation of diluted carbonic acid gas as an agent capable of inducing insensibility in surgical operations; and the anæsthetic properties of carbonic acid have been long known and often witnessed in the experiments constantly performed before travellers on the dogs which are made to breathe this gas in the Grotta del Cane near Naples. In 1800, Sir Humphry Davy threw out a hint as to the possibility of applying nitrous oxide as an anæsthetic. In 1784, Dr. Moore attempted to produce local anæsthesia in limbs requiring amputation or other operations, by previously compressing and obtunding the nerves of the implicated extremity—an idea, however, which was suggested long before Moore's time by Ambrose Paré. In the sixteenth and seventeenth centuries various authorities, as Valverdi, Hoffman, &c., suggested the possibility of producing temporary anæsthesia during surgical operations, by a plan sometimes successfully adopted by modern robbers, viz., by such an amount of "garotting" or compression of the vessels of the neck as would produce the requisite amount of stupor and coma.[1] Some surgeons also proposed to induce before operating a state of fainting, and consequently of insensibility, by a previous profuse bloodletting, &c. The administration of a large opiate has been also repeatedly suggested and tried by various authorities; but the amount of dose required to produce true anæsthesia and insensibility to the pain of a surgical operation was found to be far too large to be free from imminent danger to the life of the patient.

But at a still earlier date different medicinal agents seem to have been suggested, and practically employed too, for the purpose of producing a state of anæsthesia during surgical operations. These agents were sometimes used in the form of odors or vapors, or by inhalation; and sometimes they were administered by the stomach. Two different drugs appear to have been more particularly used at different epochs with the view of inducing insensibility to the agony and torture otherwise following the surgeon's knife, viz., preparations, 1, of Indian hemp (*Cannabis sativa var. Indica*); and 2, of Mandragora (*Atropa Mandragora*).

The anodyne, ecstatic, and anæsthetic effects of Indian hemp, and of the various preparations made from it, as bang, hachish, &c., have been long known in Africa and Asia. "The bang," as Sir Joseph Banks observed, half a century ago, "is prepared, and, I believe, used in all parts of the East from Morocco to China. In Barbary," he adds, "it is always taken, if it can be procured, by criminals condemned to suffer amputation; and it is said to enable those miserables to bear the rough operations of an unfeeling executioner, more than we Europeans can the keen knife of our most skilful surgeons." M. Julien lately pointed out to the French Academy an old Chinese work, proving that 1500 years ago a preparation of hemp or ma-yo was employed medicinally in China to annul the pain attendant upon cauterization and surgical operations. The wonderful power of endurance of the Hindu Suttee appears to have been sometimes procured by the influence of this powerful drug. Some high biblical commentaries maintain that the gall and vinegar or myrrhed wine offered to our Saviour immediately before his crucifixion was a preparation, in all probability, of hemp, which was in these, as well as in later times, occasionally given to criminals before punishment or execution—while 700 years previously it is possibly spoken of, according to the same authorities, by the prophet Amos as the "wine of the condemned."

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[1] Professor Fleming of Cork, has latterly resuggested the same form of anæsthetic, apparently unaware of its former proposition by Valverdi, &c.

The symptoms described by Homer as produced on Ulysses and his companions by their drinking of the Egyptian nepenthes, are far more like the effects of hemp than of any other known agent. Herodotus twice mentions the ecstatic influence which the inhalation of the vapor of burning hemp produces upon the Scythians and Messagetans, who, according to his account, breathed it for the purposes of excitement and inebriation.

The other plant mentioned—the Mandragora—is now banished from the materia medica, but its therapeutic virtues certainly seem to call for some renewed investigation. Most of the old Greek and Roman physicians and writers, such as Galen, Aretæus, Celsus, &c., ascribe to it strong soporific powers; and several of them, but especially Dioscorides, Pliny, and Apuleius, describe its decoction or tincture as endowed with such anæsthetic powers that those drinking a proper dose of it are insensible to the pains of the surgeon's knife and cautery. It is given (writes Dioscorides eighteen centuries ago) "to cause insensibility (ποιεῖν ἀναισθησίαν) in those who are to be cut or cauterized; for being thrown into a deep sleep they do not perceive pain." The observations of Pliny, Apuleius, &c., are to the same effect. In the twelfth and thirteenth centuries Hugo of Lucca used, and his pupil Theodoric (who died in 1298) has described a somniferous ball or sponge, "Spongia somnifera," the vapors raised from which were capable, when inhaled, of setting patients into an anæsthetic sleep during surgical operations. This somniferous ball was, in the first instance, made by filling and imbibing a sponge with dried extracts of mandragora, opium, and other sedatives; and when required for use, the sponge was dipped for a time in hot water, and the patient made to breathe the vapor thus raised from it till an anæsthetic sleep was produced. A modern French surgeon, M. Dauriol, states that he has successfully induced a state of anæsthesia in various surgical patients, by the means described 600 years ago by Theodoric. Why the mandragora fell into disuse as an anæsthetic agent in surgery does not appear in any professional records. Aretæus, after speaking of the deep and long-continued sopor produced by drinking an infusion of mandragora, adds, that occasional danger results from using it, and the patient may die convulsed. The frequency and the fear of such results may probably have been the cause of its anæsthetic employment in surgery falling into abeyance. Chamappe, a French surgeon who wrote in 1538, tells us that at that time, "some surgeons give, like Theodoric, soporiferous medicines to their patients, that they may not feel the incisions of the scalpel;" and he describes the "somniferous sponge" of Hugo as adapted for this purpose. But already, towards the end of the same century, Ambrose Paré, the celebrated Parisian surgeon, alludes to the exhibition of mandragora "to avert the pain attendant upon the amputation of a limb," as a practice only used "formerly" by operators, and apparently as not followed in his own day. An early English author, Bulleyn (1579) described the possibility of setting patients into an anæsthetic state during lithotomy, &c., by the use of mandragora; but at the same time he speaks of the sleep thus artificially produced, as a "trance, or a deepe terrible dreame."

The older authors do not always give explicit accounts of the substances and preparations which they recommend for use as anæsthetic agents. Occasionally, they affect an air of secrecy and mystery with regard to their composition and character. Thus, in the 8th book of his Natural Magic (1608), Baptista Porta, gives various receipts for medicines which produce sleep, insanity, &c. Amongst others, he describes a "sleeping apple" (*Pomum somnificum*), made with mandragora, opium, &c., and the smelling of which binds, he avers, the eyes with a deep sleep. Subsequently, he states that there can be extracted from soporific plants, "a quintessence which must be kept in leaden vessels, very closely stopped, that it may not have the least vent, lest it fly out. When," he continues, "you would use it, uncover it, and hold it to a sleeping man's nostrils, whose breath will suck up this subtle essence, which will so besiege the castle of his senses that he will be overwhelmed with a most profound sleep, not to be shook off without much labor. After sleep, no heaviness will remain in his head, nor any suspicion of art. These things," Porta adds, "are manifest to a wise physician; to a wicked one, obscure." Meissner relates, at considerable detail, that towards the close of the seventeenth century, a secret remedy was exhibited by Weiss to Augustus II. of Poland, while his majesty was asleep, and during the state of anæsthesia thus induced, the king's diseased foot was amputated. The operation was done without the royal patient's consent, and its performance was not discovered by him till the following morning.

The former general belief in the idea that a degree of anæsthetic and prolonged sleep could be induced artificially by certain medicated potions and preparations,[1] is shown by the frequency with which the circumstance is alluded to by our own older poets and story-tellers, and made part of the machinery in the popular romance and drama. In the history of Taliesin (one of the antique Welsh tales, contained in the Mabinogion), Rhun is described as having set the maid of the wife of Elphin into a deep sleep with a powder put into her drink, and as having then cut off one of her fingers when she was in this state of artificial anæsthesia. Shakspeare besides alluding more than once to the soporific property of mandragora, describes with graphic power, in Romeo and Juliet, and in Cymbeline, the imagined effects of subtile distilled potions, supposed capable of inducing, without danger, a prolonged state of death-like sleep or lethargy. And Middleton, in his tragedy of "Women, beware Women," published in 1657, pointedly and directly alludes, in the following lines, to the practice of anæsthesia in ancient surgery :—

> "I'll imitate the pities of old surgeons
> To this lost limb—who, ere they show their art,
> Cast one asleep, then cut the diseased part."

Indeed, the whole past history of anæsthetics is interesting, as a remarkable illustration of the acknowledged fact, that science has sometimes for a long season altogether lost sight of great practical thoughts, from being unprovided with proper means and instruments for carrying out these thoughts into practical execution; and hence, it ever and anon occurs, that a supposed modern discovery is only the re-discovery of a principle already sufficiently known to other ages, or to other remote nations of men.

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[1] Jocelyn, in his Life of St. Kentigern or St. Mungo of Glasgow—a biography written about the year 1180—speaks of Theneu, the saint's mother, having probably had given to her a lethargic or anæsthetic potion at the time of her professedly unknown impregnation. The passage is remarkable. I quote it in the original monkish Latin:—"Constat nobis sumpto *potu oblivionis* quam phisici *letaragion* vocant, obdormisse; et in membris incisionem, et aliquociens adustionem, et in vitalibus abrasionem perpersos, minime sensisse: et post somni excursionem, quae erga sese aditata fuerant ignorasse."—(*Vitæ Antiquæ Sanctorum Scotiæ*, p. 200.)

ANÆSTHETICS IN SURGERY.[1]

FROM A PATIENT'S POINT OF VIEW.

My dear Dr. Simpson—I have recently read, with mingled sadness and surprise, the declarations of some surgeons that anæsthetics are needless luxuries, and that unendurable agony is the best of tonics. Those surgeons, I think, can scarcely have been patients of their brother surgeons, and jest at scars only because they never felt a wound; but if they remain enemies of anæsthetics after what you have written, I despair of convincing them of their utility. My present object in writing is not to supplement your arguments in favor of the administration of anæsthetics to those who are about to undergo surgical operations; but, as one who knows from personal experience what operations were to the patient before ether or chloroform was employed anæsthetically, I am anxious to state certain reasons in justification of their use, which only those who suffered without their help are in a condition to urge.

Several years ago, I was required to prepare, on very short warning, for the loss of a limb by amputation. A painful disease, which for a time had seemed likely to yield to the remedies employed, suddenly became greatly aggravated, and I was informed by two surgeons of the highest skill, who were consulted on my case, that I must choose between death and the sacrifice of a limb, and that my choice must be promptly made, for my strength was fast sinking under pain, sleeplessness, and exhaustion.

I at once agreed to submit to the operation, but asked a week to prepare for it, not with the slightest expectation that the disease would take a favorable turn in the interval, or that the anticipated horrors of the operation would become less appalling by reflection upon them, but simply because it was so probable that the operation would be followed by a fatal issue, that I wished to prepare for death and what lies beyond it, whilst my faculties were clear and my emotions were comparatively undisturbed, for I knew well that if the operation were speedily followed by death, I should be in a condition, during the interval, in the last degree unfavorable to making preparation for the great change.

The week, so slow, and yet so swift in its passage, at length came to an end, and the morning of the operation arrived, There were no anæsthetics in those days, and I took no preparative stimulant or anodyne of any kind, unless two cups of tea, which with a fragment of toast formed my breakfast, be considered such.

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[1] I have much pleasure in publishing the following interesting and beautiful letter written by an esteemed professorial colleague, who holds a distinguished place in British science and literature, and who, before the days of anæsthetics, was himself the subject of a severe surgical operation.—J. Y. S.

The operation was a more tedious one than some which involve much greater mutilation. It necessitated cruel cutting through inflamed and morbidly sensitive parts, and could not be despatched by a few swift strokes of the knife. I do not suppose that it was more painful than the majority of severe surgical operations are, but I am not, I believe, mistaken in thinking that it was not less painful, and this is all that I wish to contend for.

Of the agony it occasioned, I will say nothing. Suffering as great as I underwent cannot be expressed in words, and thus fortunately cannot be recalled. The particular pangs are now forgotten; but the black whirlwind of emotion, the horror of great darkness, and the sense of desertion by God and man, bordering close upon despair, which swept through my mind and overwhelmed my heart, I can never forget, however gladly I would do so. Only the wish to save others some of my sufferings, makes me deliberately recall and confess the anguish and humiliation of such a personal experience; nor can I find language more sober or familiar than that I have used, to express feelings which, happily for us all, are too rare as matters of general experience to have been shaped into household words.

From all this anguish I should of course have been saved had I been rendered insensible by ether or chloroform, or otherwise, before submitting to the operation. On that point, however, I do not dwell, because it needs no proof, and the testimony of the thousands who have been spared such experiences by the employment of chloroform, is at hand to satisfy all who are *not* determined not to be satisfied.

But there are other modes in which anæsthetics may serve a patient than by rendering him insensible at the period of his undergoing a surgical operation, and it is to these modes of service, which may not strike even the most humane and thoughtful surgeon, and cannot be matters of experience, except to patients who have not taken anæsthetics, that I seek mainly to refer in this letter.

I am not gifted with physical courage. Physical courage I understand to signify that consciousness of a power to endure bodily agony, which accompanies a certain temperament. Its possessors know from the first instinctively, and by and bye learn from experience, that a blow, a cut, a burn, an attack of toothache, or the like infliction of injury, or onset of pain, can be endured by them, though unwelcome, up to an extent of considerable severity, without excessively incommoding them, or exhausting their patience. From severe injuries and dangerous diseases such persons recover, fortified by the assurance that they can bear without flinching what would make others complain loudly, and they are not afraid to anticipate suffering, believing that they will be able to bear it. This estimable virtue is possessed more largely by men than by women, and by savage than by civilized men, and may or may not be accompanied by moral courage.

I belong, on the other hand, to that large class, including most women, to whom cutting, bruising, burning, or any similar physical injury, even to a small extent, is a source of suffering never willingly endured, and always anticipated with more or less of apprehension. Pain in itself has nothing tonic or bracing in its effects upon such. In its relation to the body, it is a sheer and unmitigated evil, and every fresh attack of suffering only furnishes a fresh proof of the sensitiveness possessed to pain, and increases the apprehension with which its attacks are awaited.

When I, accordingly, made up my mind to submit to the operation proposed to me, it was with the fullest conviction that the pain it would occasion would far exceed my power of patient tolerance, and I prepared for it, simply as for a dreadful necessity from which there was no escape. I awoke each morning from troubled sleep to reconsider the whole reasons for and against submitting to the surgeons, and by a painful effort reached again the determination not to draw back from my first resolution. From all this distracting mental struggle, which reacted very injuriously on my bodily constitution, I should have been exempted, had I been able to look forward to the administration of chloroform. A far greater amount of internal composure and serenity would then have been mine, and this mental peacefulness would have been a powerful aid towards sustaining my strength, and fitting me to bear the shock of the operation.

Again, I concealed from the relatives who were about my sick-bed what awaited me, knowing that an announcement of the impending operation would occasion them the greatest grief, and fearing that the expression of that grief would utterly shake my resolution. On the very morning of the operation, I performed my toilet with peculiar pains and care, with a view to disarm their apprehensions, on hearing that the surgeons were to pay me a visit that day; and I had at least the satisfaction of afterwards learning that the *ruse* was successful. But I need scarcely say that the mental tension occasioned by this reserve, and the continued effort to play a part, was a prejudicial exertion, and kept my faculties injuriously on the strain. Could I have told my friends that the operation would be painless, we should have conferred about it, and they and I would have been saved much distress.

Further; during the operation, in spite of the pain it occasioned, my senses were preternaturally acute, as I have been told they generally are in patients in such circumstances. I watched all that the surgeons did with a fascinated intensity. I still recall with unwelcome vividness the spreading out of the instruments; the twisting of the tourniquet; the first incision; the fingering of the sawed bone; the sponge pressed on the flap; the tying of the bloodvessels; the stitching of the skin; and the bloody dismembered limb lying on the floor.

Those are not pleasant remembrances. For a long time they haunted me, and even now they are easily resuscitated; and though they cannot bring back the suffering attending the events which gave them a place in my memory, they can occasion a suffering of their own, and be the cause of a disquiet which favors neither mental nor bodily health. From memories of this kind, those subjects of operations who receive chloroform are of course free; and could I, even now, by some Lethean draught erase the remembrances I speak of, I would drink it, for they are easily brought back, and they are never welcome.

How far my experiences agree with those of others who have undergone similar operations I do not know, but except that I may have a more active and roving fancy or imagination than some of my fellow-sufferers, I cannot doubt that my experiences are not singular.

That the dread of pain keeps many a patient from submitting to operations, which would save life, is notorious; but the dread of a particular mode of inflicting pain is a more dissuasive motive with many than the dread of the pain so inflicted. Hundreds every day endure the great torture of toothache, rather than the small torture of the extraction of the tooth. Women in particular, suffer prolonged agonies for months, rather than submit to a fraction of the same amount of pain at a surgeon's hand, because, as produced by him, it takes the form of an incision with a sharp knife; and a red-hot iron is held in such horror by most persons, that rather than be touched by it, though the pain it occasions is but momentary, they will endure the application of chemical caustics which occasion torture for hours.

Anæsthetics render all such persons as great a service by rendering them insensible to the accompaniments of an operation, as by rendering them insensible to its pain. It is true that if they felt no pain, they might be as calm and even curious spectators of the dismembering of themselves as in dreams all men are, of what in waking life would be the most agonizing realities. But it is not less true, that sufferings equal to those of the severest operations are experienced by patients, in the course of acute or aggravated maladies, without being followed by the crushing effect of the operations which they rival in power to occasion agony; and surely this is not to be wondered at. Before the days of anæsthetics, a patient preparing for an operation, was like a condemned criminal preparing for execution. He counted the days till the appointed day came. He counted the hours of that day till the appointed hour came. He listened for the echo on the street of the surgeon's carriage. He watched for his pull at the door-bell; for his foot on the stair; for his step in the room; for the production of his dreaded instruments; for his few grave words, and his last preparations before beginning. And then he surrendered his liberty, and revolting at the necessity, submitted to be held or bound, and helplessly gave himself up to the cruel knife. The excitement, disquiet, and exhaustion thus occasioned, could not but greatly aggravate the evil effects of the operation, which fell upon a physical frame predisposed to magnify, not to repel, its severity. To make a patient incognizant of the surgeon's proceedings, and unable to recall the details of an operation, is assuredly to save him from much present and much future self-torture, and to give to him thereby a much greater likelihood of recovery.

Further; the horror with which attached relatives regard the prospect of operations on those very dear to them; a horror far surpassing that with which they would, in many cases, hear of such operations awaiting themselves, leads them often to dissuade their friends from submitting to surgical interference. The issue in too many cases is, that the poor patient listens, though but half convinced, to their arguments; tries doctor after doctor, and remedy after remedy, only to be compelled in the end, after weeks or months of prolonged suffering, to submit to the operation. The prospects of recovery, however, in such cases, are too often immensely lessened by the physical exhaustion and enfeebled general health which have resulted from the delay. The knowledge on the other hand that a mother, a sister, a wife, or a child, will be carried unconsciously through a severe operation, cannot but rob it of half its horrors in the eyes of friends, and will make them often the allies rather than the opponents of the surgeon, and keep them from showing the false kindness to their relatives, of dissuading them from submitting to the only treatment which promises a cure.

The sum, you will perceive, of what I have been urging, is, that the *unconsciousness* of the patient secured by anæsthetics is scarcely less important than the *painlessness* with which they permit injuries to be inflicted on him. To steep his senses in forgetfulness, and throw the whole intellectual machine out of action, when if allowed to work, it only moves with a rapidity and irregularity which threaten its integrity, and permanently injure it, is to do him a service, second only to that of saving him from suffering. And to make it impossible for him to recall a scene of horror, and torture himself by going over and over all its incidents again and again, is also to do him a signal service. Nor need more be said concerning the service done to his friends.

I plead, therefore, for the administration of anæsthetics on the grounds enumerated. I fear you may think my confessions exaggerated; but I can most honestly declare that they are not. When I first heard that anæsthetics had been discovered, I could not and would not believe it. I have since thanked God many a time, that He has put it into your heart, and into that of other wise and humane men, to devise so simple and so safe a way of lessening pain.

As for the fear entertained by some, that the moral good which accrues from suffering, and is intended by the Ruler of all to be secured by it, will be lost if agony is evaded by sufferers having recourse to anæsthetics, we may surely leave that to the disposal of Him who does all things well. The best answer to such complaints I have heard, was that given by an excellent old lady to another, who was doubting whether any of the daughters of Eve were at liberty to lessen by anæsthetics the pangs of child-bearing: "You need not be afraid," said the wiser lady, "that there will not be enough of suffering in the world."

I think not; but may you be honored still further to reduce its sum.—Yours most truly.

AN OLD PATIENT.

APPENDIX.

(Note to page 268.)

THE AMERICAN CASE OF ALLEGED DOUBLE HERMAPHRODITISM.

(Subsequently to the appearance of the description quoted in the text, this interesting specimen was examined, carefully and with additional incision, by my friend the late Dr. Burnett of this city, who was inclined to accept the opinion based by Professor Ackley upon his own very imperfect dissection.[1] More lately, however, a further but still incomplete dissection has been permitted the distinguished pathologist, Professor J. B. S. Jackson, whose accuracy and caution are universally acknowledged. Dr. Jackson has kindly communicated to me at some length the result of his investigation, which, however, I prefer to take from the published records of the Boston Society for Medical Improvement.[2]

"The os tincæ is represented in the figure and cast as a prominent object. Dr. J. found no trace of it, but the uterus passed insensibly into the vagina. This last was nowhere of the full size, and towards the opening into the bladder, measured at the smallest part, between four and five lines in circumference upon the inner surface; in the Society's specimen of the cast, something like transverse rugæ are represented, but nothing of the kind was seen. The fimbriated extremities of the Fallopian tubes were distinct enough, though very slightly developed. The very important fact of the existence of ovaries when the testicles were so well developed, should be well attested. Dr. J. found some thickening of the tissues about where the ovaries should be, but it was ill defined and slight; and it would not have been thought of, except in connection with the present question. Upon one side, an incision was made into this questionable part; but nothing like a Graafian vesicle was seen, nothing but a loose cellular or fibro-cellular tissue; a yellow body was exposed, which was thought at first to be a corpus luteum, but it proved to be a lobule of fat. Upon the other side, no incision seems to have been made. One of the testicles had been cut open, and its structure was quite normal; the other had scarcely been cut into. In regard to size, Dr. J. thinks that they were larger than they have been stated to be by Dr. Burnett; some allowance may perhaps be made from the specimen having been in spirit for a number of years. Neither of the organs was dissected so as to show the epididymis, though there could be no doubt of its existence. An incision was made upon a cord that had the feel of a vas deferens, and it was very nearly exposed, midway, to the extent of about two inches; towards the testicle, it appeared to diverge from that organ, so far as could be seen by holding the parts up to the light; but, as no dissection was made, Dr. J. would not say that it was not the vas deferens, and that it did not arise from the epididymis. Towards the neck of the bladder, this cord could not be traced. Upon the other side, something was felt indistinctly that might have been a vas deferens. The vesiculæ seminales were not found. In the situation of the prostate, there was a feeling of resistance as if the gland might exist; but it had not been demonstrated. The penis, scrotum, and other parts appeared to Dr. J. as they have been already described." H. R. S.)

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[1] Records of Boston Society for Medical Improvement, i. p. 356. American Journal of the Medical Sciences, Oct. 1853, p. 367.

[2] Loc. cit. ii. p. 98. American Journal of the Medical Sciences, Oct. 1854, p. 369.

(Note to page 348.)

INTRA-UTERINE ICHTHYOSIS.

(Three additional cases of Congenital Ichthyosis have been lately put upon record by Dr. Gould of Boston.[1] They are more especially remarkable as having all occurred in the same family. Two of the cases were reported to the Boston Society for Medical Improvement in 1853.[2] The parents were cousins. H. R. S.)

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[1] Boston Medical and Surgical Journal, September, 1855, p. 109.

[2] Extracts from Society's Records, vol. ii. p. 44.

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